Université catholique de Louvain
Faculty of Economic, Social and Political Sciences and
Communication
Economics School of Louvain
Louvain Institute of Data Analysis and Modeling in economics
and statistics
Institute of Economic and Social Research

Katholieke Universiteit Leuven
Faculty of Economics and Business
Department of Economics

Essays on the Chinese Economy

Doctoral Dissertation presented by

KAMPUI TSANG

Thesis submitted in fulfilment of the requirements for the degree of
KU Leuven: Doctor in Economics
UC Louvain: Doctor in Economics and Management.

Thesis committee:

Chair: Prof. Dr. Paul Schaudt (KU Leuven)
Supervisor : Prof. Dr. Jean-François Maystadt (UCLouvain)
Supervisor : Prof. Dr. Johannes Van Biesebroeck (KU Leuven)
Jury Member : Prof. Dr. Luke Heath Milsom (KU Leuven)
Jury Member : Prof. Dr. Gonzague Vannoorenberghe (UCLouvain)
Jury Member : Dr. Nele Warrinnier (World Bank)

JULY, 2026

Acknowledgements

Pursuing the PhD study has been a long journey, it would not have been possible without the guidance, support, and encouragement of many people. I am deeply grateful to everyone who has accompanied me during four years of doctoral study.

First and foremost, I would like to express my deepest gratitude to my supervisors, Johannes Van Biesebroeck and Jean-François Maystadt. Jo has been an exceptional supervisor throughout my PhD. His deep economic reasoning and sharp insights have shaped the way I understand economic questions and approach research topics. I have also benefited enormously from his careful reading and attention to detail, which have taught me to make my arguments clearer, more precise, and more convincing. Jean-François has also been an invaluable mentor. His enthusiasm and optimism have helped me through difficult moments and encouraged me to develop my research agenda with greater confidence. His versatility, together with his methodological expertise, has often inspired me to try new approaches and explore areas that were new to me. Although Jo and Jean-François are, in many ways, very different scholars, I have learned immensely from both of them. Their patience, generosity, and continuous support, both in research and in professional development, have helped me grow as an economist and develop my own path as a researcher. I feel very fortunate to have had the opportunity to learn from both of them.

This dissertation also benefited greatly from collaboration with my coauthors. I thank Jean-François Maystadt, Johannes Van Biesebroeck, and Nele Warrinnier for working with me on the chapters on economic sanctions. Working closely with them has been an important part of my PhD training. I learned a great deal from how they develop and frame research ideas, bring economic reasoning into empirical work, and connect academic findings to broader policy debates. Their insights and suggestions have greatly improved these papers. I am also grateful to Yuk-shing

Cheng and Chi Keung Woo for co-authoring with me. Their efforts throughout the publication process were invaluable, and the paper would not have been published as smoothly without them.

I am grateful to the faculty members, colleagues, and administrative staff at KU Leuven and UCLouvain who have supported me throughout my doctoral studies. The research environment at both institutions has provided an excellent place to grow as an economist. I have benefited from seminars, workshops, informal discussions, and many conversations that helped me improve my work. I would also like to thank the administrative staff for their help with many practical matters, especially during the final stages of the PhD, conference travel, reimbursement procedures, and the organization of the defense. Their support made the process much smoother. I am also grateful to Alexey Makarin for hosting my research visit at MIT. I benefited greatly from our discussions and learned a lot from his thoughtful feedback and approach to research. The trip in Boston is memorable from the seminars I attended to the burger I ate.

I am also thankful to the many colleagues and friends I met during my PhD. They made these years intellectually stimulating and personally meaningful. I am grateful for the research discussions, the shared experiences of writing and presenting, and the many moments of support in day-to-day life. Many thanks to Adriaan, Alberto, Alberto, Angel, Bas, Cansu, Caroline, Charles, Charlotte, Chong, Chun, Diego, England, Filippo, Filippo, Florian, Gianmarco, Guanyi, Hélène, Hanxiao, Haoyuan, Honghao, Julian, Justin, Juuso, Karine, Laura, Margaux, Mariela, Marjolein, Nadine, Pieter, Roberta, Robin, Rosa, Rossana, Sarah, Sébastien, Shiyuan, Silvia, Waleed, Zanardello, Zhaniya, and Zhijie. I would like to give special thanks to Sean Kuo from ECOOM, my "big brother" during these years. He has been someone I could always turn to for both academic advice and emotional support, and his encouragement has meant a great deal to me. I have learned a lot from his experiences, stories, and sense of humor, which often helped me face difficult moments with more lightness. Without his support and companionship, completing my PhD would have been much more difficult.

I would also like to thank the participants at the seminars, conferences, and workshops where I presented different parts of this dissertation. Their comments and questions helped me clarify the contributions of each chapter and improve the empirical analyses. I am particularly grateful for the feedback I received from the jury members. Their suggestions were constructive and thoughtful, and helped me improve all chapters of the dissertation in a comprehensive way.

Beyond academic life, I am deeply thankful to my friends for their companionship and support. They gave me encouragement when my PhD journey did not go smoothly and and reminded me to keep the faith and stay true to myself.

Finally, I owe my deepest gratitude to my family. Their unconditional love and support have been the foundation of everything I have done. Even though they may not have followed every detail of my research, they have always believed in me and supported my decisions. Pursuing a PhD far from home required patience, understanding, and sacrifice from them. I am deeply grateful for their trust and encouragement, especially during difficult moments. This dissertation is dedicated to them, with love and gratitude.

Contents

Examination Committee **i**

Acknowledgements **iii**

List of Figures **ix**

List of Tables **xii**

1 General Introduction **1**

2 Sanctions and Shields: The Impact of US Sanctions on Chinese Firms **5**

2.1 Introduction 6
2.2 US Sanctions against China 9
2.2.1 US sanctions toolkit 10
2.2.2 US sanctions policy 11
2.3 Data 15
2.3.1 Firm-level data: Daily stock returns and quarterly accounts 15
2.3.2 Sanctions and ownership data 16
2.3.3 Strategic industries and political connections 19
2.3.4 Descriptive statistics 20
2.4 Stock Market Performance 22
2.4.1 Empirical specification 22
2.4.2 Baseline results 24
2.4.3 Robustness checks 27
2.4.4 Validity check: parallel trends assumption 29
2.5 Heterogeneous effects of US sanctions 32
2.5.1 Impact of different sanction tools 32

2.5.2 Different dimensions of financial performance 34
2.5.3 Government shielding 37
2.6 Conclusion 41
Appendix 2.A Background Information on Targeted Entities 49
2.A.1 Chinese Military Companies 49
2.A.2 Xinjiang Production and Construction Corps (XPCC) 49
Appendix 2.B Industry Composition of Firms 51
Appendix 2.C Variable definitions 52
Appendix 2.D Cumulative abnormal returns model 54
Appendix 2.E Additional robustness checks 55

3 Tracing Sanctions through the Value Chain: Evidence from the US Cotton Ban 69
3.1 Introduction 70
3.2 Background 74
3.2.1 Cotton production in Xinjiang 74
3.2.2 Forced labor in Xinjiang 75
3.2.3 The US Cotton Ban 76
3.3 Data 79
3.3.1 National trade data from UN Comtrade 79
3.3.2 Provincial trade data from China Customs 81
3.3.3 Other datasets 81
3.4 Direct effects on US imports 82
3.4.1 Baseline specification 82
3.4.2 Baseline results 84
3.4.3 Enforcement of the US Cotton Ban through the value chain 87
3.5 Indirect effects on third country imports 91
3.5.1 Specification and results 91
3.5.2 Potential explanations 96
3.6 Conclusion 100
Appendix 3.A Sample of commodities 106
Appendix 3.B Province ranking based on input sourcing 107
Appendix 3.C Attention to human rights crises 109
Appendix 3.D Country rankings using trade data 112
Appendix 3.E Additional results 116

4 Mining the Energy Transition: Chinese Critical Mineral Investment and Protest in sub-Saharan Africa 121

4.1 Introduction . . . 122
4.2 Background . . . 126
4.2.1 Chinese Investment in Transition Minerals in sub-Saharan Africa . . . 126
4.2.2 Chinese Operation and Protest in sub-Saharan Africa . . . 128
4.3 Data . . . 130
4.3.1 Conflict Data . . . 131
4.3.2 Chinese Outward Investment Data . . . 131
4.3.3 Other Mining Operation Data . . . 133
4.3.4 Other Data . . . 134
4.3.5 Summary Statistics . . . 135
4.4 Empirical Strategy . . . 136
4.4.1 Identification assumption . . . 137
4.4.2 Threats to the identification . . . 139
4.5 Main Result . . . 141
4.5.1 Do Chinese critical mineral mines cause more protest? 141
4.5.2 Robustness tests . . . 142
4.5.3 Additional analyses . . . 148
4.5.4 Heterogeneous Effects . . . 150
4.6 Mechanisms . . . 153
4.6.1 Environmental Degradation and Local Grievances . . . 153
4.6.2 Other channels . . . 157
4.7 Conclusion . . . 160
Appendix 4.A Dataset of Chinese critical mineral investment . . . 171
Appendix 4.B Mining Operations in analyses . . . 177
Appendix 4.C Afrobarometer Data and DHS Data . . . 178
Appendix 4.D Definition of Variables . . . 179
Appendix 4.E Summary Statistics . . . 181
Appendix 4.F Additional Figures . . . 185
Appendix 4.G Additional Tables . . . 191

List of Figures

2.1	Cumulative number of Chinese persons and entities targeted by US sanctions	10
2.2	Placebo tests	27
2.3	Event study: Effects of US sanctions on stock market valuations	30
2.4	HonestDID confidence bounds allowing variation in the trend	31
2.E.1	Event study based on alternative estimators: Stock market effects	58
2.E.2	Placebo tests: Financial and investment sanctions	59
2.E.3	Event study: Inventory and Capital Expenses, Financial sanctions against XPCC	64
2.E.4	Event study: Shielding, Financial sanctions against XPCC	65
3.1	China's share in world cotton production	74
3.2	Timeline for the imposition of the US Cotton Ban	78
3.3	Event study of the direct impact on US imports	86
3.4	Effect of the US Cotton Ban on third country imports of Chinese cotton products	94
3.5	Event study of the indirect impact on EU imports	96
3.6	Heterogeneous impact of the Cotton Ban by product category	101
3.B.1	Chinese provinces ranked by cotton-related input sourcing from Xinjiang	108
3.C.1	European countries ranked by relative online search intensity for human rights crises	111
3.D.1	Country rankings by cotton input sourcing from China	113
3.D.2	EU countries ranked by cotton exports to the United States	115

4.1	Chinese Import of Critical Minerals	127
4.2	Chinese Outward FDI Stocks in Mining Sector	128
4.3	Evolution of Chinese-owned Critical Mineral Mining Operations	129
4.4	Protest Events and Critical Mineral Mines	132
4.5	Number of Chinese Operating Critical Mineral Mines, by sub-Saharan African Country	134
4.6	No Differential Pre-trend: Event Study	140
4.7	Child Health Outcomes, DHS Data	156
4.A.1	Example I: Operations of Zijin Mining Group	173
4.A.2	Example II: Operations of MMG Limited	174
4.A.3	Map: Chinese-owned Critical Mineral Mining Operations, by 2023.	175
4.A.4	Governance of Country as Determinants	176
4.F.1	Event Study: Two double-differences, by Ownership	185
4.F.2	No Reverse Causality: Chinese Ownership as Dependent Variable	186
4.F.3	Sensitivity Check: Leave-One-Out Checks by Country, Commodity, and Year	187
4.F.4	Permutation Tests	188
4.F.5	Type of Conflict	189
4.F.6	Mechanism I: Environmental Degradation, Event Study	190

List of Tables

2.1	Economic sanctions imposed by the United States	17
2.2	Stock market performance: Summary statistics	21
2.3	Financial performance: Summary statistics	21
2.4	Stock market performance: The effect of US sanctions against Chinese companies	25
2.5	Stock market performance: Heterogeneous effects	33
2.6	Financial performance: The effect of US sanctions on Chinese companies	36
2.7	Shielding: Effects of all US sanctions	38
2.8	Shielding: Effects of US sanctions on strategic industries, military-related firms and politically-connected firms	42
2.B.1	Number of firms in targeted and control portfolios by industry	51
2.C.1	Stock market performance: Definition of variables	52
2.C.2	Financial performance: Definition of variables	53
2.E.1	Stock market performance: Robustness checks	55
2.E.2	Stock market performance: Quarterly returns	56
2.E.3	Stock market performance: CAR model	57
2.E.4	Financial performance: Effect of US trade sanctions by ownership type	60
2.E.5	Financial performance: Effect of US investment sanctions by ownership type	61
2.E.6	Financial performance: Effect of US financial (GloMag) sanctions on XPCC-subsidiaries	62
2.E.7	Financial performance: Effects on XPCC-subsidiaries, alternative specifications	63
2.E.8	Financial performance: Shielding on XPCC-subsidiaries, Covid pandemic	66

2.E.9	Shielding: Effects of US sanctions on politically connected firms, alternative specifications	67
3.1	Summary statistics on Chinese exports of cotton and control products (monthly, 2019)	80
3.2	Summary statistics on Chinese provincial exports of cotton and control products (monthly, 2019)	82
3.3	Direct impact of the Cotton Ban	85
3.4	Direct impact of the Cotton Ban on provincial exports	90
3.5	Direct impact of the Cotton Ban according to countries' input sourcing from China	92
3.6	Indirect impact of the Cotton Ban: Chinese exports to the EU	95
3.7	Indirect impact of the Cotton Ban: Price versus quantity	97
3.8	Indirect impact of the Cotton Ban: Re-export versus reputation considerations	100
3.A.1	Number of products in the sample (treated products in parenthesis)	106
3.B.1	Destination of the output of cotton-related sectors in Xinjiang	107
3.C.1	Keywords for selected human right crises	109
3.D.1	Rankings of origin countries	112
3.E.1	Direct impact of the Cotton Ban: PPML estimator	116
3.E.2	Direct impact of the Cotton Ban: Replication with China's Customs data	117
3.E.3	Direct impact of the Cotton Ban according to countries' input sourcing from China: Robustness check using 27 origin countries	118
3.E.4	Direct impact of the Cotton Ban according to countries' input sourcing from China: Triple-difference analysis using only US imports	119
4.1	Main result: Comparison to Other Mines without Chinese Ownership	142
4.2	Sensitivity Check I: Alternative Estimator and Specification	144
4.3	Sensitivity Check II: Alternative Sample	147

4.4	Heterogeneous Effects: Institutional Quality	151
4.5	Mechanism I: Environmental Degradation and Chinese Investment	155
4.6	Mechanism I: Environment-related Protest and Chinese Investment	157
4.7	Mechanism II: Economic Well-being	158
4.8	Mechanism III: Corruption and Trust, Afrobarometer	160
4.A.1	List of Critical Minerals Included	173
4.B.1	Countries Hosting Chinese Critical Mineral Mines	177
4.B.2	Cells with Mining Activities, Type of Critical Minerals	177
4.C.1	Waves of Afrobarometer	178
4.C.2	Waves of DHS	178
4.D.1	List of Variables	179
4.D.2	List of Variables, Continued	180
4.E.1	Distribution of Protest Outcomes: Types of Cells	181
4.E.2	Summary Statistics: Balanced Table, Pre-treatment Characteristics	182
4.E.3	Summary Statistics for Variables in Section 4.5	183
4.E.4	Summary Statistics for Variables in Section 4.6	184
4.G.1	Sensitivity Test: Clustered SE and Spatial Correction SE	191
4.G.2	Sensitivity Test: Alternative Functional Form	192
4.G.3	Chinese Investment and Protest: Number of Fatalities	193
4.G.4	Chinese Investment and Protest: World Mineral Price	194
4.G.5	Chinese Investment and Protest: Chinese-owned and Foreigner-owned Mines	195
4.G.6	Chinese Investment and Protest: Other Additional Analyses	196
4.G.7	Heterogeneous Effects: Additional Institutional Quality Measures	197
4.G.8	Heterogeneous Effects: Alignment with China, Ethnic Diversity, and Firm's Characteristics	198
4.G.9	Mechanism I: Environmental Degradation, Robustness	199
4.G.10	Mechanism I: Environmental Degradation, Institutional Quality	200
4.G.11	Mechanism I: Environmental Degradation, Mediation Analysis	201

Chapter 1

General Introduction

China's rise has profoundly reshaped the global economy and international politics. Since the beginning of its reform and opening-up period, China has transformed into one of the world's largest manufacturing hubs, a central node in global supply chains, and a major investor in infrastructure, mining, and other strategic sectors. Its growing economic influence has also translated into greater political influence on the international stage. Inevitably, this expanding role has intensified geopolitical competition with established powers, particularly the United States and other advanced economies. Against this background, trade in strategic commodities, investment in strategic sectors, and the use of geoeconomic policy have become increasingly important instruments through which states pursue geopolitical objectives and seek to strengthen their position in global competition.

This dissertation studies the political economy of geoeconomics, focusing on how these economic instruments affect firm performance, bilateral trade relations, and local political dynamics. The three chapters examine China from two complementary perspectives. In the first two chapters, China is the target of economic sanctions imposed by the United States. These chapters study two forms of targeted sanctions: sanctions against Chinese firms and an import ban on Chinese cotton products linked to Xinjiang. They provide two specific contexts in which to quantify the economic and trade effects of sanctions, an area where empirical evidence remains limited. The third chapter shifts the perspective and studies China as a major global investor. It examines whether Chinese investment in critical minerals, motivated by the need to secure access to essential inputs for green technologies, affects local protest in sub-Saharan Africa. Altogether, this thesis provides new empirical evidence on the consequences of economic sanctions and strategic invest-

ment as two increasingly important forms of economic statecraft, with China at the center of both.

This dissertation makes two main contributions at the intersection of geoeconomics and the Chinese economy. First, by combining credible identification strategies with granular firm-level, product-level, and geocoded data, it provides new evidence on the economic and political effects of geoeconomic policies, as well as the channels through which these effects arise across firms, trade flows, and local communities. As the empirical evidence on their potential effects remains limited, this dissertation fills the gap in the geoeconomics literature

Second, the dissertation contributes to the political economy literature on China's changing role in the global economy by examining China on both sides of economic statecraft. The first two chapters study China as a target of US sanctions and trade restrictions, whereas the third chapter turns to China's role as an investor in strategically important sectors abroad. This perspective highlights that China is not only exposed to geoeconomic pressure from other major powers, but is also an actor shaping economic and political outcomes through its outward investment and position in strategic supply chains. The findings therefore speak to broader debates on the economic and political implications of China's rise.

The ***first*** chapter, "Sanctions and Shields: The Impact of US Sanctions on Chinese Firms", coauthored with Jean-François Maystadt, Johannes Van Biesebroeck, and Nele Warrinnier, examines the effects of the US economic sanctions on Chinese publicly listed companies. These sanctions are considered "smart" sanctions, as they target specific entities in order to minimize negative spillovers on other firms. In the first part of the study, using a difference-in-differences framework, we find that the announcement of the US sanctions reduces the market value of targeted firms. However, in the second part of the analysis, we find no significant effect of sanctions on firms' operating performance. Further analysis suggests that this discrepancy can be explained by shielding measures implemented by the Chinese government to mitigate the impact of sanctions. Our findings provide new evidence on the effects of economic sanctions on firm performance, contributing to an emerging but still understudied area of research.

In the ***second*** chapter, "Tracing Sanctions through the Value Chain: Evidence from the US Cotton Ban", coauthored with Jean-François Maystadt and Johannes Van Biesebroeck, we evaluate the direct and spillover effects of trade sanctions. Specifically, we examine the regional import ban on Xinjiang cotton implemented by the United States. Using a triple-differences strategy, we find that the US trade

sanction significantly reduces Chinese cotton exports to the United States. To further explore the mechanisms, we show that the decline in exports is primarily driven by the negative sanction effect on cotton products produced in Xinjiang as well as on products from Chinese provinces that heavily source cotton from Xinjiang. This finding suggests that the "cotton ban" effectively targets the commodity it intends to restrict. Moreover, we find evidence of negative spillover effects on Chinese cotton exports to the European Union, which we attribute to reputational concerns among firms operating in EU markets. Overall, our findings demonstrate that "smart" trade sanctions can generate substantial negative effects on the targeted commodities and that these effects can extend beyond sanctioning countries through the political consumerism.

The ***third*** chapter, "Mining the Energy Transition: Chinese Critical Mineral Investment and Protest in sub-Saharan Africa", I examine the impact of emerging Chinese investment in critical mineral on the political movement in the hosting country. Over the past two decades, China has rapidly increased its footprint in the global critical mineral extraction sector, with a significant portion of its investment concentrated in sub-Saharan Africa. This chapter studies protest dynamics in sub-Saharan Africa by comparing the protest-intensifying effects of Chinese-owned mines with those of other similar mining operations. Using a novel panel dataset that combines geocoded protest events with data on Chinese investments and other mining operations, I isolate the additional effect of Chinese mining operations through a triple-differences strategy. The result shows that regions hosting operating Chinese mines significantly higher levels of protests compared to the regions without such operations. I explore potential mechanisms and find suggestive evidence linking Chinese mining activities to more severe environmental degradation and adverse health impacts on local communities. These findings enhance our understanding of the local consequences of the expansion of Chinese investment in critical minerals, which has become an important strategy for bolstering supply chain resilience amid escalating geopolitical tensions.

Taken together, the three chapters provide novel empirical evidence on the effects of geoeconomic policies involving China, examining China both as a target of foreign economic statecraft and as an active investor abroad. In the first chapter, I show that US sanctions imposed on Chinese firms lead to a significant and persistent decline in their market value, while their effects on financial performance depend largely on the response of the Chinese government. In the second chapter, I find that the US import ban on cotton products linked to Xinjiang significantly

reduces Chinese cotton exports to the United States. Moreover, the sanction generates spillover effects on Chinese exports to the European Union, suggesting that reputational concerns among importers in third markets can amplify the effects of targeted sanctions. In the third chapter, I show that Chinese investment in the critical mineral sector, while improving access to essential inputs for green technologies, increases local political instability in sub-Saharan Africa through adverse environmental impacts. Overall, the dissertation shows that economic statecraft can generate substantial economic and political consequences for firms and local communities, both directly and indirectly.

Chapter 2

Sanctions and Shields: The Impact of US Sanctions on Chinese Firms[1]

The United States increasingly uses sanctions against China to advance foreign policy goals. We analyze the economic impact on Chinese firms using stock market and accounting data from publicly-listed firms between 2018 and 2022. Upon designation, stock market valuations drop by 2 percentage points, translating to a cumulative market value loss of 65.4 billion RMB. We also find evidence that the Chinese government shields firms through preferential borrowing conditions, lower taxes, and increased subsidies, at a cost of 14 billion RMB. Unlike previous studies on Russia, political connections, rather than economic or strategic considerations primarily influence which firms receive support.

[1]This chapter is co-authored with Jean-François Maystadt, Johannes Van Biesebroeck, and Nele Warrinnier. A previous version of this study circulated under the title "Targeting the Corps: The Incidence of Humanitarian Sanctions on Firm Performance in China" (Maystadt et al., 2024). This chapter is based in part on and extends my master's thesis, "Humanitarian Sanctions in China: Evidence from Stock Market Data." (Tsang, 2023).

2.1 Introduction

Economic sanctions have long been used by sovereign states as a tool of foreign policy, and their importance is growing amid rising geopolitical tensions in recent years. According to the Global Sanctions Data Base (GSDB-R3), the number of international sanctions being announced and enforced has more than tripled since the 1990s (Morgan et al., 2023). By attempting to impose economic damages on other countries, sanctioning states aim to force a change in contested policies without having to resort to military intervention. Recent examples include international sanctions against Iran's nuclear program (see, for instance, Haidar, 2017; Draca et al., 2023) and Russia's invasion of Ukraine (see, for instance, Crozet and Hinz, 2020; Ahn and Ludema, 2020).

Sanction policies come in many forms, from visa and travel bans to asset freezes, financial sanctions and trade embargoes. While economic sanctions have traditionally been directed towards states, over the last three decades policymakers have increasingly used targeted or 'smart' sanctions. Their objective is to cause economic harm to specific individuals, firms, or organizations which are considered most responsible for the contested policies, while minimizing the costs for non-targeted units, e.g., the general population. Given current geopolitical tensions, a better understanding of the impact and effectiveness of these type of sanctions is vital for current and future policy decisions. Recent case studies on Iran and Russia have shown the importance of also investigating governments' reactions to smart sanctions, particularly when the target entities are in economically powerful nations (Crozet et al., 2021; Draca et al., 2023; Nigmatulina, 2022). Surprisingly, no solid empirical evidence has been provided for the effects of sanctions on China, the largest economy under US sanctions.[2]

In this paper, we provide a comprehensive analysis of the effectiveness of US sanctions against China, covering all types of financial, trade and investment sanctions. Broadly speaking, the United States has used sanctions against China to advance two main foreign policy objectives: countering China's military ascendancy and promoting the protection of human rights. Since 2018, it has ratcheted up its use of sanctions by targeting Chinese individuals and entities with restrictions on their financial transactions, trade, and investment. Using lists of targeted firms and detailed data on firm ownership, we first identify all publicly-listed companies in

[2]Recently, Chorzempa et al., 2024 have provided a dataset with a comprehensive overview of US sanctions on China as of May 2024 and Felbermayr et al., 2024 survey facts on the global use of sanctions between 1950 and 2023.

China that are fully or partially owned (even indirectly) by targeted entities between 2018 and 2022. In a staggered event study using high-frequency daily stock price information, we compare their excess returns with otherwise similar, non-targeted firms before and after the sanction designations.

Our first finding is that within two days, daily stock returns of targeted firms decline by 1.96 percentage points relative to the returns of firms in the control portfolio. It indicates that investors expect sanctions to impose economic costs on the sanctioned firms. This decrease amounts to a cumulative loss in market value of 65.4 billion RMB (9.72 billion USD). Interestingly, the magnitude of the expected cost varies little between financial and investment sanctions, while targeted trade sanctions are expected to cause less economic damage.[3] A placebo test illustrates that it is very rare to observe a 2-day abnormal stock market return of this magnitude for the target portfolio. An event study illustrates that the effect does not reverse in the following days. If anything, it grows slightly, showing a 3.34 percentage points decline in market value over the full quarter after sanctions are imposed.

To understand the nature of the costs associated with sanctions, we next exploit quarterly accounting data on firms' financial performance to investigate the impact of sanctions. Across ten performance dimensions that cover profitability, firm size, and liquidity, we do not find any significant effects, neither on private nor on state-owned firms. However, we do find that starting the quarter after the sanction designation, targeted firms borrowed more and obtained more subsidies from the government compared to non-targeted firms. It suggests that the Chinese government has chosen to shield some of the targeted entities and absorb most of the costs associated with the sanctions. The one-off cost of shielding these companies—including additional borrowing and subsidies—is estimated at 14 billion RMB or approximately 2.1 billion USD.

Shielding is particularly apparent for firms affiliated with the Xinjiang Production and Construction Corps, a large conglomerate operating out of Xinjiang province and sanctioned for alleged human rights abuses. We conduct several analyses to establish that these shielding measures are unlikely to be related to government measures in response to the COVID-19 pandemic. To a lesser extent, firms targeted with investment sanctions are shielded as well, but it is not concentrated among state-owned enterprises, strategic sectors, or military-related firms. That

[3]One caveat to this conclusion is that we only observe the impact of sanctions on the subset of publicly-listed firms. If targeted non-listed firms differ greatly in importance across the three types of sanctions, this conclusion will not hold.

contrasts with existing evidence of government shielding of economically strategic sectors in Russia (Ahn and Ludema, 2020; Nigmatulina, 2022). These strategic sectors suffer equal damage in China, but are not singled out for protection. Political considerations seem to matter more since politically-connected targeted firms are more likely to increase borrowing after sanction designation.

We contribute to several strands of literature. There is a substantial body of work that aims to quantify the consequences of international sanctions. Many studies employ a cross-country analysis, often using the dataset of Hufbauer et al. (1990), to assess whether sanctions are effective and under what conditions (see, for instance, Marinov (2005)). Such comparisons between countries have their limitations in terms of credible counterfactual. Gutmann et al. (2023) also rely on cross-country identification, but focus on identifying the most costly sanctions, the economic outcomes that are affected most, and the transmission channels. Other studies have looked at the impact of sanctions on poverty (Neuenkirch and Neumeier, 2016) or income inequality (Afesorgbor and Mahadevan, 2016).

Our approach aligns more closely with recent literature that exploits variation within a sanctioned country (Haidar, 2017; Kelishomi and Nisticò, 2022; Kim et al., 2023) or within a sanctioning country (Besedeš et al., 2017; Besedeš et al., 2021). In particular, our stock market event study follows the methodology of Draca et al. (2023) who evaluate the effectiveness of sanctions imposed on Iran's political elite during the negotiations over its nuclear program. Their findings reveal that market returns for targeted entities on the Tehran Stock Exchange respond positively and significantly to news of negotiation breakthroughs. It suggests that sanctions incentivize elites to negotiate an agreement in exchange for sanctions relief. Our analysis differs in studying a different stage in the sanction cycle, namely the stock market's response to their introduction rather than their relief. It is complementary as it provides a comprehensive analysis of the effectiveness of US sanctions against China, a country largely neglected in the literature on the economics of sanctions.

The second part of our empirical analysis contributes to the literature examining the effects of sanctions on companies' operational performance. A few studies have employed a firm-level accounting data and a difference-in-differences approach to evaluate the sanction effects on Russian firms after the 2014 annexation of Crimea. For example, Ahn and Ludema (2020) shows that the US and EU sanctions adversely impacted the financial performance of targeted Russian firms, but also that strategic companies were less affected. In contrast, Nigmatulina (2022) finds no systematic negative financial effects on targeted companies, but provides evidence of

substantial shielding of state-owned firms by the Russian government. This aggravated the existing misallocation of resources between sanctioned and non-sanctioned firms and worsened allocative efficiency in the Russian economy. In contrast, our results show that in the case of China's shielding, political considerations outweigh economic or strategic ones.

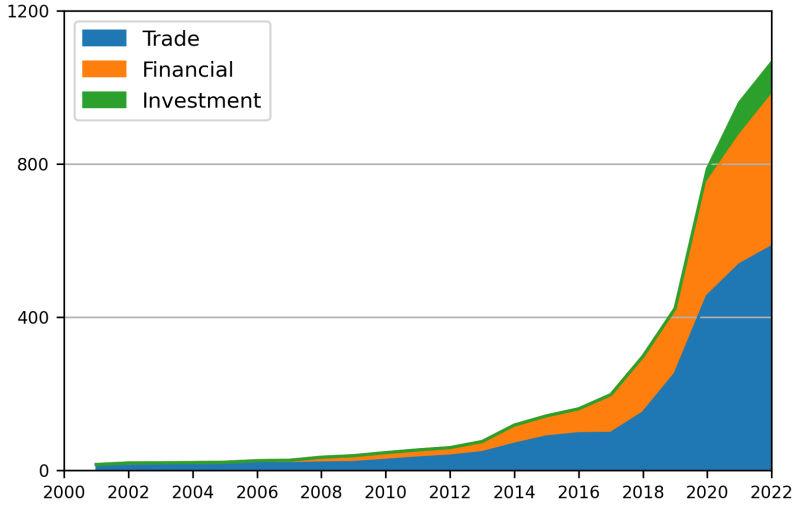
We also add to the political economy literature. One strand of this literature assesses the effect of political events on firms by exploiting stock market reactions to news shocks. A wide range of topics have been covered by these event studies, such as the advantage to firms of having political connections (Fisman, 2001; Acemoglu et al., 2016), fallout from political scandals (Liu et al., 2017), or the implications of domestic conflicts and covert foreign operations (Guidolin and La Ferrara, 2007; Dube et al., 2011). A second strand of studies investigates political economy aspects regarding the financing of Chinese companies, studying topics like the impact of political relations of senior management (Li and Cheng, 2020), legal reforms (Liu et al., 2021), or state control over the banking sector (Yuan et al., 2022).

The remainder of the paper is organized as follows. Section 2.2 provides background information on US sanction policy towards China. Section 2.3 describes the data used in the analysis including a detailed overview of the ownership structures of targeted Chinese firms. Section 2.4 introduces our research design and the empirical findings with robustness checks for the analysis on stock market data. In Section 2.5 we explore the heterogeneity of the harmful effects of sanctions and shielding actions by the Chinese government. This includes the analysis using accounting data on firm performance. Section 2.6 concludes.

2.2 US Sanctions against China

Since 2018, the United States increasingly directs sanctions against Chinese individuals and entities to advance its foreign policy and national security goals. Between 2018 and 2022, approximately 900 individuals and entities in China were subject to economic sanctions by various US government departments. In this section, we provide an overview, first, of the sanction tools used against China and, next, of the individuals and entities that have been targeted.

Figure 2.1: Cumulative number of Chinese persons and entities targeted by US sanctions



Source: Own calculations based on information from the US Office of International Trade Administration's consolidated screening list, the US Department of Homeland Security, Customs and Border Protection, the Office of Foreign Assets Control, and the Bureau of Industry and Security.

2.2.1 US sanctions toolkit

The US sanction policy toward China has mainly relied on three tools: trade, financial, and investment sanctions. As shown in Figure 2.1, trade sanctions are used most frequently, although the use of financial and investment sanctions has increased especially after 2019.

Trade sanctions impose restrictions on the export or import of goods or services from targeted entities. They can be applied to an entire country, to specific industries within a country, or even to specific goods, as is for example the case with an arms embargo. Several US agencies are responsible for the implementation and enforcement of trade sanctions. The US Bureau of Industry and Security (BIS) is responsible for implementing and enforcing export controls of dual-use goods.[4] To this end, the BIS compiles a list of foreign individuals or entities deemed a national security concern, commonly referred to as the Entity List. In addition, the BIS also manages a Military End User (MEU) List, for entities considered to be

[4]Export control regulation of dual-use goods, software, or technology—which can be used for both civilian and military purposes—falls under the Export Administration Regulation (EAR).

military end users in adversarial countries.[5] Listed individuals or entities on these export control lists are subject to export restrictions and licensing requirements for certain technologies and goods.[6] In addition, the US Customs and Border Protection (CBP) implements and enforces import controls under the form of Withhold Release Orders (WROs) to prevent goods manufactured using forced labor from entering the United States. Upon detention of a shipment by the CBP, the US importer is required to offer sufficient proof to demonstrate that the goods were not manufactured with the use of forced labor, otherwise the shipment needs to be re-exported.

Financial sanctions are designed to financially isolate and constrain targeted individuals or entities. In the United States, they are commonly referred to as blocking sanctions and are implemented and enforced by the US Office of Foreign Assets Control (OFAC). Individuals or entities subject to financial sanctions are added to the Specially Designated Nationals and Blocked Persons (SDN) list. All their assets and property under US jurisdictions are frozen and they are prohibited from engaging in any financial transactions with US individuals or entities. Over time, OFAC has created other (non-SDN) sanction lists that prohibit targeted groups of individuals or entities from carrying out specific activities. Typically, the range of the prohibitions applied to non-SDN lists is not as broad as for the SDN list.

Investment sanctions restrict US individuals or entities from investing in targeted companies outside the United States. They are also implemented by OFAC which publishes and regularly updates a comprehensive list of companies subject to investment restrictions. Enforcement falls upon US financial institutions which have the responsibility to block and report any investment transactions in companies under sanctions. A failure to do so may result in fees and penalties.

2.2.2 US sanctions policy

The United States employs sanctions against China to achieve two primary policy goals: countering China's military ascendancy and promoting the protection of human rights.

Military competition. Military-motivated sanctions imposed by the United

[5]This new export control list was launched in 2020 under Section 744.21 of the EAR. Adversarial countries include China, Russia, and Venezuela.

[6]Designated entities are subject to license requirements for all items defined in Supplement No. 2 to Part 744 of the EAR, including, micro-organisms, toxins, computers, telecommunications equipment, and sensors.

States on China date back to the 1940s. Following the establishment of the People's Republic of China in 1949, the United States implemented an arms embargo against China, which was broadened into a complete trade embargo after the onset of the Korean War. Both sets of trade sanctions were lifted in 1971, one year before President Nixon's first visit to China.

In the 1990s, the United States imposed export controls on several Chinese entities believed to be involved in the development of nuclear technology and missile systems, such as the Chinese Academy of Engineering Physics and China Aerodynamics Research and Development Center. In the 2000s and early 2010s, additional export controls were imposed on around 100 Chinese individuals and entities for military reasons, such as transshipping of goods for military end use in China, and the export and re-export of sanctioned goods to countries under US sanctions such as Iran and Russia.

In the second half of the 2010s, Sino-American military competition escalated and the number of individuals and entities subject to export controls increased dramatically. Between 2018 and 2022, more than 400 Chinese individuals and entities were subjected to export controls for their alleged involvement in military activities[7] and around 70 Chinese entities were subjected to export controls after being classified as military end users.

Military-motivated sanctions are not limited to state-owned enterprises, but extend to private companies deemed to acquire US technology and expertise that can be used for military purposes, as part of the Chinese "military-civic fusion" strategy.[8] For instance, in May 2019 the United States imposed export controls on the Chinese multi-technology firm Huawei and its subsidiaries which attracted worldwide media coverage. In August 2020, several Huawei affiliates were added to the BIS Entity List, further restricting their access to US technology. Moreover, between 2018 and 2020, OFAC enforced financial sanctions on 195 Chinese individuals and entities for supporting the governments of Belarus, Iran, North Korea, Russia, Syria and Venezuela.

Besides export controls and financial sanctions, the United States also seeks to halt investment flows to individuals and entities with links to the Chinese military-

[7] Military activities subject to sanctions include: transshipping of goods for military-end use in China, Iran and Russia; participating in the construction and militarization of artificial islands in the South China Sea; and assisting with the development of missile and nuclear weapon systems in China.

[8] This strategy dates back to the late 1990s and aims to harness new technologies from the private sector to speed up the modernization of the Chinese military.

industrial complex. Between 2020 and 2022, it introduced several waves of investment bans on publicly-listed securities of Chinese companies operating in aerospace, aviation, electronics, shipbuilding and telecommunications sectors. OFAC specifically introduced a new list, referred to as the Non-SDN Chinese Military-Industrial Complex Companies List (NSDN-CMIC), to enforce these investment sanctions. By the end of 2022, 68 companies were subject to US investment bans, including major corporations like Aviation Industry Corporation of China, China Mobile, and China General Nuclear Power Corporation.[9]

Human rights accountability. The United States imposed sanctions because of human rights violations for the first time in 1989, under the form of an arms embargo on China following the government crackdown on protesters on Tiananmen Square. It remains in effect even today. A dedicated policy framework to promote accountability for human rights abuses and corruption came into shape in 2017 under the Global Magnitsky Human Rights Accountability Act (GLOMAG).[10] OFAC implements and enforces GLOMAG sanctions by adding sanctioned individuals or entities to its SDN List after which they become subject to financial "blocking sanctions".

Recent US humanitarian sanction policy towards China has focused on alleged human rights abuses of Uyghur and other Muslim ethnic minority groups in the Xinjiang Uyghur Autonomous Region (hereafter Xinjiang). The ethnic policies implemented by the Chinese government in Xinjiang have been criticized by lawmakers and the broader public in many Western countries (see, for example, Lehr, 2020; Bukharin, 2021; Murphy et al., 2022) and have been condemned by the United Nations High Commissioner for Human Rights.[11] Beijing disputes these accusations, asserting that they are fabricated and based on misinformation.[12] In response to

[9]Appendix 2.A provides additional details on targeted Chinese military companies.

[10]In 2012, the United States passed the Sergei Magnistky Rule of Law Accountability Act in honor of Sergei Magnitsky, a Russian whistleblower who exposed fraud by Russian tax officials and later died under suspicious circumstances in prison. The 2017 Act broadened the application of these sanctions globally to any foreign person involved in human rights abuses.

[11]In a report published on August 22, 2022, the UN stated that "Serious human rights violations have been committed in Xinjiang in the context of the Government's application of counter-terrorism and counter-extremist strategies" and that the corresponding policies implemented in the region have resulted in "interlocking patterns of severe and undue restrictions on a wide range of human rights,' with a 'discriminatory component' that has affected 'Uyghur and other predominantly Muslim communities." (OHCHR, 2022, p.43).

[12]For instance, in September 2022, the Chinese government released a report titled "Fight against Terrorism and Extremism in Xinjiang: Truth and Facts" as a response to allegations of human rights violations in Xinjiang. The report is accessible at https://www.ohchr.org/sites/default/files/documents/countries/2022-08-31/ANNEX_A.pdf.

these alleged human rights abuses, US lawmakers introduced the Uyghur Human Rights Policy Act to Congress in 2019. Signed into law by President Trump in June 2020, the legislation mandates that the US government impose sanctions on foreign officials and entities held responsible for human rights violations in Xinjiang and report regularly on human rights conditions in the region.

Between 2019 and 2022, the United States launched multiple rounds of sanctions against the government officials and entities in China allegedly involved in human rights abuses in Xinjiang. A first sanction designation was put in place in September 2019 when the CBP issued a WRO on all garments produced by Hetian Taida Apparel Co. Ltd accused of using forced labor in their manufacturing operations. More trade sanctions followed and by December 2021, the CBP had issued 11 WROs, including a region-wide ban on the import of all cotton and tomato products manufactured in Xinjiang. US Congress took further measures and adopted the Uyghur Forced Labor Prevention Act (UFLPA), signed into law by President Biden in December 2021 and taking full effect in June 2022. The UFLPA overhauled all previous import restrictions and prohibited the importation of all goods produced either wholly or partially in Xinjiang from entering the United States. The United States also imposed export restrictions on 71 Chinese entities by adding them to the Bureau of Industry and Security (BIS) Entity list. The sanctioned entities included 20 regional public security bureaus in China and AI facial recognition companies, such as Hikvision and Dahua.

In addition to several rounds of trade sanctions, the United States also imposed financial sanctions on individuals and entities linked to the alleged human rights abuses in Xinjiang. On July 7, 2020, four Chinese officials and the Xinjiang Public Security Bureau were added to OFAC's SDN list. Three weeks later, on July 31, 2020, the Xinjiang Production and Construction Corps (hereafter XPCC) was added to the SDN list. The XPCC is an important economic conglomerate that controls both publicly-traded and private subsidiaries through a sprawling corporate network, operating across a wide range of industries, such as agriculture, civil engineering, mining and power generation; see Appendix 2.A for additional information. It accounts for 34 percent of cotton production in China and plays an essential role in national and global supply chains (Bukharin, 2021; Murphy et al., 2022). In December 2020, the United States imposed import restrictions on all XPCC-produced cotton, followed by export controls in June 2021. Between 2021 and 2022, another 4 officers linked to the XPCC were added to OFAC's SDN list.

2.3 Data

2.3.1 Firm-level data: Daily stock returns and quarterly accounts

Our goal is to evaluate how US sanction designations impact the valuation and performance of sanctioned firms. To this end, we collect daily stock prices of all firms with A-shares listed on the Shanghai or Shenzhen Stock Exchange from the China Market and Accounting Research Database (hereafter CSMAR).[13]

Both stock exchanges are open for trading during four hours each day (9:15 am to 11:30 am and 1:00 pm to 3:00 pm) and five days a week (Monday through Friday), except for public holidays and a few days that are declared in advance. As of the end of 2017, there were a total of 3,784 companies listed on either exchange with a combined market capitalization of 46 trillion RMB (7.1 trillion USD). We base our analysis on daily stock market returns between 9 May 2018 and 21 December 2021. In line with our empirical design, the starting date corresponds to 60 days prior to the first US sanction that we consider, while the end date marks the second trading date after the last sanction.

We complement stock market valuations with quarterly accounting data, also taken from the CSMAR database. All firms are required to file quarterly financial reports at the stock exchange they are listed at. To investigate the impact of sanctions on firm performance, we retrieve a broad range of financial information, including the firm's operating revenue, net profit, net inventory, total assets, but also information on working capital, debt, tax and subsidy. Our analysis also starts in the third quarter of 2018, but extends to the second quarter of 2022 to capture medium-term consequences on firms' operating performance.[14]

[13] A-shares are publicly-listed ownership shares in China-based companies which are denominated in RMB and traded on the Shanghai or Shenzhen Stock Exchange. Trading of A-shares can only been conducted among Chinese institutional investors and citizens, with the exception of the eligible foreign institutional investors under the Qualified Foreign Institutional Investors (QFII) program.

[14] Our results are robust to limiting the sample period to the fourth quarter of 2021, i.e., the first quarter after the last sanction designation, to match the stock market sample.

2.3.2 Sanctions and ownership data

Target portfolio

Determining which firms are targeted by US sanctions is key to our analysis. Table 2.1 contains details on the various sanctions imposed by the United States on publicly-listed Chinese companies during the sample period. Many of these were discussed in Section 2.2 above. Starting dates are taken from the sanction announcements.[15] In case of multiple sanction designations, we take the date of initial sanction designation as the start of the sanction period.

Next, we identify publicly-listed firms that are owned by targeted firms or entities. The target portfolio counts 134 listed companies that were under at least one US sanction program during the sample period. 88 firms are targeted by investment sanctions, as publicly released by the US Department of State on January 14, 2021. Almost all of them are classified as Non-SDN Communist Chinese Military Companies (NS-CCMC List) and have been targeted first between November 13, 2020 and January 15, 2021. A group of 4 firms are listed by OFAC in its regular Non-SDN Chinese Military-Industrial Complex Companies List (NS-CMIC List). Two of them are excluded since their stock did not trade during the treatment period.

33 targeted firms are under trade sanctions. A challenge here is that the US authorities do not list subsidiaries of sanctioned companies in a comprehensive manner as in the NS-CCMC or NS-CMIC lists. 6 firms are directly mentioned in either of the two lists managed by BIS (BIS Entity List and Military End User List). Similarly as for the investment sanctions, we verify for each of the sanctioned entities whether their name matches with any of the publicly-listed companies in China. We then extend the group of firms under trade sanctions by exploiting ownership data to identify publicly-listed parent companies. Using the Aiqicha and Qichamao databases we verify whether any of the targeted entities are wholly or partly owned by a publicly-listed Chinese parent company. Both databases provide comprehensive information about the registry information and ownership structure of all Chinese companies, listed or private.[16] Publicly-listed companies that own a

[15] Announcements are based on public documents released by respective US departments (OFAC, BIS, and CBP). For the GloMag sanctions against XPCC, we use the announcement made by Mike Pompeo, the US Secretary of State at the time, on Thursday July 30, 2020, at a hearing before the Senate Foreign Relations Committee (Mauldin and McBride, 2020, July 30). In that case, the OFAC public announcement was made the next day.

[16] In principle, every company in China, irrespective of size or ownership, should register with the (regional) State Administration for Market Regulation. Relevant company information is published online via the National Enterprise Credit Information Publicity System from which Aiqicha and Qichamao source business registry data for the universe of firms.

Table 2.1: Economic sanctions imposed by the United States

Start date	Sanction program	Type	No. of sanctioned companies
Aug 2, 2018†	BIS Entity List	Trade	1
Oct 10, 2019	BIS Entity List	Trade	4
June 8, 2020	BIS Entity List	Trade	3
July 21, 2020	BIS Entity List	Trade	4
July 31, 2020	OFAC SDN List (GloMag on XPCC)	Financial	13
Aug 28, 2020	BIS Entity List	Trade	4
Nov 13, 2020	OFAC NS-CCMC List (EO 13959)	Investment	5
Dec 21, 2020	BIS Entity List	Trade	1
Dec 21, 2020	BIS Entity List	Trade	2
Dec 24, 2020	BIS Military End User List	Trade	5
Jan 14, 2021	OFAC NS-CCMC List (EO 13959, amended by EO 13974)	Investment	78*
Jan 15, 2021	OFAC NS-CCMC List (EO 13959, amended by EO 13974)	Investment	1
June 4, 2021	OFAC NS-CMIC List (EO 13959, amended by EO 14032)	Investment	4
June 24, 2021	CBP WRO	Trade	1
June 25, 2021	BIS Entity List	Trade	1
July 13, 2021	BIS Entity List	Trade	1
July 13, 2021	BIS Entity List	Trade	3
Nov 29, 2021	BIS Entity List	Trade	1
Dec 20, 2021	BIS Entity List	Trade	2

Notes: Start date refers to local time in China. Sanction events that do not involve any Chinese publicly-listed companies, e.g. sanctions on natural persons, are omitted. The number of sanctioned companies refers only to publicly listed companies included in our target portfolio for the corresponding sanction episode.
† The quarterly analysis omits this sanction event which occurred during the first quarter of the sample period.
* Companies (excluding their parent companies previously sanctioned) listed on https://2017-2021.state.gov/communist-chinese-military-companies-listed-under-e-o-13959-have-more-than-1100-subsidiaries/.

stake of more than 20 percent in a privately held target company are also included in the target portfolio, which added 27 firms.[17] Finally, we identify one more listed firm under trade sanctions by the CBP and we exclude one firm without any stock trades during the sample period.

We proceed in a similar way, exploiting ownership information, to identify listed firms subject to financial sanctions. The main difference is that we start with only a single, but a very large designated entity as XPCC is the only commercial entity

[17]We omit 9 subsidiaries that might have been influenced by previous sanctions against their parent companies. Our main results are virtually unchanged in a robustness check that uses an expanded target portfolio including these 9 firms.

in mainland China targeted under the GloMag (humanitarian) sanction program during our period of investigation.[18] A challenge is that XPCC possesses holdings in many firms operating across various sectors. It often does not hold its property stakes directly, but indirectly through a sprawling network of affiliated corporations. To identify all XPCC holdings as accurately as possible, we employ a two-step process.

First, we map out XPCC's entire corporate network of privately held firms. Based on the Aiqicha and Qichamao databases, we construct a ten-layer corporate network with 15,473 connected companies which are virtually all unlisted. Second, we identify all listed Chinese companies that are partially owned by XPCC-connected companies and calculate their overall ownership share. To do so, we use historical information on the top 10 shareholders of all companies listed on a Chinese stock exchange from the CSMAR database. For each shareholder of listed firms we look for a match with any of the companies in the XPCC corporate network using name matching on their current and alternative names. This method results in 107 successful, but often duplicative, matches enabling us to identify 25 publicly-listed companies that are owned, either directly or indirectly, by XPCC and its subsidiaries as of the end of 2017Q4. From these 25 listed firms, 13 companies had an XPCC ownership of at least 20 percent and therefore are added to our target portfolio.

Control portfolio

Next, we construct a control portfolio of observationally similar firms that have no connections to the firms in the target portfolio. We start from all 3,453 A-shares trading on the Shanghai or Shenzhen Stock Exchange between the third quarter of 2018 and the second quarter of 2022 with an initial listing before 2018. That way, we cover the full period of sanctions listed in Table 2.1. We then impose two sample restrictions.

First, we exclude firms that do not qualify as targeted firms because the owner-

[18] Three companies from Hong Kong and Macau were sanctioned under the GloMag program on December 9, 2019, and December 9, 2020 for their involvement in corrupt activities in Europe and South East Asia (see https://home.treasury.gov/news/press-releases/sm849 and https://home.treasury.gov/news/press-releases/sm1206 for additional details). As they are not publicly-listed on either Chinese stock exchange and information about their shareholdings in publicly-listed Chinese assets is not available, they are not included in the analysis. The United States also sanctioned 42 Chinese and Hong Kong officials under the Hong Kong Autonomy Act for their alleged engagement in undermining Hong Kong's autonomy. Given that no companies or larger assets were sanctioned in this instance, we also do not include these sanctions.

ship link to sanctioned entities is too weak, as they can still be indirectly affected. This eliminates 95 firms that have ownership shares below 20 percent in any of the sanctioned companies on the BIS's Lists. We also drop 12 companies (and 31 others owned by them) in which XPCC owned a stake of less than 20 percent.

Second, we exclude firms which differ too much from our target portfolio. We limit the control portfolio to firms that operate in any of the 32 (2-digit) sectors covered by the firms in the target portfolio. This eliminates another 763 firms that only operate in other sectors. A group of 17 financial firms are excluded as they face different regulations than firms operating in other sectors. Lastly, we exclude 9 companies that faced US-imposed sanctions that were initiated after our sample period. The baseline sample used in the analysis on stock returns, contains 2,393 firms in the control portfolio.

2.3.3 Strategic industries and political connections

We specifically investigate whether effects are heterogeneous for strategic industries or for firms with political connections. Using the "Guiding Catalogue of Key Products and Services for Strategic Emerging Industries" published by the National Development and Reform Commission of China in 2018, we identify companies involved in strategic activities. We create a list of keywords representing the key activities mentioned as secondary-level strategy industries (40) in the "Catalogue" and verify whether they appear in each firm's primary business scope description. 35 sanctioned companies are thus identified as strategic firms.

In addition, some firms are considered as strategic from a military perspective. First, we categorize eight industries as military related, each of which includes at least one company sanctioned by the US government for military-related reasons. All sanctioned firms in these industries, a total of 45, are considered military-related sanctioned companies. Second, we use the Chinese Listed Firms Personnel Database (CLFPD) (Wang, 2020) which provides biographical information on the board members of each publicly-listed company in China. We only use information dating back to 2012 for two reasons. First, using pre-determined board structures help us identify connected companies without possible self-selection problems that could arise if sanctioned companies changed their board compositions in response to the sanctions. Second, the board composition in 2012 is not influenced by recent regulations, such as the "Rule 18" issued by the Organization Department of the Central Committee of the Communist Party of China in 2013, which limit government officials

from being board members. We define a company as military-related if at least one of its board members currently or previously served in the Chinese military. A drawback of using historic information is that the sample size for our target portfolio is reduced to 51 companies, 11 of which are classified as military-related.

The political connections of a sanctioned company may also influence the extent of government protection it receives. To investigate heterogeneity in this dimension, we again use the CLFPD. We define a company as politically connected if at least one of its board members currently or previously worked in national or local government or in any of China's Communist Party organs. 42 of the 51 companies observed in the 2012 version of the CLFPD were politically connected.

2.3.4 Descriptive statistics

Table 2.B.1 in the Appendix shows the breakdown of the target and control portfolios by industry. The sectoral distribution of targeted firms is rather broad. Targeted firms are relatively more concentrated in some (mainly strategic) industries like e.g. Computer, Communication and Electric Device Manufacturing or the Production and Supply of Electric Power and Thermal Power. We therefore include industry-by-time fixed effects in the analysis to account for the uneven firm composition across industries between the two portfolios.

Table 2.2 shows summary statistics for the variables used in the analysis based on daily stock market returns for firms in the targeted and control portfolios. At the end of 2017, the target portfolio had a slightly higher average daily return and return on equity, but the difference was not statistically significant. Targeted firms did have higher leverage ratio and market capitalization than the non-targeted controls.[19]

Table 2.3 reports the corresponding summary statistics for all dependent variables in the analysis based on quarterly accounting data. On average, targeted firms have higher operating revenue, inventories, total assets, and net fixed assets, but lower total debt and average interest rate than the firms in the control portfolio.

Tables 2.C.1 and 2.C.2 provide a detailed overview of all dependent and control variables used in both analyses. We deflate all financial indicators by a quarterly

[19]The relatively large raw market returns on target portfolio is mainly driven by the 33 firms subject to trade sanctions. These firms are concentrated in the computer electric machinery, and software and IT services sectors. To alleviate concerns over the large difference in the pre-treatment average returns between the targeted and control portfolio, we exclude these 33 sanctioned firms from the sample and the corresponding estimates are similar to the baseline estimates shown in Table 2.4.

Table 2.2: Stock market performance: Summary statistics

	Target			Control			Difference	
	(1)			(2)			(3)	
	No.	Mean	Std dev	No.	Mean	Std dev	(2)−(1)	p-value
Average daily return	134	0.12	0.96	2,386	-0.02	0.75	-0.10	(0.260)
Return on equity	134	2.17	24.35	2,393	-0.06	4.06	-2.23	(0.292)
Leverage ratio	134	0.48	0.20	2,393	0.40	0.22	-0.08***	(0.000)
Market capitalization	134	17.15	1.02	2,389	16.52	0.76	-0.63***	(0.000)

Notes: This sample is used in the analysis based on daily stock market returns. The targeted and control portfolios account, respectively, for 8.4% and 62.5% of the total market capitalization of the Shanghai and Shenzhen Stock Exchanges. The Average daily return and the Market capitalization are the unweighted average calculated in 2017. Total assets, Return on equity, Leverage ratio and Book-to-market ratio are the values at the end of 2017. The Total assets and Market capitalization indicators are inverse hyperbolic sine transformed. Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

Table 2.3: Financial performance: Summary statistics

	Target			Control			Difference	
	(1)			(2)			(3)	
	No.	Mean	Std Dev	No.	Mean	Std Dev	(2)−(1)	p-value
Operating revenue	65	21.53	1.87	1,975	20.67	1.47	-0.86***	(0.000)
Net profit	65	0.83	1.97	1,975	0.98	2.58	0.15	(0.553)
EBIT	63	1.67	1.93	1,945	1.98	3.70	0.31	(0.237)
Inventory	65	16.66	8.81	1,972	14.74	11.62	-1.92*	(0.093)
Total assets	65	23.69	1.58	1,975	22.77	1.21	-0.92***	(0.000)
Net fixed assets	65	21.62	1.67	1,975	20.80	1.62	-0.81***	(0.000)
Cash outflow for acquiring capital	65	1.28	1.13	1,975	1.52	2.09	0.24	(0.108)
Cash and equivalent	65	19.23	13.68	1,975	17.94	15.27	-1.29	(0.458)
Cash from operations	65	1.20	1.30	1,975	1.49	3.64	0.29	(0.114)
Working capital	65	26.23	26.64	1,975	26.00	31.99	-0.24	(0.944)
Total debt	56	17.73	12.95	1,439	21.15	31.31	3.42*	(0.078)
Short-term debt	63	10.30	9.67	1,846	12.22	16.65	1.92	(0.136)
Average interest rate	65	0.39	0.27	1,907	0.47	0.43	0.09**	(0.016)
Subsidy	65	0.23	0.30	1,969	0.20	0.57	-0.02	(0.544)
Average tax rate	62	15.96	8.82	1,915	17.55	8.36	1.59	(0.165)

Notes: This sample is used in the analysis based on quarterly accounting data. Statistics refer to 2018Q3. Operating revenue, Total assets, and Net fixed assets are inverse hyperbolic sine transformed. All other variables, except for Average interest rate and Average tax rate, are normalized by four-quarters lagged values of total assets. Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

price index to control for movements in the general price level. All but five firm indicators are normalized by four-quarters lagged values of total assets. We scale the outcome variables by predetermined level of total assets to mitigate endogeneity concerns (Laeven and Majnoni, 2003) and scaling issues with alternative transformations (Chen and Roth, 2023).

2.4 Stock Market Performance

2.4.1 Empirical specification

We consider sanctions imposed by the US government against Chinese companies from August 2018 onwards as negative information shocks. If investors expect sanctions to impose costs on these firms or hamper their future sales, some might sell their shares, immediately reducing stock market returns.

In the baseline specification (2.1), we employ a standard event study methodology to examine the impact on the valuation of the target portfolio. Identification relies on daily variation of stock returns of targeted shares versus those of control firms and the timing of the announcement of sanctions. As the US government announced and implemented different sanctions against Chinese companies in a staggered fashion, the sample period consists of a pre-event period with at least sixty trading days before and two trading days after each of the included events (Draca et al., 2023). The empirical specification is a difference-in-differences model, given by:

$$Return_{it} \quad = \quad \gamma_i + \gamma_t + \theta \left(\tfrac{1}{2}\, Event_t \times Target_{it}\right) + u_{it}. \tag{2.1}$$

The outcome variable, $Return_{it}$, is the raw daily stock returns for a Chinese company i listed on the Shanghai or Shenzhen Stock Exchange on trading day t. They are expressed as the growth in closing prices compared to the previous day, multiplied by 100. All specifications include firm fixed effects γ_i to give it a difference-in-differences interpretation and (trading) day fixed effects γ_t to absorb common shocks. Standard errors of all regressions are clustered at the firm level.

The explanatory variable of interest is the interaction term between the sanction event day dummy $Event_t$ and the indicator variable $Target_{it}$ for firms classified as sanctioned assets specifically for that event. The coefficient θ measures the percentage effect on the stock market valuation of the targeted firms. A negative coefficient implies that investors expect sanctions to negatively affect future profits, thereby adversely affecting the market value of targeted firms. In principle, this can put pressure on owners to change course on the contested policies or lobby the relevant government to change policies. Trade and financial sanctions immediately take effect when they are announced. Investment sanctions generally take effect 60 days after announcement.[20] Because we expect investors to react to the news and

[20] An exception applies to investment sanctions imposed by EO 13942, where the designated

immediately incorporate the effect of future sanctions on the current valuation of the targeted firms, we use the announcement date as the event date also here. We cannot entirely rule out anticipation effects or the possibility of information leakage, although we do not find anecdotal evidence suggesting that either occurred. As a result, our estimates should be interpreted as a lower bound of the true average effect of sanctions.

The event indicator is defined as a two-day treatment period from the day the sanction designation takes effect. We multiply the interaction term by one half in order to interpret the θ coefficient as the effect on daily returns. We only consider trading days. For example, for the financial sanction designation on XPCC-affiliated assets, the sanctions took effect on Friday, 31 July 2020 and the second event-day is Monday, 3 August 2020. Table 2.1 presents nineteen sanction events included in the analysis as well as the respective start dates. The different lists used to identify the various sanctions allow us to distinguish them across three types: Investment, Trade and Financial.

The validity of our estimates as an average treatment effect hinges on the parallel trends assumption. We test this assumption in the pre-sanction period by running the following regression:

$$Return_{it} = \gamma_i + \gamma_{spt} + (\mathbf{X}_{i,0} \times \gamma_t) + \sum_{\substack{\tau=-20 \\ \tau \neq -1}}^{19} \theta_\tau \left(Day_\tau \times Target_i\right) + u_{it}. \quad (2.2)$$

where Day_τ is a binary indicator for forty individual trading days covering the period from 20 days before to 20 days after each sanction event. We bin the lower endpoint for all trading days at $\tau = -20$ and the coefficient for the base period $\tau = -1$ is omitted. We end the sample period for targeted firms 19 days after the event, compared to 2 days after the event in the baseline specification. Moreover, to account for the dynamics of pre-treatment stock market volatility, we also include sector-by-province-by-day fixed effects γ_{spt} and interaction terms between firm's pre-determined characteristics and day fixed effects $(\mathbf{X}_{i,0} \times \gamma_t)$. If the coefficients on all interaction terms before the announcement of sanctions are statistically insignificant, this provides supportive evidence that targeted and control firms followed similar pre-treatment trends prior to the sanction announcement. We conduct a sensitivity analysis based on the HonestDID approach of Rambachan and Roth (2023).

effective date (November 11, 2021) is ten months after the announcement date (January 13, 2021).

After discussing the results from the baseline regression (2.1) in the next section, we provide a number of robustness checks using additional estimators or alternative samples that we discuss there.

2.4.2 Baseline results

Table 2.4 shows the estimated effects of the US sanctions against firms in the target portfolio. The estimation window ranges from May 9, 2018 to December 21, 2021. For each sanctioned company, the study period includes at least sixty days prior to the relevant sanction event and the treatment period that covers the last two days. The estimated coefficient on the interaction term of interest ($Target \times Event$) is negative and statistically significant. Stock returns of targeted companies fall right after the US sanctions are announced, with a cumulative daily return of 1.96 percentage points below that of non-targeted firms on Chinese stock markets over the two-day event window. This decrease amounts to a loss in market value of 65.4 billion RMB (9.72 billion USD).[21]

In column (2), the two-day interaction term is split into two separate one-day interaction terms. The news had a significant impact on both trading days, with the target portfolio's stock return 1.44 and 0.52 percentage points below that of non-sanctioned companies. The much larger effect on the first day suggests that Chinese markets tend to incorporate the news quickly after the announcement.

Results in columns (3) to (7) contain several robustness checks. The inclusion of additional fixed effects in the regression can reduce the potential risks of omitted variable bias. For a valid identification strategy, the control portfolio must provide a credible counterfactual evolution for the stock prices of targeted firms in the absence of sanctions. One concern is the difference in industrial composition between the target and control portfolios and potentially different stock price evolutions by sector. In column (3), we report results for an augmented specification with (2-digit) industry-trading day interaction fixed effects to capture time-variant industry-specific shocks. It focuses identification on a within-industry comparison of stock price evolutions. The point estimate of -1.68 is slightly lower in abso-

[21] In addition to stock market returns, we examine the effect of US sanctions on the stock return volatility of sanctioned companies. We use two firm-level proxies for stock return volatility: (1) the absolute value of daily returns and (2) the standard deviation of daily returns within a 5-trading-day rolling window. Using the same parsimonious specification as in Column (1) of Table 2.4, we find that the estimated coefficients on the two-day interaction term are positive and statistically significant: 0.68** for absolute returns and 0.83*** for the 5-day rolling standard deviation. This finding suggests that the US sanction increases the stock return volatility of sanctioned company.

Table 2.4: Stock market performance: The effect of US sanctions against Chinese companies

	(1) Baseline	(2) Separate events	(3) Sector-by-day	(4) Full Fixed Effects	(5) PSM	(6) Cengiz *et al.*	(7) Sun and Abraham
$Target \times Event$	-1.961***		-1.684***	-1.135***	-1.185***	-1.957***	-1.976***
	(0.393)		(0.384)	(0.416)	(0.286)	(0.393)	(0.380)
$Target \times Day1$		-1.440***					
		(0.314)					
$Target \times Day2$		-0.521**					
		(0.242)					
Observations	2,161,466	2,161,466	2,161,231	1,983,629	200,052	2,161,466	2,161,466
Firm FE	✓	✓	✓	✓	✓	✓	✓
Day FE	✓	✓			✓	✓	✓
Sector×day FE			✓				
Sector×Province×day FE				✓			
Firm's Controls ×day FE				✓			
Number of firms	2,527	2,527	2,527	2,323	268	2,527	2,527
Number sanctioned	134	134	134	119	134	134	134

Notes: The dependent variable is the daily stock return (in %). The 'Event' is considered to be in effect on the last two trading days and the sample window extends to at least sixty trading days prior. Firm controls include day fixed effects interacted with firm-level characteristics measured in 2017: market capitalization, average daily returns, return on equity, and leverage ratio. Column (5) restricts the control portfolio to nearest neighbors identified using propensity score matching with a logit model. Column (6) utilizes the stacked difference-in-differences estimator proposed by Cengiz et al., 2019. Column (7) employs the estimator introduced by Sun and Abraham, 2021. Standard errors (in parentheses) are clustered at the firm level; Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

lute value but it is not significantly different. In column (4), we add a full set of interaction fixed effects between 2-digit sectors, provinces and days to account for differential stock market performances by sector-province pairs as well as interaction fixed effects between firm-level pre-determined characteristics and days to account for differential stock market performances by firm-level characteristics.[22] With a richer set of fixed effects, the estimate increases to -1.14 but remains statistically significant at the 1 percent level and is not significantly different from the estimate in column (1).

To improve the comparability of the control portfolio, we account explicitly for differences in pre-treatment characteristics between targeted and control firms. We limit the control group to a matched sample of firms to make the benchmark portfolio as similar as possible to the treated one. In column (5), we use propensity score matching (PSM) keeping the nearest neighbors. As it reduces the sample of control firms markedly, it is no surprise that the coefficient is estimated less precisely, but it remains statistically significant at the 1 percent level. Estimated at

[22]Firm-level characteristics include market capitalization, average daily return, return on equity, and leverage ratio. All characteristics are measured using their 2017 values.

−1.19, the point estimate is smaller in absolute values but statistically indifferent from the baseline.

The staggered nature of our treatment runs the risk of considering early treated stocks as part of the control portfolio for later sanctions. Bias may then arise in case of heterogeneous treatment effects (Athey and Imbens, 2022; de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021). Results in columns (6) to (7) assess the importance of that bias and the robustness of our results using two alternative estimators that deal with this issue (Cengiz et al., 2019; Sun and Abraham, 2021).[23] Results for both estimators are remarkably similar to the baseline result. These heterogeneity-robust regressions make sure that the comparison group only consists of companies that have never been treated, but in our setting sanction designations tend to be sufficiently widely spaced to make overlap of staggered events very rare.

Magnitude of the sanctions effect. To have an idea how common it is to observe a difference in stock market return of 1.96 percent, the magnitude of our baseline estimate, we conduct a placebo test. We estimate the difference in average daily returns between the target and control portfolios for all possible one-day or two-day trading periods. In the absence of an event that affects only the valuation of firms in the target portfolio, returns should be approximately equal for these two comparable portfolios.

Specification (2.1) is estimated for all possible event windows between May 9, 2018 and December 21, 2021, the first and last days of the original sample. The first regression designates May 9 as the Event, or May 9-10 in the two-day event specification, with a control period extending 60 days earlier. We estimate all possible average sanction effects by extending the sample period by one day for each regression and each time moving the "placebo" event by one day. In the histograms shown in Figure 2.2, we compare the "placebo" to the "true" sanction effects which are taken from columns (1) and (2) of Table 2.4 and indicated in red.[24]

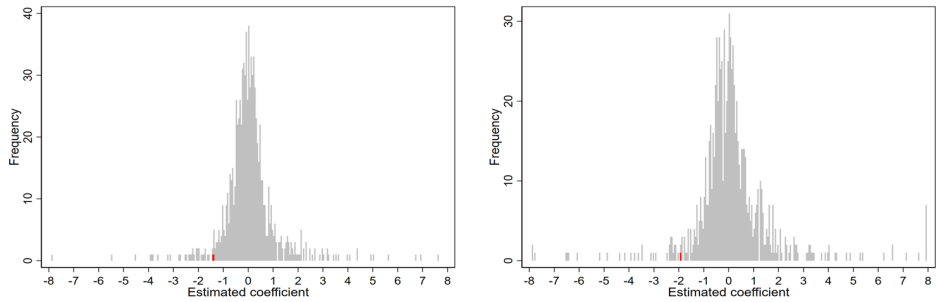
Panels (a) and (b) of Figure 2.2 plot the histograms of 882 and 881 estimated coefficients from the regressions with one-day or two-day interaction terms, respectively. On any given day, the average difference in stock returns between the two portfolios is zero. The "true" sanction effects (in black) are located in the far left tail of the distributions, signifying a significant and atypical difference in the average

[23] We use the Stata packages *'stackedev'* and *'eventstudyinteract'*.

[24] We have also performed this exercise by using the same sample period for each regression, from May 9, 2018 to December 21, 2021, only moving the placebo events by one day. The results are almost identical.

Figure 2.2: Placebo tests

(a) One-day event (b) Two-day event



Notes: All coefficients are estimated using specification (2.1). The coefficients of the "true" sanction effects are reported in red. Panel (a) is the histogram of all point estimates from 882 regressions with a "placebo" single-day interaction term between May 9, 2018 and December 20, 2021. Panel (b) is the histogram of point estimates from 881 regressions with "placebo" two-day interaction terms. In panel (a), the true effect is at the top 5% percentile (the 44th highest value); in panel (b) it is at the top 3% (the 30th highest value).

stock market returns for the target relative to the control portfolio. Specifically, the t-statistics of the highlighted point estimates rank in the top 5% and 3% in panels (a) and (b) (they are the 44th and the 30th highest value out of 882 or 881 estimates). In other words, it is very rare to observe such a large difference in returns between these two portfolios even across a four year period.

2.4.3 Robustness checks

Alternative specifications. Table 2.E.1 in Appendix 2.E reports additional robustness checks that investigate the sensitivity of the baseline result. In column (1), we use as dependent variable an alternative outcome, the daily change ratio, which is reported directly by CSMAR.[25] In column (2), we add a full set of interactions between firm and day-of-the-week fixed effects. This accounts for the possibility that daily stock returns for sanctioned companies behave systematically differently on particular days of the week. In column (3), we add quarter-by-sector-by-day of the week fixed effects to account for the possibility this is a sectoral phenomenon. Heterogeneity in stock market returns can also be related to firms' financial situation. In all these cases, the point estimate on the $Target \times Event$ interaction term barely changes and is never statistically different from the baseline estimate.

[25]Instead of using the raw previous closing price, it is calculated based on the 'ex-right closing price' of the previous day received by the exchange.

In columns (4) and (5), we broaden the estimation sample. First, we extend it by including companies from all 2-digit sectors (still excluding banking and finance) in the control group. Next, we include all observations that were omitted from the control group for reasons discussed in section 2.3. In the latter case, the point estimate on the effect of US sanctions is slightly less negative, but insignificantly different and still statistically different from zero.

In columns (6) and (7), we use the opposite approach. First, we restrict the sample to state-owned enterprises to mitigate concerns that the baseline estimate is driven by differences in firms' political exposure to US sanctions across ownership types. Second, we restrict the control group to a matched sample constructed using coarsened exact matching (CEM). The estimated coefficients are -1.80 and -1.26, respectively, and remain statistically significant at the 1 and 5 percent levels. They are not statistically different from the baseline estimate.

In column (8), we remove outlier observations in the raw daily stock market returns. In column (9), we exclude companies from the control group that have received "Special Treatment" status from the China Securities Regulatory Commission, indicating financial distress. The point estimates barely change and bracket the baseline effect. Finally, in columns (10) and (11), we report standard errors that are clustered at the 2-digit sector level and sanction-episode level. They are almost identical.

From columns (12) to (14), we vary the threshold from 20 percent to 30, 40, and 50 percent. The corresponding estimates are qualitatively similar to the baseline estimate indicating that our results are not sensitive to the choice of threshold.

Analysis at quarterly frequency. We next report results for equation (2.1) estimated at quarterly frequency, similar to the analysis using accounting data that we discuss below. The sample period is now from 2018Q3 to 2022Q2 and the daily stock market returns are aggregated over all trading days in the quarter. We assume that US sanctions are in effect for sanctioned companies starting from the quarter when the initial sanction was imposed on them. The sanction indicator remains switched on afterwards and we indicate this by denoting it by *Post* rather than *Event*. Companies that were sanctioned but had their sanctions lifted before the end of the sample period are omitted from the analysis. As a result, the number of sanctioned companies is reduced to 65.

Results are reported in Table 2.E.2 in the Appendix using the standard FE estimator as well as the estimators accounting for staggered treatment. Quarterly

returns show a one-time decline between 3.27 and 3.41 percent in the post-sanction period. This is slightly higher than the two-day estimates in Table 2.4, suggesting it takes more than two days for the full effect of sanctions to be reflected in stock market valuations. With a smaller target portfolio and many fewer (quarterly) observations, the effects are estimated less precisely, but they are all statistically significant at the five percent level.

Cumulative abnormal returns (CAR) model. The CAR model is an alternative, widely-used methodology to assess relative stock market performance of treated and control companies in an event study. We follow MacKinlay (1997) and employ a market model to construct a two-day cumulative abnormal return after the start of US sanctions for each listed company. We use a pre-event window of 125 trading days that ends 30 trading days before the commencement of the relevant sanction event.[26] This pre-sanctions period is much longer than before because it is used to estimate a specific model to predict stock returns. We then regress our estimates of cumulative abnormal returns on a treatment dummy, firm-level covariates, sector and province fixed effects, an indicator for state-owned enterprises, and sanction episode fixed effects.

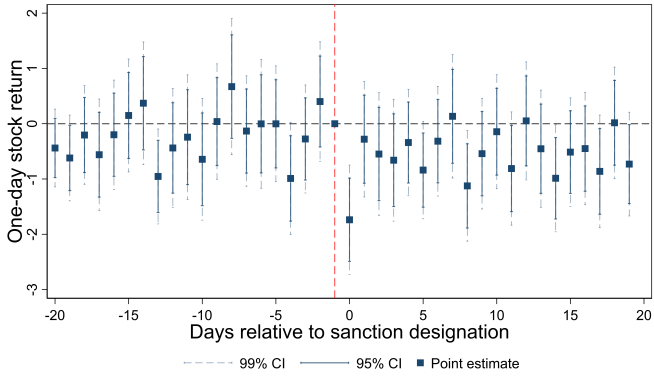
The cross-sectional results for the CAR model, reported in Table 2.E.3 in the Appendix, are in line with the point estimates of the baseline effects. In the three columns we gradually introduce the full set of control fixed effects. The point estimates in columns (1) and (2) range from -0.81 to -0.79, while the effect is estimated much higher, at -1.46, when sanction episode fixed effects are included as well in column (3). These estimates are the cumulative effects over the two-day post-event period and are comparable to the baseline estimate of -1.96 reported in Table 2.4.

2.4.4 Validity check: parallel trends assumption

We next check whether the parallel trends assumption holds which is vital for the validity of the difference-in-differences estimator. We estimate specification (2.2) with two-way fixed effects, excluding the term for the last trading day before the sanction designation ($t = -1$). This is the reference period, indicated by a vertical red line. Because daily stock returns are relatively volatile, we adopt a richer specification to better account for pre-treatment dynamics by including firm fixed effects, firms' pre-determined characteristics interacted with day fixed effects, as well

[26] Appendix 2.D contains the details on the construction and estimation of the CAR model.

Figure 2.3: Event study: Effects of US sanctions on stock market valuations



Notes: Coefficient estimates on the interaction terms between the target portfolio dummy and leading-lagging dummies for 20 days before and 20 days after the sanction designation (the vertical red line is the day before) using the specification in Column (4) of Table 2.4. The vertical segments are the 95 and 99 percent confidence intervals. All point estimates (one-day stock returns) are normalized relative to the omitted period, $t = -1$.

as sector-by-province-by-day fixed effects, as in column (4) of Table 2.4.[27] Figure 2.3 plots the point estimates and the corresponding 95 and 99 percent confidence intervals for the coefficients on the interaction terms between the target portfolio indicator with leading and lagged dummy variables for forty trading days.[28]

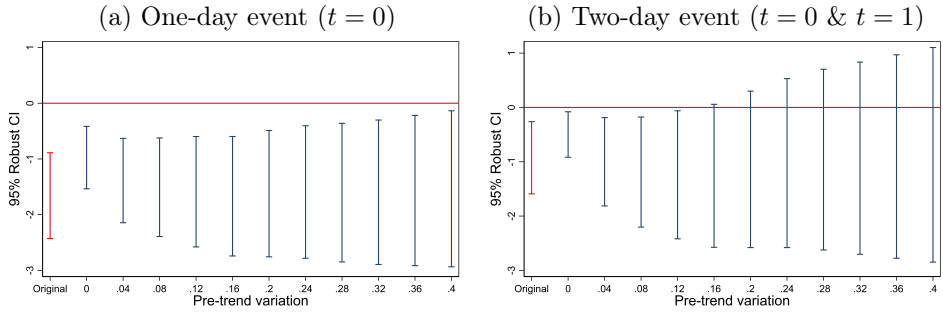
Throughout the pre-treatment period (to the left of the red vertical line), all but three of the coefficients are statistically insignificant at the 5 percent level and they tend to cluster around zero. There is certainly no sign of a pre-treatment trend. Given the staggered treatment design that combines various sanction designations that take place on different calendar dates, it is surprising that even three coefficients are statistically significant. However, the vast majority of targeted companies (78 of 134) are sanctioned on a single day and it is not impossible that some other events on a particular day affected a large fraction of targeted firms.

During the treatment period, the strongest negative effect is found on the first trading day after sanction designation. The market values of the companies being sanctioned are impacted immediately. In the period that follows, there are several days where effects are negative and statistically significant. This is consistent with

[27] At $t = -1$, the average daily return of the target portfolio is 0.36%, compared with 0.03% for the control portfolio. As a result, when using the more parsimonious specification in column (1) of Table 2.4 and normalizing the event-study coefficients to $t = -1$, all estimated coefficients are shifted downward. In this specification, 17 out of 19 pre-sanction coefficients are negative and 9 are statistically significant at the 10 percent level. We therefore use a richer set of fixed effects to better account for volatile pre-sanction dynamics in stock returns.

[28] The first lead-indicator for $t = -20$ combines all previous days in the pre-event window.

Figure 2.4: HonestDID confidence bounds allowing variation in the trend



Notes: Robust confidence intervals estimated following the methodology of Rambachan and Roth (2023). The horizontal axis indicates the fraction of the standard error (which equaled 0.4) applied to let the time trend vary. Values are normalized at $t = -1$.

the negative impact on the stock return in the full quarter after the start of sanctions (shown in Table 2.E.2) being almost twice as large as the initial, two-day impact. While some point estimates in the post-event period are positive, there is clearly no rebound, i.e., the negative effect on the stock market valuation is permanent. The average pre-treatment coefficient is approximately 0.34 units larger than the average post-treatment coefficient, and the difference is statistically significant at the 1 percent level.

In Figure 2.E.1 in the Appendix we show the corresponding graphs for the two heterogeneity-robust regression estimators (Cengiz et al., 2019; Sun and Abraham, 2021), and the (PSM) matched sample. The patterns in all three graphs are similar to the baseline one. Most importantly, by far the most negative point estimate is always at $t = 0$ and coefficients in the pre-sanctions period are closer to zero without showing any trend.

Recently, Rambachan and Roth (2023) proposed a sensitivity analysis for the difference-in-differences (DID) estimator, building on the event study design. It examines the robustness of the DID estimate if the parallel trends assumption may be violated. In particular, it provides a confidence bound for the effect under the assumption that a linear or non-linear pre-treatment trend might be present, but that the counterfactual post-treatment trend does not significantly diverges from it.

We employ the smoothness restrictions method to test for robustness and report the results in Figure 2.4.[29] We let the M factor that measures the variation applied to the slope of the pre-trend ranges from 0 to 0.4. The maximum value equals the

[29] We use the 'HonestDiD' package in Stata to carry out this sensitivity analysis.

standard error of the slope that is estimated in the pre-treatment period. The left graph is for a single-day event, the right graph for a two-day event window. The first confidence interval that is shown, labeled 'Original', assumes no pre-trend and is the one generated by a standard DID. The second confidence interval (for $M = 0$) uses the estimated pre-event trend to provide an extrapolated, counterfactual control in the post-event period. The third confidence interval (for $M = 0.04$) lets the pre-trend vary by one tenth of a standard deviation above or below the estimated value to create a range of control values that the post-event stock returns are compared to. Subsequent values of M on the horizontal axis, refer to successively higher variation applied to the pre-trend slope. The last confidence interval (for $M = 0.4$) applies a variation of a full standard deviation.

We find that the estimated effect of US sanctions on stock market valuations are robust to violations of the parallel trends assumption. As we allow for greater variation in the slope of the pre-trend (higher M), the confidence intervals naturally become wider. However, a large variation is needed for the confidence intervals to include a zero effect. The cutoff values are 0.52 for the first trading day, 130% of the standard error of the slope on the pre-trend, and 0.12 for the first two trading days, 30% of the standard error.

2.5 Heterogeneous effects of US sanctions

2.5.1 Impact of different sanction tools

So far, we have focused on the average effect of all US sanctions, but various sanction tools may differ in their impact on the market value of sanctioned Chinese companies. Table 2.5 provides results that allow for different magnitudes of effects for the three types of sanctions. The baseline estimate is repeated in column (1) for comparison. In column (2), the $Target_{it}$ indicator in equation (2.1) is only switched to one for the thirteen XPCC-affiliated companies under financial sanctions, while the control portfolio is restricted to non-sanctioned firms in the same sectors as the targeted firms. The point estimate rises by one quarter to -2.48 and is still statistically significant at the one percent level. In columns (3) and (4), we follow the same procedure limiting the set of targeted firms to those under trade or investment sanctions. The average effect of the various waves of trade sanctions is negative, but much smaller and not significantly different from zero. The effect of the investment sanctions, which affect the most firms, but were almost all imposed

Table 2.5: Stock market performance: Heterogeneous effects

		Type of sanctions		
	(1) All	(2) Financial (XPCC)	(3) Trade	(4) Investment
Target × *Event*	-1.961***	-2.480***	-0.593	-2.641***
	(0.393)	(0.413)	(0.751)	(0.538)
Observations	2,161,466	34,835	1,383,545	415,827
Firm FE	✓	✓	✓	✓
Day FE	✓	✓	✓	✓
Number of firms	2,527	565	1,603	2,162
Number sanctioned	134	13	33	88

Notes: The dependent variable is the daily stock return (in %). The 'Event' is considered to be in effect on the last two trading days and the sample window extends to at least sixty trading days prior. Column (1) repeats the baseline estimate for comparison. In columns (2)-(4), equation 2.1 is estimated for three subsets of the overall target portfolio, separating targeted firms by the type of sanctions and limiting the control portfolio to firms in the same sectors as the targeted subsets. Standard errors (in parentheses) are clustered at the firm level; Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

on a single day, is the most negative, at −2.64.

Stock market reactions to investment and financial sanctions are relatively similar. Chinese investors seem to believe that financial sanctions, which restrict a wide range of economic activities of XPCC-affiliated companies, and investment sanctions, which banned US investors from trading the public securities of targeted companies, significantly and negatively impact the net present value of these companies. Conversely, the negligible market reaction to trade sanctions suggests that investors believe firms can more easily adjust to them or that those types of sanctions are simply less damaging. For example, the literature investigating the effect of Western sanctions on Iran and Russia has shown that trade sanctions can be evaded (Haidar, 2017; Scheckenhofer et al., 2025). One caveat is that it might be more difficult for investors to identify firms targeted by trade sanctions. An additional limitation is that we only observe publicly-listed Chinese companies. Among the three types, trade sanctions are likely to affect the most companies, including also smaller and privately-held companies. We might be underestimating the damage inflicted by trade sanctions.[30]

Figure 2.E.2 in the Appendix contains similar placebo tests as before focusing on the two largest sanction events in the sample period: (*i*) the financial (GloMag)

[30]To further address the concern that the heterogeneous effects across different types of sanctions may be driven by differences in industrial composition or firm characteristics, rather than by the varying effects of different sanction measures, we conduct two additional checks. First, we include industry fixed effects. Second, we control for firms' pre-sample characteristics. In both cases, the estimated average sanction effects remain qualitatively consistent with those reported in Table 2.5.

sanctions imposed on 13 companies affiliated with the XPCC starting July 31, 2020, and (*ii*) the investment sanctions against 78 military-related companies starting January 14, 2021. In both cases, the estimated coefficients reported in Table 2.5, the "true" effects, are even further in the left tail of the distribution. Such reductions in market value for these target portfolios on any single trading day are very rare, in the case of the investment sanctions a less than once in a year occurrence.[31]

2.5.2 Different dimensions of financial performance

Quarterly specification. The analysis based on stock prices established that the market value of targeted companies declines at the onset of economic sanctions. Next, we use accounting data to investigate which dimensions of companies' financial performance are affected most. We employ a similar difference-in-differences estimation strategy, now at a quarterly frequency:

$$Y_{it} \quad = \quad \gamma_i + \gamma_t + \theta^y \left(Post_t \times Target_{it}\right) + u_{it}. \tag{2.3}$$

One difference with specification (2.1) is that t now denotes quarters. The dependent variable Y_{it} represents various measures of Chinese listed companies' performance and the coefficients θ^y measure the impact of sanctions on each of them. The key explanatory variable is a similar interaction term as before, with one change. The $Post_t$ dummy switches from zero to one the moment a Chinese company is sanctioned, but then remains equal to one for the remainder of the sample period. In an event study using stock market data, the firm's market valuation is expected to incorporate the negative effect of sanctions almost instantaneously, with no further change if no news arrives. However, its financial performance is expected to be negatively affected as long as sanctions are in place.

The target portfolio contains 65 firms. Similarly as for the event study, we exclude 69 firms from the original sample that switch sanction status more than once throughout the sample period. For example, a group of subsidiaries of Chinese military companies were initially targeted by OFAC under the NS-CCMC List in January 2021, but later exempt from sanctions under the new NS-CMIC List since June 2021. Hence, they are excluded from the analysis. As before, the control portfolio is limited to firms from the same (2-digit) sectors as the sanctioned companies.

[31] We conduct a synthetic difference-in-differences analysis (Arkhangelsky et al., 2021) for these two episodes. The estimated coefficients are -2.13 and -2.08, respectively, and both estimates are statistically significant at the conventional levels.

It contains 1,975 firms.[32]

Profitability, size and liquidity. Results in Table 2.6 show the average effect of all sanctions on ten outcome variables that provide a comprehensive picture of the financial performance of Chinese listed companies. For operating revenue, total assets, and net fixed assets, the coefficient of interest measures the difference in growth rate (in percent) for targeted relative to control firms. The other dependent variables are normalized by lagged total assets and the coefficients measure the relative change in that ratio for targeted firms.

The estimated impact on four dimensions of firm profitability—operating revenue, net profits, earnings before interest and taxes (EBIT) or inventories—is reported in panel (a). None of these dependent variables show a remotely significant effect for sanctioned companies. All three dimensions of firm size—total assets, net fixed assets, or capital expenses—are negatively affected by sanctions, but none of the coefficients reported in panel (b) is statistically significant either. The first two point estimates are especially small, which is not surprising given that it would take time for sanctioned companies to downsize. Finally, panel (c) reports the effects on three dimensions of liquidity—cash holdings, operating cash inflows and working capital. All of the three point estimates are again negative, but none is again remotely statistically significant. These last three measures are expected to deteriorate rapidly as the operating environment for targeted firms worsens, but the impact of US sanctions barely registers.[33]

Results in Tables 2.E.4 and 2.E.5 in the Appendix investigate the effects separately for trade and investment sanctions. We again find barely any significant effect on the same dimensions of profitability, firm size and liquidity, not even if we allow for different effects by ownership status (SOE or non-SOE).[34] For eight of

[32]To verify whether the parallel trend assumption holds for the various dimensions of financial performance, we perform separate event studies for each dependent variable Y_{it} at the quarterly level. We report them below, together with the corresponding HonestDID plots, for a few cases where the DID estimates are significantly different from zero. Full results are available upon request.

[33]To check whether the null sanction effects on sanctioned companies' financial indicators reflect limited statistical power, we conduct a minimum detectable effects analysis. We find that, for the financial-ratio outcomes, the estimated MDEs are generally between 0.17 and 0.21 standard deviations. This suggests that the null estimates for net profit, EBIT, inventories, capital expenses, cash holdings, and working capital are unlikely to be driven solely by low statistical power. The results are less informative for the IHS-transformed outcomes, where the estimated MDEs are substantially larger, and for operating cash inflow, where the simulated power does not reach 0.8 within the searched effect-size range.

[34]The only two exceptions are (*i*) operating cash inflow in Table 2.E.4, where the significant difference between SOEs and non-SOEs merely confirms the lack of an effect for SOEs; and (*ii*)

Table 2.6: Financial performance: The effect of US sanctions on Chinese companies

(a) Profitability	(1) Operating revenue	(2) Net profit	(3) EBIT	(4) Inventory
Target × *Post*	0.032	-0.247	-0.029	0.834
	(0.053)	(0.235)	(0.310)	(1.004)
Observations	32,315	32,352	23,979	32,078
Mean of dependent variable	20.71	1.027	2.199	14.85
Firm FE	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓
Number of firms	2,040	2,040	1,983	2,036
Number sanctioned	65	65	59	65
(b) Firm Size	(5) Total assets	(6) Net fixed assets	(7) Capital expenses	
Target × *Post*	-0.013	-0.041	-0.206	
	(0.037)	(0.036)	(0.170)	
Observations	32,400	32,399	31,167	
Mean of dependent variable	22.80	20.83	1.536	
Firm FE	✓	✓	✓	
Quarter FE	✓	✓	✓	
Number of firms	2,040	2,040	2,037	
Number sanctioned	65	65	65	
(c) Liquidity	(8) Cash	(9) Operating cash inflow	(10) Working capital	
Target × *Post*	-1.515	-0.121	-1.623	
	(1.583)	(0.249)	(2.321)	
Observations	32,351	32,352	32,351	
Mean of dependent variable	18.08	1.491	26.44	
Firm FE	✓	✓	✓	
Quarter FE	✓	✓	✓	
Number of firms	2,040	2,040	2,040	
Number sanctioned	65	65	65	

Notes: The sample period covers sixteen quarters between 2018Q3 and 2022Q2. The *Post* indicator switches to one from the moment a targeted company is first sanctioned. The dependent variable of each regression is indicated in the column heading. In columns (1), (5), and (6), the dependent variables are inverse hyperbolic sine transformed. All other outcome variables are scaled by a four-quarter lag of total assets. Standard errors (in parentheses) are clustered at the firm level; Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

the ten variables, the point estimates for the two types of firms have opposite signs which indicates that the average effects across all firms are even smaller.

The only instance of sanctions showing a significant negative effect on financial performance appears for the financial (GloMag) sanctions on XPCC-affiliates, reported in Table 2.E.6.[35] The targeted firms show significantly higher inventories.

capital expenses in Table 2.E.5, where there is a surprising positive, but small, effect for non-SOEs.

[35]There are no separate effects by ownership type in Table 2.E.6 as all targeted firms are state owned.

The absolute magnitudes of the differences are non-negligible at +39%.

2.5.3 Government shielding

The negative effect estimated on stock market data suggests that investors expect a cost of sanctions for targeted firms, albeit a relatively small cost. However, we find only insignificant effects on firms' operating performance, except for one variable in the case of financial sanctions on XPCC-related firms. A potential explanation is that the Chinese government provides support to sanctioned companies to absorb the effects of US sanctions and that this ex-post shielding was not fully anticipated.

Who gets shielded? Using the same estimation strategy as before, we examine the changes for five additional indicators which may reflect government support. Panel (a) of Table 2.7 contains the average effects of all US sanctions. Following their implementation, state-owned sanctioned firms borrow significantly more, especially short-term than control firms. An extensive literature on the Chinese economy documents that the financial sector operates with substantial frictions and that the allocation of bank loans is often influenced by political considerations (Song et al., 2011; Wu, 2018). The same group of firms also records a higher level of direct subsidies, but that point estimate is not statistically significant; neither is the effect on interest rates and tax rates.

The same indicators do not show any significant changes for privately-owned sanctioned companies. This could be due to selective shielding only of state-owned firms, but may also reflect heterogeneity across the different sanction programs. In particular, results in panel (b) show that shielding is especially prevalent for the financial sanctions against the XPCC and its subsidiaries, which are all state owned. In this case, changes for borrowing and subsidy are consistent with strong government shielding.[36] Moreover, the effects are quite large, ranging from 48% to 68% of the mean value of the dependent variable. Firms in the target portfolio receive more loans—the increase in lending averages 1 billion RMB for a total of 13.6 billion RMB. Sanctioned firms receive more subsidies[37]—the 0.09 point estimate implies an average increase of 7 million RMB for a total of 85.9 million RMB.

[36] As always, we limit the control portfolio to firms that operate in the same 2-digit sectors as the relevant sanctioned firms in each analysis.

[37] We use "Other Income" on the income statements as a proxy for government subsidies. The Chinese "Accounting Standard for Business Enterprises No. 16 - Government grants (Revised in 2017)" specifies that this indicator records the income derived from government subsidies associated with a firm's daily activities.

Table 2.7: Shielding: Effects of all US sanctions

(a) All	Total borrowing	Short-term borrowing	Avg Interest Rate	Subsidy	Avg. Tax Rate
	(1)	(2)	(3)	(4)	(5)
$Target \times Post$	0.387	-0.651	0.006	0.007	-2.326
	(2.259)	(1.045)	(0.031)	(0.038)	(1.555)
$SOE \times Target \times Post$	5.231**	3.717**	-0.041	0.039	1.787
	(2.136)	(1.716)	(0.036)	(0.038)	(1.719)
Observations	16,701	26,076	25,430	30,870	21,204
Firm FE	✓	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓	✓
Mean of dependent variable	22.86	13.08	0.481	0.204	17.51
Number of firms	1,436	1,882	1,949	2,032	1,901
Number sanctioned	50	58	64	65	57
(b) Financial Sanction (GloMag on XPCC)	(6)	(7)	(8)	(9)	(10)
$SOE \times Target \times Post$	14.14**	10.37***	-0.052	0.089*	0.399
	(5.836)	(3.744)	(0.046)	(0.050)	(1.922)
Observations	5,173	7,444	7,255	8,493	6,298
Firm FE	✓	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓	✓
Mean of dependent variable	29.34	15.17	0.548	0.137	18.97
Number of firms	431	526	544	566	539
Number sanctioned	10	11	12	13	12
(c) Trade Sanction	(11)	(12)	(13)	(14)	(15)
$Target \times Post$	-0.203	-0.876	0.014	-0.001	-3.354*
	(2.300)	(1.094)	(0.033)	(0.042)	(1.807)
$SOE \times Target \times Post$	3.999**	0.612	-0.060	-0.004	3.347
	(1.805)	(1.400)	(0.043)	(0.037)	(2.332)
Observations	13,817	21,913	21,307	25,983	17,730
Firm FE	✓	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓	✓
Mean of dependent variable	21.32	12.82	0.465	0.215	17.15
Number of firms	1,195	1,589	1,640	1,708	1,597
Number sanctioned	28	31	32	32	25
(d) Investment Sanction	(16)	(17)	(18)	(19)	(20)
$Target \times Post$	7.363*	2.081	-0.058	0.124	3.192**
	(3.996)	(2.722)	(0.062)	(0.085)	(1.354)
$SOE \times Target \times Post$	-3.399	-1.175	0.039	-0.064	-3.730**
	(3.075)	(2.611)	(0.067)	(0.080)	(1.508)
Observations	6,065	9,307	9,040	10,891	7,297
Firm FE	✓	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓	✓
Mean of dependent variable	22.67	13.36	0.440	0.232	17.67
Number of firms	507	668	695	719	668
Number sanctioned	12	16	20	20	20

Notes: The sample period covers sixteen quarters between 2018Q3 and 2022Q2. The *Post* indicator switches to one from the moment a targeted company is first sanctioned. The dependent variable of each regression is indicated in the column heading. The different panels contain results for different subsets of sanction programs: panel (a) includes all sanctions; panel (b) includes only the GloMag sanctions imposed against XPCC-affiliated companies (which are all SOEs); panel (c) includes only trade sanctions (including CBP's WRO, BIS entity list and the BIS's MEU list); and panel (d) includes investment sanctions, i.e., firms included on the NS-CCMC and NS-CMIC lists. Standard errors (in parentheses) are clustered at the firm level; Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

The relatively strong effects on inventories (positive), as well as the systematic evidence of government shielding of XPCC-related firms hit by financial sanctions are robust. In Table 2.E.7 we show sensitivity analyses with various alternative model specifications, e.g., including sector-by-quarter fixed effects, omitting outliers or companies with "special treatment" status, restricting the control group to state-owned firms or enlarging it with firms from all sectors. Moreover, the event study results in Figures 2.E.3 and 2.E.4 provide evidence supportive of the parallel trends assumption. None of the seven quarterly estimates in the pre-sanction period for any of the seven indicators of interest is statistically significant at the 5 percent level. The graphs on the right show the sensitivity analysis proposed by Rambachan and Roth (2023). While the first two confidence bounds do not include a zero effect, i.e., not allowing a time trend or using the estimated pre-event time trend in the counterfactual, that is not the case when we allow for variation in the time trend. That is not surprising, as the trends are estimated on merely seven period and have relatively large standard errors.

In panels (c) and (d), we investigate potential heterogeneous effects across the other two sanction tools. There are only two instance where there is some evidence of shielding in the case of trade sanctions (in panel c). State-owned sanctioned firms borrow more and non-SOEs faces a lower average tax rate. In contrast, in the case of investment sanctions (in panel d), the only significant instance of shielding is for non-SOEs, i.e., for total borrowing.

Is it explained by COVID? A potential concern is that the shielding of XPCC affiliates may reflect the government's policy response to the COVID pandemic. The first COVID outbreak occurred in December 2019, and some of the episodes in China fell within our treatment period. If the Chinese government had implemented fiscal policies to bolster the financial performance of firms in light of pandemic-related disruptions, such measures could confound the shielding effects we attribute to US sanctions. However, this concern is not that compelling. First, although the COVID pandemic originated in Hubei in November 2019 and spread nationwide in the first quarter of 2020, for most of the study period its impact in China was only sporadic until the fourth quarter of 2021. For example, the cumulative number of reported infections in Xinjiang by the end of our sample period (December 2021) was below one thousand, and the region experienced only a sin-

gle large-scale lockdown in Urumqi over the sample period.[38] Second, the Chinese government offered tax and fee reductions as well as interest subsidies to small and medium-sized enterprises and to firms directly involved in epidemic prevention and control, such as manufacturers of medical equipment. There were, however, no central government policies that directly assisted large companies or XPCC affiliates. Similarly, the Xinjiang local government introduced preferential policies to support small and medium-sized enterprises, particularly in sectors heavily affected by the pandemic, such as transportation, catering, accommodation, and tourism. None of these measures extended to the XPCC-affiliated firms that we study, which are all stock-market listed, large firms. Moreover, including sector-by-quarter fixed effects, as in panel (a) of Table 2.E.7, yields estimates similar to the baseline estimates.

Although there is no explicit evidence that the Chinese government provided COVID-related supports to the publicly-listed XPCC-subsidiaries, we consider two additional tests. First, , we exclude the periods of major outbreaks in China from the analysis, specifically the first quarter of 2020 and the first two quarters of 2021. As shown in panel (a) of Table 2.E.8, all estimated coefficients are statistically significant, except for those on average interest rate and average tax rate.

Second, we perform a placebo test by considering the onset of the COVID pandemic, i.e., the first two quarters of 2020, as treatment period and exclude subsequent quarters when sanctions came into effect. This analysis aims to test for significant differences in shielding variables between sanctioned and control firms appearing at the start of the COVID pandemic in China. The estimated coefficients in panel (c) of Table 2.E.8 are all statistically insignificant.

Is shielding strategic? Our findings of negative, but limited, effects of US sanctions on Chinese firms with some evidence of government shielding, are consistent with the literature on sanctions on Russia. Ahn and Ludema (2020) report evidence that the Russian government protected strategic firms from US and EU sanctions. Nigmatulina (2022) finds similar support to state-owned Russian enterprises affected by sanctions. In the case of China, we find suggestive, but inconclusive evidence that SOEs are protected most and that bank loans from the state-controlled financial industry are an important instrument. It might be the case, however, that government protection is better explained by the political sensitivity of the humanitarian

[38] Baidu provides statistics on COVID-19 infections: https://baike.baidu.com/reference/55675012/533aYdO6cr3_z3kATPTezv6jOn6XYoyp6LzWVuFzzqIPmGapB5jqU4s3rtU-9_NyWgjEvddjY8JamO29ShUQq6pPL7RkXet32jahBnafluC_rsY-zJMb_occG_xV3p6x5Sv9. (Accessed on September 26, 2025)

sanctions or by the special status of the XPCC.

The results in Table 2.8 investigate in more detail what types of firms are most likely to be shielded by the Chinese government. In panel (a), we include an interaction term between the sanctions indicator and a 'Strategic' indicator for firms in a sector or producing a product or service that is considered as strategic (defined in Section 2.3.3). It turns out that strategic firms are not really shielded more than others. None of the triple interaction terms is remotely significant. Similarly, as shown in panels (b) and (c), there is also no evidence that military-related companies are shielded to a greater extent.

The shielding actions of the Chinese government, such as those favoring the XPCC-linked firms, seems to be better explained by political considerations. Many other studies have already shown the value of political connections to officials in government or in the Chinese Communist Party to firms operating in the Chinese economy (Chang and Wong, 2004; Chen and Kung, 2019; Li et al., 2008). We define a company as politically connected if at least one of its board members has worked as a national or local party or government official as of 2012. Because of this older information, the sample size decreases to 51 sanctioned companies and 1,350 companies overall. On the positive side, the use of predetermined data on political connections avoids endogeneity problems with US sanctions and biased information due to the more recent depoliticization regulation (details in Section 2.3.3).

Results in panel (d) of Table 2.8 indicate that politically-connected firms receive more financing, as reflected by the additional borrowing after sanctions are imposed. The significant positive effect on borrowing even remains if a triple-interaction terms for SOEs is also included, or if XPCC-affiliated firms are excluded from the target portfolio (Table 2.E.9 in the Appendix).

2.6 Conclusion

We analyze the economic impact of various US sanction programs on Chinese firms using firm-level stock market and accounting data. Since 2018, the US has increasingly used sanctions to advance two main foreign policy goals: countering China's military ascendancy and promoting the protection of human rights.

The imposition of sanctions can be considered as an information shock that might lead investors to reappraise the valuation of the targeted companies. We first examine the impact on the stock market performance of sanctioned Chinese firms using information on daily stock market returns. The baseline estimate indicates

Table 2.8: Shielding: Effects of US sanctions on strategic industries, military-related firms and politically-connected firms

	Total borrowing	Short-term borrowing	Avg Interest Rate	Subsidy	Avg. Tax Rate
(a) Secondary-level Strategic industries	(1)	(2)	(3)	(4)	(5)
Target × Post	2.789	2.522	-0.039	0.018	-0.727
	(2.332)	(2.193)	(0.031)	(0.032)	(0.991)
Strategic × Target × Post	1.713	-1.442	0.036	0.026	-0.710
	(2.392)	(2.197)	(0.036)	(0.035)	(1.419)
Observations	16,701	26,076	25,430	30,870	21,204
Firm FE	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	22.86	13.08	0.481	0.204	17.51
Number of firms	1,436	1,882	1949	2032	1901
Number sanctioned	50	58	64	65	57
(b) 8 Military-related industries	(6)	(7)	(8)	(9)	(10)
Target × Post	6.064**	6.170**	-0.046	0.065	-1.836
	(2.555)	(2.906)	(0.034)	(0.052)	(1.883)
Military × Target × Post	-3.319	-6.228**	0.037	-0.048	1.039
	(2.287)	(2.920)	(0.039)	(0.049)	(1.989)
Observations	16,701	26,076	25,430	30,870	21,204
Firm FE	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	22.86	13.08	0.481	0.204	17.51
Number of firms	1,436	1,882	1,949	2,032	1,901
Number sanctioned	50	58	64	65	57
(c) Board members with military career	(11)	(12)	(13)	(14)	(15)
Target × Post	5.188*	1.731	-0.018	0.051	-0.627
	(2.710)	(1.219)	(0.022)	(0.040)	(0.935)
Military × Target × Post	-6.095**	3.341	-0.002	-0.057	-0.691
	(2.550)	(4.747)	(0.032)	(0.046)	(1.613)
Observations	12,205	17,804	17,290	20,333	13,458
Firm FE	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	22.46	13.03	0.509	0.189	18.65
Number of firms	1,004	1,260	1,297	1,345	1,240
Number sanctioned	38	46	50	51	49
(d) Board members with political connection	(16)	(17)	(18)	(19)	(20)
Target × Post	-1.179	-2.677	-0.019	0.021	-0.468
	(2.857)	(1.900)	(0.059)	(0.048)	(1.616)
Connection × Target × Post	6.123**	5.875***	0.000	0.021	-0.358
	(2.587)	(2.222)	(0.062)	(0.041)	(1.822)
Observations	12,205	17,804	17,290	20,333	13,458
Firm FE	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	22.46	13.03	0.509	0.189	18.65
Number of firms	1,004	1,260	1,297	1,345	1,240
Number sanctioned	38	46	50	51	49

Notes: The sample period covers sixteen quarters between 2018Q3 and 2022Q2. The *Post* indicator switches to one from the moment a targeted company is first sanctioned. The dependent variable of each regression is indicated in the column heading. Each panel contains results for a different triple-interaction term that is based on various ways of defining a firm characteristic that might make it more likely to be shielded from sanctions. In panel (a), strategic industries are based on the secondary-level (40 key products) classified in the "Guiding Catalogue of Key Products and Services for Strategic Emerging Industries" of Chinese NDRC in 2019. Military-related firms are identified based on their industry in panel (b) or based on the number of board members with military experience in panel (c), which is derived from the Chinese Listed Firms Personnel Database(Wang, 2020). In panel (d), political connections are also identified based on the number of board members with party or government experience. Standard errors (in parentheses) are clustered at the firm level; Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

that sanctions lead to a drop in market evaluation of, on average, 2 percentage points, equivalent to a loss in market value of approximately 65.4 billion RMB. The estimated effect is robust, and we find no evidence of mean reversal in subsequent days or weeks, indicating that the stock market devaluation is non-transitory. US sanction tools can be broadly categorized into three categories: financial, investment and trade sanctions. An analysis allowing for a heterogeneous response indicates that investors perceive financial and investment sanctions as more costly to firms compared to trade sanctions.

We next investigate the reasons behind this relatively modest loss in market value in firms' quarterly accounting data. While we barely find any impact on dimensions of profitability, firm size, or liquidity, we do find evidence of a reaction by the Chinese government. In particular, state-owned companies subject to sanctions receive larger loans from the state-controlled banking system. However, we do not observe significant impacts on the protection provided to non-state-owned counterparts. Furthermore, we find that firms linked to the XPCC—a large conglomerate operating from Xinjiang and accused of human rights violations—received substantial government support in the form of higher subsidies. We estimate the total cost of shielding XPCC companies, in terms of additional loans and subsidies, at 14 billion RMB (2.1 billion USD). We do not observe similar shielding actions by the Chinese government to other types of US sanctions. The protection does not apply uniformly, and we provide suggestive evidence that this discrepancy may be attributed to political motivations rather than economic strategic reasons.

Our findings suggest that smart sanctions can impose significant economic costs on targeted firms. Rather than only reflecting the sanctioning country's stance, they can materially harm the interests of those being targeted. However, even though sanctions can be targeted, national governments still have the option to step in and shield the targets. By awarding additional loans and subsidies, the cost of sanctions can be nationalized and absorbed more broadly.

Although our study offers novel evidence on the effects of international sanctions on the market value and financial performance of targeted firms, several potential impacts are omitted. First, we do not investigate the political impacts in terms of the changes in the undesirable policies. Clearly, this is the ultimate goal of the sanctions, but designing sanctions that have real economic consequences is a first step in this process. Second, we focus only on the economic harm that sanctions impose on publicly-listed firms. Given the recency of the sanctions, we are not able to measure their economic impact on non-listed Chinese companies and spillover

effects on both upstream suppliers and downstream customers via the production network, nor the consequences on Chinese international trade or the costs incurred by Chinese households. Third, we find a reduction in firms' market valuation after investment or financial sanctions, but not when trade sanctions are imposed. A possible reason might be that this type of sanctions is most easily circumvented. Future research can help to fill these gaps.

References

Acemoglu, D., Johnson, S., Kermani, A., Kwak, J., & Mitton, T. (2016). The value of connections in turbulent times: Evidence from the united states. *Journal of Financial Economics*, *121*(2), 368–391.

Afesorgbor, S. K., & Mahadevan, R. (2016). The impact of economic sanctions on income inequality of target states. *World Development*, *83*, 1–11.

Ahn, D. P., & Ludema, R. D. (2020). The sword and the shield: The economics of targeted sanctions. *European Economic Review*, *130*, 103587.

Arkhangelsky, D., Athey, S., Hirshberg, D. A., Imbens, G. W., & Wager, S. (2021). Synthetic difference-in-differences. *American Economic Review*, *111*(12), 4088–4118.

Athey, S., & Imbens, G. W. (2022). Design-based analysis in difference-in-differences settings with staggered adoption. *Journal of Econometrics*, *226*(1), 62–79. https://doi.org/https://doi.org/10.1016/j.jeconom.2020.10.012

Bao, Y. (2020). The xinjiang production and construction corps: An insider's perspective. *China: An International Journal*, *18*(2), 161–174.

Besedeš, T., Goldbach, S., & Nitsch, V. (2017). You're banned! The effect of sanctions on German cross-border financial flows. *Economic Policy*, *32*(90), 263–318.

Besedeš, T., Goldbach, S., & Nitsch, V. (2021). Cheap talk? Financial sanctions and non-financial firms. *European Economic Review*, *134*, 103688.

Bukharin, I. (2021). *Long shadows: How the global economy supports oppression in Xinjiang.* C4ADS.

Cengiz, D., Dube, A., Lindner, A., & Zipperer, B. (2019). The effect of minimum wages on low-wage jobs. *Quarterly Journal of Economics*, *134*(3), 1405–1454.

Chang, E. C., & Wong, S. M. (2004). Political control and performance in China's listed firms. *Journal of Comparative Economics*, *32*(4), 617–636. https://doi.org/https://doi.org/10.1016/j.jce.2004.08.001

Chen, J., & Roth, J. (2023). Logs with Zeros? Some Problems and Solutions. *Quarterly Journal of Economics*, *139*(2), 891–936. https://doi.org/10.1093/qje/qjad054

Chen, T., & Kung, J. K.-s. (2019). Busting the "princelings": The campaign against corruption in china's primary land market. *The Quarterly Journal of Economics*, *134*(1), 185–226.

Chorzempa, M., Lovely, M. E., & Wan, C. (2024). *The rise of US economic sanctions on China: Analysis of a new PIIE dataset* [Peterson Institute for International Economics, Policy Brief No. 24-14].

Crozet, M., & Hinz, J. (2020). Friendly fire: The trade impact of the Russia sanctions and counter-sanctions. *Economic Policy*, *35*(101), 97–146.

Crozet, M., Hinz, J., Stammann, A., & Wanner, J. (2021). Worth the pain? Firms' exporting behaviour to countries under sanctions. *European Economic Review*, *134*, 103683.

de Chaisemartin, C., & D'Haultfœuille, X. (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review*, *110*(9), 2964–96.

Draca, M., Garred, J., Stickland, L., & Warrinnier, N. (2023). On target? Sanctions and the economic interests of elite policymakers in Iran. *The Economic Journal*, *133*(649), 159–200.

Dube, A., Kaplan, E., & Naidu, S. (2011). Coups, corporations, and classified information. *The Quarterly journal of economics*, *126*(3), 1375–1409.

Felbermayr, G., Morgan, T. C., Syropoulos, C., & Yotov, Y. V. (2024). *Economic sanctions: Stylized facts and quantitative evidence* [CGPA Working Paper No. 2024-01].

Fisman, R. (2001). Estimating the value of political connections. *American economic review*, *91*(4), 1095–1102.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, *225*(2), 254–277.

Guidolin, M., & La Ferrara, E. (2007). Diamonds are forever, wars are not: Is conflict bad for private firms? *American Economic Review*, *97*(5), 1978–1993.

Gutmann, J., Neuenkirch, M., & Neumeier, F. (2023). The economic effects of international sanctions: An event study. *Journal of Comparative Economics*, *51*(4), 1214–1231.

Haidar, J. I. (2017). Sanctions and export deflection: Evidence from Iran. *Economic Policy*, *32*(90), 319–355.

Hufbauer, G. C., Schott, J. J., & Elliott, K. A. (1990). *Economic sanctions reconsidered: History and current policy* (Vol. 1). Peterson Institute.

Kelishomi, A. M., & Nisticò, R. (2022). Employment effects of economic sanctions in iran. *World Development*, *151*, 105760.

Kim, J., Kim, K., Park, S., & Sun, C. (2023). The economic costs of trade sanctions: Evidence from North Korea. *Journal of International Economics*, *145*, 103813.

Laeven, L., & Majnoni, G. (2003). Loan loss provisioning and economic slowdowns: Too much, too late? *Journal of financial intermediation*, *12*(2), 178–197.

Lehr, A. (2020). *Addressing forced labor in the Xinjiang Uyghur Autonomous Region*. Center for Strategic & International Studies Briefs. https://csis-website-prod.s3.amazonaws.com/s3fs-public/publication/200730_Lehr_XinjiangUyghurForcedLabor_brief_FINAL_v2.pdf

Li, H., Meng, L., Wang, Q., & Zhou, L.-A. (2008). Political connections, financing and firm performance: Evidence from chinese private firms. *Journal of development economics*, *87*(2), 283–299.

Li, Z., & Cheng, L. (2020). What do private firms do after losing political capital? evidence from china. *Journal of Corporate Finance*, *60*, 101551.

Liu, G., Liu, Y., Zhang, C., & Zhu, Y. (2021). Social insurance law and corporate financing decisions in china. *Journal of Economic Behavior & Organization*, *190*, 816–837.

Liu, L. X., Shu, H., & Wei, K. J. (2017). The impacts of political uncertainty on asset prices: Evidence from the bo scandal in china. *Journal of financial economics*, *125*(2), 286–310.

MacKinlay, A. C. (1997). Event studies in economics and finance. *Journal of economic literature*, *35*(1), 13–39.

Marinov, N. (2005). Do economic sanctions destabilize country leaders? *American Journal of Political Science*, *49*(3), 564–576.

Mauldin, W., & McBride, C. (2020, July 30). Pompeo warns new measures planned for China over human rights. https://www.wsj.com/articles/pompeo-warns-new-measures-planned-for-china-over-human-rights-11596120988?mod=e2tw

Maystadt, J.-F., Tsang, K., Van Biesebroeck, J., & Warrinnier, N. (2024). Targeting the corps: The incidence of humanitarian sanctions on firm performance in China. *CEPR Discussion Paper*, (18790).

Morgan, T. C., Syropoulos, C., & Yotov, Y. V. (2023). Economic sanctions: Evolution, consequences, and challenges. *Journal of Economic Perspectives*, *37*(1), 3–29.

Murphy, L., Elimä, N., & Tobin, D. (2022). *Until nothing is left: China's settler corporation and its human rights violations in the Uyghur Region. A report on the Xinjiang Production and Construction Corps* [Sheffield Hallam University. Helena Kennedy Centre for International Justice]. https://www.shu.ac.uk/-/media/home/research/helena-kennedy-centre/projects/until-nothing-is-left-docs/murphy-elima-tobin----until-nothing-is-left.pdf

Neuenkirch, M., & Neumeier, F. (2016). The impact of us sanctions on poverty. *Journal of Development Economics*, *121*, 110–119.

Nigmatulina, D. (2022). *Sanctions and misallocation: How sanctioned firms won and Russia lost* [CEP Discussion Paper No. 1886].

Office of the United Nations High Commissioner for Human Rights. (2022). *OHCHR assessment of human rights concerns in the Xinjiang Uyghur Autonomous Region, People's Republic of China*. https://www.ohchr.org/sites/default/files/documents/countries/2022-08-31/22-08-31-final-assesment.pdf

Rambachan, A., & Roth, J. (2023). A more credible approach to parallel trends. *Review of Economic Studies*, *90*(5), 2555–91.

Scheckenhofer, L., Teti, F. A., & Wanner, J. (2025). *Dodging trade sanctions? Evidence from military goods* [CESifo Working Paper No. 11743].

Song, Z., Storesletten, K., & Zilibotti, F. (2011). Growing like China. *American economic review*, *101*(1), 196–233.

Sun, L., & Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, *225*(2), 175–199.

Tsang, K. (2023). Humanitarian sanctions in china: Evidence from stock market data [Unpublished master's thesis, KU Leuven].

Wang, Y. (2020). *Chinese Listed Firms Personnel Database*. Harvard Dataverse. https://doi.org/10.7910/DVN/TPR4VU

Wu, G. L. (2018). Capital misallocation in china: Financial frictions or policy distortions? *Journal of Development Economics*, *130*, 203–223.

Yuan, W., Ouyang, D., & Zhang, Z. (2022). Did china's bank ownership reform improve credit allocation? *European Economic Review*, *141*, 103782.

Appendix 2.A Background Information on Targeted Entities

2.A.1 Chinese Military Companies

In compliance with Section 1237 of the Strom Thurmond National Defense Authorization Act of 1999, the US Department of Defense (DoD) placed an initial batch of 20 Communist Chinese Military Companies on the CCMC List on June 12, 2020. This included companies like Norinco Group, Huawei, and China General Nuclear Power Corp., which are considered to be engaged in defense and strategic sectors. The DoD regards them as essential to the Chinese military-civil fusion development strategy, as they supply advanced technologies to the People's Liberation Army of China. By January 2021, the DoD's list extended to 44 companies. Most of them are state-owned enterprises and are publicly listed in China, Hong Kong, or the United States, either directly or through their subsidiaries.

The OFAC first released the NSDN-CCMC list on December 28, 2020. It included all companies on the DoD's list, except 9 companies added by the DoD only in January 2021. It also encompassed subsidiaries whose names "closely matched" those listed on the DoD's list or the original NSDN-CCMC list, although the definition of closely matched changed multiple times. The investment sanctions outlined in Executive Order (EO) 13959 (and EO 13974) applied to the companies listed on the DoD's list and NSDN-CCMC list.

The Biden administration revamped the investment sanction regime by substituting EO 13974 with EO 14032 and directing the OFAC to oversee a new NSDN list named the "Non-SDN Chinese Military-Industrial Complex Company" (CMIC) on June 3, 2021. By the end of 2022, the NSDN-CMIC list includes 68 Chinese companies, most of them are state-owned. Of these, 26 originated from the old CCMC list, while 18 from the old list have been excluded and are no longer subject to investment sanctions. The remaining companies are newly added by the OFAC. The new investment sanctions apply exclusively to the companies explicitly identified on the NSDN-CMIC list, and not to subsidiaries with names that "closely matched". The listed companies are considered to be engaged in defense and strategic sectors, such as aviation, nuclear technology and shipbuilding, and supporting China's military-civil fusion strategy. However, the NSDN-CMIC list also targets Chinese companies involved in the development and use of surveillance technology to "facilitate repression or serious human rights abuse constitute un-usual and extraordinary threats."

2.A.2 Xinjiang Production and Construction Corps (XPCC)

The Xinjiang Production and Construction Corps, also known as Bingtuan ("The Corps"), is a state-owned conglomerate in Xinjiang, a province in Northwest China. The XPCC was established in 1954 under the orders of Mao Zedong with the dual objective of maintaining border security and promoting local economic development. Similar policies were used in the Tang and Qing dynasties under a system named

"tuntian", in which military units settled in sparsely populated frontier areas. The central government exercises direct control over the XPCC by appointing its highest-ranking officials and supervising its economic planning (Bao, 2020).

The XPCC was also tasked with maintaining control over this religious and ethnically diverse region (Bao, 2020). Over time, it built farms and towns, and provided land and employment to disbanded military units, as well as re-settled Han migrants from other parts of China as part of a larger assimilation campaign. Dissolved during the Cultural Revolution in 1975, the XPCC was reinstated in 1981 and remains the only production and construction corps in China.

The XPCC's headquarters are located in Urumqi and it has 14 divisions and 176 regiments (as of 2013), many of which are clustered along the Chinese border.[39] By the end of 2019, 3.25 million people lived in areas controlled by the XPCC, representing 13 percent of the population of Xinjiang.[40]

To fulfill its mandated tasks, the XPCC has a distinctive administrative and governance structure, as detailed in Bao (2020). First, it maintains a military organizational structure that integrates government and party functions. Such a hierarchical system facilitates the XPCC's coordination and organization of large-scale operations. Second, although it is officially under Xinjiang province, the XPCC exercises autonomous authority over domestic affairs and manages its own separate administrative, economic, educational, and judicial systems. Given this dual governance structure in Xinjiang, the XPCC is often described as a state within the state (Murphy et al., 2022).

Moreover, the XPCC is an important economic conglomerate in China. Through a sprawling corporate network, the XPCC owns and controls thousands of companies spanning a wide range of industrial sectors, such as civil engineering, mining and power generation, but also in agriculture. Its products, especially cotton and tomatoes, have been deeply embedded in national and global supply chains (Bukharin, 2021; Murphy et al., 2022). For example, in 2019, XPCC produced 2,028 thousand tons of cotton, accounting for 34 percent of total cotton production in China. GDP produced in the XPCC-controlled area amounts to 274.7 billion RMB, approximately 20 percent of total Xinjiang GDP.

[39]The total number of the XPCC regiments has been reported differently by various sources. According to several Chinese government organs, there were 176 regiments by the end of 2013. However, the China Central Television, an official media outlet, stated that there were 185 regiments in 2009, including 11 regiments specifically focused on construction and engineering.

[40]Statistics on XPCC in this subsection are taken from various issues of the Statistical Communiqué of Xinjiang Production and Construction Corps.

Appendix 2.B Industry Composition of Firms

Table 2.B.1: Number of firms in targeted and control portfolios by industry

Industry (2-digit)	Targeted portfolio	Control portfolio
Agriculture	2	12
Air Transport	1	9
Ancillary Activities for Mining	2	13
Animal Husbandry	1	13
Automobile Manufacturing	6	112
Business Services	1	52
Civil Engineering Construction	7	54
Computer, Communication and Other Electronic Device Manufacturing	38	264
Construction Decoration and Other Construction	1	25
Construction and Installation	1	1
Electric Machines and Apparatuses Manufacturing	7	206
Farm and Sideline Products Processing	3	46
Food Manufacturing	1	40
General Equipment Manufacturing	2	111
Instrument and Meter Manufacturing	1	41
Medicine Manufacturing	2	211
Non-metallic Mineral Products	2	81
Petroleum Processing, Coking and Nuclear Fuel Processing	1	13
Production and Supply of Electric and Thermal Power	5	58
Professional Technical Services	1	33
Railway, Shipbuilding, Aerospace and Other Transportation Equipment Manufacturing	12	23
Raw Chemical Materials and Chemical Products	8	209
Real Estate Industry	1	105
Research and Experimental Development	1	5
Retailing	2	80
Rubber and Plastic Product Industry	1	65
Software and IT Services	10	176
Special Equipment Manufacturing	10	173
Telecommunications, Broadcast, Television and, Satellite Transmission Services	1	12
Textile Industry	1	34
Wholesale	1	76
Wine, Drinks and Refined Tea Manufacturing	1	40
Total	134	2,393

Notes: The number of companies is for the sample used in the analysis using daily stock market data. The sample used in the analysis on quarterly accounting data differs only slightly.

Appendix 2.C Variable definitions

Table 2.C.1: Stock market performance: Definition of variables

Variable	Description	Source
Return	Raw daily stock returns: $100 \times (Closing_t - Closing_{t-1})/Closing_{t-1}$.	CSMAR
$CAR_{m,n}$	Cumulative abnormal returns between trading day m and n.	CSMAR
Change ratio	Daily stock returns from previous ex-right closing price: $100 \times (Closing_t - Closing_{t-1}^{ex-right})/Closing_{t-1}^{ex-right}$.	CSMAR
Quarterly return	Raw quarterly stock returns from the closing price at the end of each quarter: $100 \times (Closing_q - Closing_{q-1})/Closing_{q-1}$.	CSMAR
Target	Binary variable is equal to 1 if the listed company is classified as a targeted company.	Aiqicha and CSMAR
Event	Binary variable is equal to 1/2 if the trading days are either the first or second trading day since the U.S. sanction.	Hand collected by authors
*Day*1	Binary variable is equal to 1 if the it is the first trading day since the U.S. sanction.	Hand collected by authors
*Day*2	Binary variable is equal to 1 if the it is the second trading day since the U.S. sanction.	Hand collected by authors
Return on equity	Returns on equity as of 2017Q4. Defined as $Net\ profit/Total\ equity$.	CSMAR
Leverage ratio	Debt to capital ratio as of 2017Q4. Defined as $Total\ liabilities/(Total\ liabilities + Total\ equity)$	CSMAR
Market cap.	Average market capitalization for 2017, inverse hyperbolic sine transformed.	CSMAR

Table 2.C.2: Financial performance: Definition of variables

Variable	Description	Source
Operating revenue	Operating revenue transformed by inverse hyperbolic sine transformation.	CSMAR
Net profit	Net profit scaled by four quarter lag of total assets.	CSMAR
EBIT	Earnings before interest and taxes, which is defined as the sum of net profit, income tax expenses, and financial expenses, scaled by four quarter lag of total assets.	CSMAR
Inventory	Net inventories scaled by four quarter lag of total assets.	CSMAR
Total assets	Total assets transformed by inverse hyperbolic sine transformation.	CSMAR
Net fixed assets	Net fixed assets transformed by inverse hyperbolic sine transformation.	CSMAR
Cash outflow for acquiring capital	Total cash outflow for acquisition and construction of fixed assets, intangible assets, and other long-term assets scaled by four quarter lag of total assets.	CSMAR
Cash and equivalent	Total cash and cash equivalent scaled by four quarter lag of total assets.	CSMAR
Cash from operating	Net cash flow from operating activities scaled by four quarter lag of total assets.	CSMAR
Working capital	Current assets minus current liabilities, scaled by four quarter lag of total assets.	CSMAR
Total debt	Total loans scaled by four quarter lag of total assets.	CSMAR
Short-term debt	Short term loans scaled by four quarter lag of total assets.	CSMAR
Average interest rate	Total interest expenses divided by total liabilities. Restricted to values laying between 0% and 100%	CSMAR
Subsidy	Other income scaled by four quarter lag of total assets.	CSMAR
Average tax rate	Income tax expenses divided by earnings before taxes. Restricted to values laying between 0% and 100%.	CSMAR
Target	Binary variable is equal to 1 if the listed company is classified as target portfolio.	Aiqicha and CSMAR
Post	Binary variable is equal to 1 for all quarters following the first instance of sanctions imposed on the listed company.	Hand collected by authors

Appendix 2.D Cumulative abnormal returns model

One of the robustness checks in Section 2.4.3 uses a cumulative abnormal returns (CAR) model to construct an alternative dependent variable. Details on the calculations are as follows:

1. We employ a standard market model to construct a 2-day cumulative abnormal return for each publicly-listed company, using the following regression equation (for details, see MacKinlay (1997)) :

$$Return_{it} \quad = \quad \gamma_i + \beta\, R_t + \epsilon_{it}, \tag{2.D.1}$$

   where R_t is the capitalization-weighted market return on trading day t. We first partition the sample into targeted and control portfolios using the same approach as for the standard event study analysis, but separately for each sanction event. Then, we estimate this model on the combined portfolios over a pre-event period that spans 125 trading days, ending 30 days prior to the commencement of the relevant US sanction event.

2. Utilizing the estimated parameters from the market model, we compute a two-day cumulative abnormal return for each firm as $CAR_i[0,1] = \sum_{\tau=0}^{1}(Return_{i\tau} - \hat{\gamma}_i - \hat{\beta}R_\tau)$ where $\tau = 0$ and $\tau = 1$ represent the first and second trading days since the start of the sanction event.

3. Finally, we combine the observations over all individual sanction events and estimate the following cross-sectional regression analysis over the aggregate sample:

$$CAR_i[0,1] \quad = \quad \delta + \phi\, Target_i + X_i\Psi + \gamma_j + \gamma_p + \gamma_o + \gamma_c + \varepsilon_i, \tag{2.D.2}$$

   where X_i denotes a set of firm-level covariates for firm i, γ_j are sector fixed effects, γ_p are province fixed effects, γ_o is a state-owned enterprise indicator, and γ_c are sanction episode fixed effects, one for each sanction event.

Appendix 2.E Additional robustness checks

In this section, we present the results of a variety of robustness checks for the analysis shown in the main text. The various tables and figures are ordered in the same way they are discussed in the text. First, we present additional results for the sanction effects on the stock market performance of Chinese listed companies around the announcement of US sanctions. Second, we report additional findings on the financial performance of Chinese firms over the period from 2018Q3 to 2022Q2.

Table 2.E.1: Stock market performance: Robustness checks

	(1) Alternative Outcome	(2) Firm-Day of Week	(3) Quarter-Sector-Day of Week	(4) All Sectors	(5) All Firms	(6) Only SOE	(7) Matched Sample (CEM)
Target × *Event*	-1.956***	-1.975***	-1.838***	-1.965***	-1.744***	-1.799***	-1.275**
	(0.392)	(0.391)	(0.387)	(0.393)	(0.375)	(0.411)	(0.514)
Observations	2,161,466	2,161,466	2,161,462	2,777,901	2,885,379	586,309	544,005
Firm FE	✓		✓	✓	✓	✓	✓
Day FE	✓	✓	✓	✓	✓	✓	✓
Firm-Day of Week FE		✓					
Quarter-Sector-Day of Week FE			✓				
Number of firms	2,527	2,527	2,527	3,239	3,365	701	650
Number of sanctioned	134	134	134	134	143	104	97

	(8) Winsorized	(9) No ST	(10) Sector-clustered SE	(11) Episode-clustered SE	(12) Ownership Threshold 30%	(13) Ownership Threshold 40%	(14) Ownership Threshold 50%
Target × *Event*	-1.885***	-2.014***	-1.961***	-1.961***	-1.974***	-1.989***	-1.904***
	(0.381)	(0.395)	(0.386)	(0.459)	(0.405)	(0.410)	(0.440)
Observations	2,161,466	1,878,666	2,161,466	2,161,466	2,159,264	2,158,084	2,153,260
Firm FE	✓	✓	✓	✓	✓	✓	✓
Day FE	✓	✓	✓	✓	✓	✓	✓
Number of firms	2,527	2,182	2,527	2,527	2523	2521	2512
Number sanctioned	134	123	134	134	130	128	119

Notes: The dependent variable is the daily stock return (in %), except for column (1) where it is the change ratio (multiplied by 100). 'Events' are considered to be in effect on the last two trading days and the sample window extends to sixty trading days prior. In column (2), interactions between firm and day of the week fixed effects replace firm fixed effects. In column (3), we add quarter-sector-day of week fixed effects. In column (4), companies from all industrial sectors (except finance) are included in the control group. In column (5), all firms, including those omitted from the baseline sample for reasons specified in Section 2.3.2, are included in the control group. . In column (6), we only include state-owned enterprises in the sample. In column (7), we restrict the analysis to a matched sample using the coarsened exact matching with sector dummies. In column (8), we winsorize daily stock return (in %) at the 1st and 99th percentile. In column (9), we exclude companies with "Special Treatment" status. In column (10), we cluster standard errors at the sector level. In column (11), we cluster standard errors at the sanction episode level. From Columns (12)–(14), we vary the ownership threshold to 30%, 40%, and 50% and exclude sanctioned companies whose ownership shares fall below the corresponding threshold, respectively. Except for columns (10) and (11), standard errors (in parentheses) are clustered at the firm level; Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

Table 2.E.2: Stock market performance: Quarterly returns

	Baseline	Cengiz et al.	Sun and Abraham
	(1)	(2)	(3)
$Target \times Post$	-3.405**	-3.360**	-3.268**
	(1.420)	(1.392)	(1.269)
Observations	32,350	251,527	32,350
Firm FE	✓	✓	✓
Quarter FE	✓	✓	✓
Number of firms	2,040	2,040	2,040
Number sanctioned	65	65	65

Notes: We estimate specification (2.1) with stock returns aggregated by quarter. The outcome variable is the quarterly stock return (in %) calculated as $100 * \frac{(clo_q - clo_{q-1})}{clo_q}$ with clo_q the closing price at the end of each quarter. The sample period is from 2018Q3 to 2022Q2. The event indicator *Post* switches to one in the quarter a firm is first sanctioned and remains equal to one till the end of the sample period. The estimators employed are shown in the column heads. Standard errors (in parentheses) are clustered at the firm level; Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

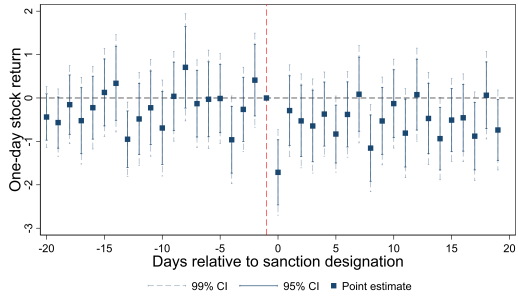
Table 2.E.3: Stock market performance: CAR model

	(1)	(2)	(3)
$Target \times Event$	-0.809**	-0.789**	-1.455***
	(0.402)	(0.402)	(0.413)
Market Cap. (IHS)	-0.379***	-0.388***	-0.378***
	(0.067)	(0.067)	(0.067)
Average return (%)	-0.040	-0.049	-0.049
	(0.082)	(0.077)	(0.076)
Return on equity	0.002	-0.006	-0.003
	(0.007)	(0.007)	(0.007)
Leverage ratio	-0.167	-0.243	-0.253
	(0.266)	(0.279)	(0.279)
Observations	6,964	6,964	6,964
Sector FE		✓	✓
Province FE		✓	✓
SOE FE		✓	✓
Episode FE			✓
Number of firms	2,371	2,371	2,371
Number sanctioned	124	124	124

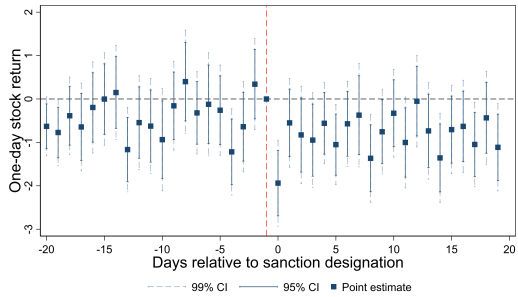
Notes: The dependent variable is the two-day cumulative abnormal return (in %) starting from the day of the US sanction designation. Details on the CAR model are in Appendix 2.D. Standard errors (in parentheses) are clustered at the firm level; Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

Figure 2.E.1: Event study based on alternative estimators: Stock market effects

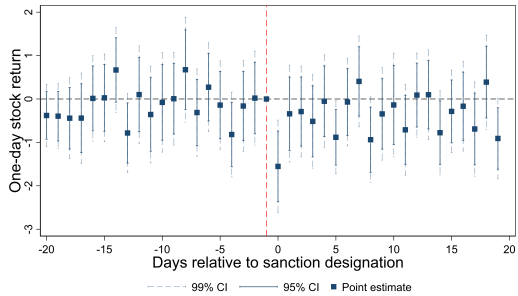
(a) Estimator of Cengiz et al. (2019)



(b) Estimator of Sun and Abraham (2021)



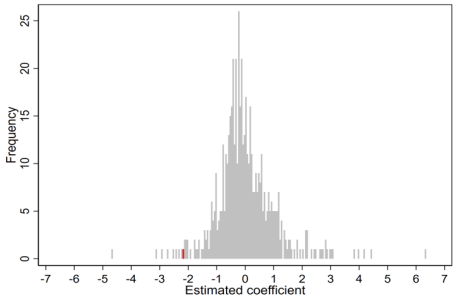
(c) Estimator on (PSM) matched sample



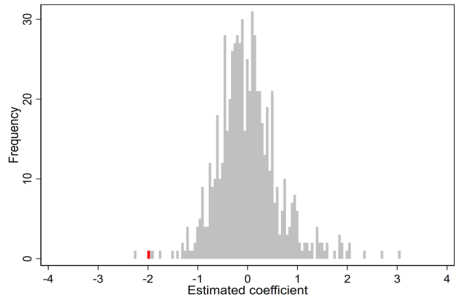
Notes: Coefficient estimates on the interaction terms between the target portfolio dummy and leading-lagging dummies for 20 days before and 20 days after the sanction designation (the vertical red line is the day before). The vertical segments are the 95 percent confidence intervals. All point estimates (one-day stock returns) are normalized relative to the omitted period, $t = -1$. Panels (a) and (b) use the estimators proposed by Cengiz et al. (2019) and Sun and Abraham (2021), respectively, with firm fixed effects, sector-province-day fixed effects, and four firm-characteristics-by-day fixed effects. Panel (c) follows the specification in column (5) of Table 2.4.

Figure 2.E.2: Placebo tests: Financial and investment sanctions

(a) Financial sanctions (one-day event)



(b) Investment sanctions (one-day event)



Notes: All coefficients are estimated using specification (2.1). The coefficients of the "true" sanction effects are reported in black. Panel (a) is the histogram of all point estimates from 545 regressions with "placebo" one-day interaction term between May 9, 2018 and July 31, 2020 and the 13 XPCC-related firms subject to financial GloMag sanctions constitute the target portfolio. Panel (b) is the histogram of point estimates from 657 regressions with as target portfolio the 78 firms subjected to investment sanctions on Jan 14, 2021. In panel (a), the t-statistics of the true effect is at the top 1st% (the 1st highest value); in panel (b) it is at the top 4$ (the 24th highest value).

Table 2.E.4: Financial performance: Effect of US trade sanctions by ownership type

(a) Profitability	(1) Operating revenue	(2) Net profit	(3) EBIT	(4) Inventory
$Target \times Post$	-0.138	-0.498	0.204	0.623
	(0.123)	(0.509)	(0.807)	(1.440)
$SOE \times Target \times Post$	0.249*	0.458	-0.446	2.151
	(0.147)	(0.540)	(0.840)	(1.948)
Observations	27,141	27,169	19,934	26,928
Mean of dependent variable	20.66	0.984	2.161	14.95
R-squared	0.903	0.177	0.239	0.397
Firm FE	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓
Number of firms	1,713	1,713	1,665	1,709
Number sanctioned	32	32	27	32

(b) Firm Size	(5) Total assets	(6) Net fixed assets	(7) Capital expenses
$Target \times Post$	-0.085	-0.054	-0.524
	(0.080)	(0.068)	(0.429)
$SOE \times Target \times Post$	0.093	0.012	0.440
	(0.089)	(0.082)	(0.431)
Observations	27,211	27,210	26,168
Mean of dependent variable	22.73	20.66	1.439
R-squared	0.961	0.939	0.234
Firm FE	✓	✓	✓
Quarter FE	✓	✓	✓
Number of firms	1,713	1,713	1,711
Number sanctioned	32	32	32

(c) Liquidity	(8) Cash	(9) Operating cash inflow	(10) Working capital
$Target \times Post$	-0.709	0.021	-1.202
	(2.653)	(0.295)	(3.976)
$SOE \times Target \times Post$	-0.551	-0.033	1.252
	(2.897)	(0.342)	(4.080)
Observations	27,168	27,169	27,168
Mean of dependent variable	18.17	1.377	28.39
R-squared	0.331	0.120	0.421
Firm FE	✓	✓	✓
Quarter FE	✓	✓	✓
Number of firms	1,713	1,713	1,713
Number sanctioned	32	32	32

Notes: The sample period covers sixteen quarters between 2018Q3 and 2022Q2. The *Post* indicator switches to one from the moment a targeted company is first hit by trade sanctions. The dependent variable of each regression is indicated in the column heading. In columns (1), (5), and (6), the dependent variables are inverse hyperbolic sine transformed. All other outcome variables are scaled by a four-quarter lag of total assets. Standard errors (in parentheses) are clustered at the firm level; Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

Table 2.E.5: Financial performance: Effect of US investment sanctions by ownership type

(a) Profitability	(1) Operating revenue	(2) Net profit	(3) EBIT	(4) Inventory
$Target \times Post$	0.128	-0.110	0.310	1.512
	(0.173)	(0.403)	(0.493)	(1.182)
$SOE \times Target \times Post$	-0.016	-0.408	-0.766	-3.570
	(0.184)	(0.515)	(0.674)	(2.761)
Observations	11,411	11,423	8,186	11,356
Mean of dependent variable	20.83	0.894	2.077	17.30
R-squared	0.916	0.166	0.226	0.312
Firm FE	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓
Number of firms	723	723	701	723
Number sanctioned	20	20	20	20

(b) Firm Size	(5) Total assets	(6) Net fixed assets	(7) Capital expenses
$Target \times Post$	0.073	0.103	0.444
	(0.075)	(0.150)	(0.364)
$SOE \times Target \times Post$	0.017	-0.124	-0.526
	(0.095)	(0.155)	(0.389)
Observations	11,440	11,439	10,991
Mean of dependent variable	22.87	20.68	1.384
R-squared	0.965	0.949	0.226
Firm FE	✓	✓	✓
Quarter FE	✓	✓	✓
Number of firms	723	723	722
Number sanctioned	20	20	20

(c) Liquidity	(8) Cash	(9) Operating cash inflow	(10) Working capital
$Target \times Post$	-8.244	0.458	-4.807
	(9.560)	(0.937)	(11.437)
$SOE \times Target \times Post$	4.278	-1.183	2.239
	(10.210)	(0.991)	(12.838)
Observations	11,423	11,423	11,423
Mean of dependent variable	18.79	1.125	28.66
R-squared	0.284	0.117	0.368
Firm FE	✓	✓	✓
Quarter FE	✓	✓	✓
Number of firms	723	723	723
Number sanctioned	20	20	20

Notes: The sample period covers sixteen quarters between 2018Q3 and 2022Q2. The *Post* indicator switches to one from the moment a targeted company is first hit by investment sanction. The dependent variable of each regression is indicated in the column heading. In columns (1), (5), and (6), the dependent variables are inverse hyperbolic sine transformed. All other outcome variables are scaled by a four-quarter lag of total assets. Standard errors (in parentheses) are clustered at the firm level; Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

Table 2.E.6: Financial performance: Effect of US financial (GloMag) sanctions on XPCC-subsidiaries

(a) Profitability	(1) Operating revenue	(2) Net profit	(3) EBIT	(4) Inventory
Target × *Post*	0.062	-0.075	0.290	5.322***
	(0.103)	(0.544)	(0.643)	(2.032)
Observations	9,040	9,054	7,102	8,967
Mean of dependent variable	20.96	1.342	2.536	13.78
Firm FE	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓
Number of firms	570	570	558	570
Number sanctioned	13	13	12	13
(b) Firm Size	(5) Total assets	(6) Net fixed assets	(7) Capital expenses	
Target × *Post*	-0.119	-0.105	-0.369	
	(0.081)	(0.105)	(0.317)	
Observations	9,072	9,072	8,670	
Mean of dependent variable	23.05	21.44	1.906	
Firm FE	✓	✓	✓	
Quarter FE	✓	✓	✓	
Number of firms	570	570	569	
Number sanctioned	13	13	13	
(c) Liquidity	(8) Cash	(9) Operating cash inflow	(10) Working capital	
Target × *Post*	1.175	-0.483	-1.800	
	(3.024)	(0.600)	(2.905)	
Observations	9,054	9,054	9,054	
Mean of dependent variable	17.58	1.900	17.38	
Firm FE	✓	✓	✓	
Quarter FE	✓	✓	✓	
Number of firms	570	570	570	
Number sanctioned	13	13	13	

Notes: The sample period covers sixteen quarters between 2018Q3 and 2022Q2. The *Post* indicator switches to one from the moment any of the XPCC-affiliated companies is hit by financial sanctions. The dependent variable of each regression is indicated in the column heading. In columns (1), (5), and (6), the dependent variables are inverse hyperbolic sine transformed. All other outcome variables are scaled by a four-quarter lag of total assets. Standard errors (in parentheses) are clustered at the firm level; Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

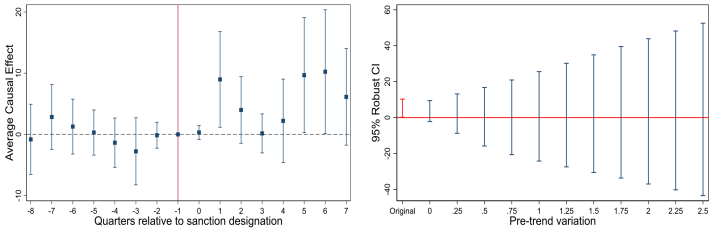
Table 2.E.7: Financial performance: Effects on XPCC-subsidiaries, alternative specifications

(a) Sector-by-Quarter FE	Inventory	Capital Expenses	Total borrowing	Short-term borrowing	Avg Interest Rate	Subsidy	Avg. Tax Rate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Target × Post	4.267**	-0.469	14.87*	8.896***	-0.058	0.074**	-0.420
	(1.739)	(0.285)	(8.704)	(2.959)	(0.048)	(0.037)	(2.030)
Observations	8,967	8,670	5,167	7,444	7,253	8,493	6,296
Firm FE	✓	✓	✓	✓	✓	✓	✓
Sector-Quarter FE	✓	✓	✓	✓	✓	✓	✓
Mean of dependent variable	13.87	1.906	29.35	15.17	0.549	0.137	18.97
Number of firms	570	569	431	526	544	566	539
Number sanctioned	13	13	10	11	12	13	12
(b) Winsorized 1% and 99%	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Target × Post	4.420**	-0.406	8.382**	7.065***	-0.050	0.036**	0.695
	(1.862)	(0.276)	(3.717)	(2.420)	(0.046)	(0.018)	(1.785)
Observations	8,967	8,670	5,173	7,444	7,255	8,493	6,298
Firm FE	✓	✓	✓	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓	✓	✓	✓
Mean of dependent variable	13.24	1.749	25.79	13.89	0.538	0.121	18.85
Number of firms	570	569	431	526	544	566	539
Number sanctioned	13	13	10	11	12	13	12
(c) No ST	(15)	(16)	(17)	(18)	(19)	(20)	(21)
Target × Post	2.310	-0.383	14.63**	6.797***	0.008	0.061***	-0.893
	(1.762)	(0.412)	(6.896)	(2.597)	(0.052)	(0.022)	(2.144)
Observations	7,676	7,502	4,491	6,382	6,192	7,373	5,769
Firm FE	✓	✓	✓	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓	✓	✓	✓
Mean of dependent variable	13.79	1.962	28.83	14.08	0.512	0.139	18.64
Number of firms	484	484	369	445	463	482	471
Number sanctioned	9	9	8	8	8	9	9
(d) Only SOE sample	(22)	(23)	(24)	(25)	(26)	(27)	(28)
Target × Post	5.149**	-0.621*	6.261	8.839**	-0.049	0.073	0.466
	(2.319)	(0.353)	(4.226)	(3.717)	(0.048)	(0.052)	(2.005)
Observations	3,392	3,269	2,168	2,752	2,764	3,198	2,415
Firm FE	✓	✓	✓	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓	✓	✓	✓
Mean of dependent variable	13.22	1.622	30.38	15.21	0.541	0.129	20.04
Number of firms	214	213	168	189	204	213	206
Number sanctioned	13	13	10	11	12	13	12
(e) All companies	(29)	(30)	(31)	(32)	(33)	(34)	(35)
Target × Post	3.791**	-0.149	10.95***	8.462**	-0.069	0.100**	0.388
	(1.915)	(0.273)	(3.911)	(3.515)	(0.045)	(0.051)	(1.906)
Observations	48,664	47,361	26,222	39,598	38,942	46,543	32,191
Firm FE	✓	✓	✓	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓	✓	✓	✓
Mean of dependent variable	15.16	1.451	23.02	12.85	0.516	0.183	18.86
Number of firms	3,098	3,106	2,222	2,847	2,978	3,094	2,908
Number sanctioned	13	13	10	11	12	13	12

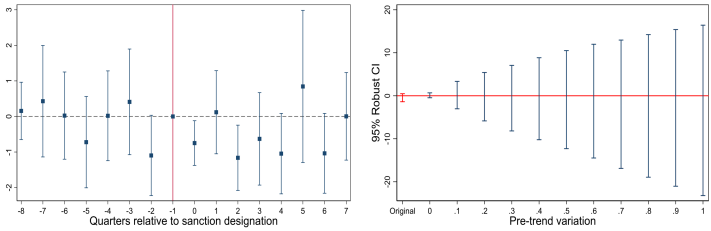
Notes: The sample period covers sixteen quarters between 2018Q3 and 2022Q2. The *Post* indicator switches to one in 2020Q3 when financial (GloMag) sanctions were imposed on 13 XPCC-affiliated companies, the target portfolio, which are all SOEs. Apart from panel (e), the control portfolio consists solely of non-sanctioned companies operating in the same sectors as those in the target portfolio. The dependent variables are indicated in the column heading. All outcome variables are scaled by the four-quarter lag of total assets, except for average interest rate and average tax rate. Standard errors (in parentheses) are clustered at the firm level; Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

Figure 2.E.3: Event study: Inventory and Capital Expenses, Financial sanctions against XPCC

(a) Inventory



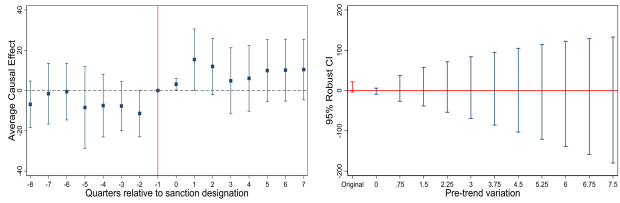
(b) Capital Expenditure



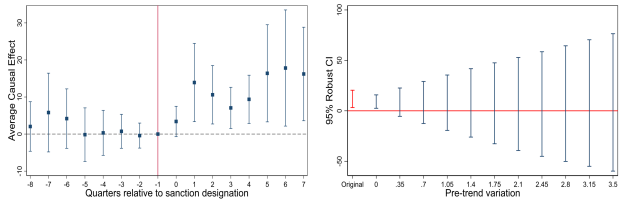
Notes: Each marker is a coefficient estimate on an interaction term between sixteen quarter dummies and the target portfolio dummy, with the vertical segments the corresponding 95 percent confidence intervals. The red vertical line is placed in 2020Q3, the first quarter of the treatment period. All point estimates are normalized relative to the omitted period, 2020Q2. The robust confidence intervals, on the right, are estimated based on Rambachan and Roth (2023).

Figure 2.E.4: Event study: Shielding, Financial sanctions against XPCC

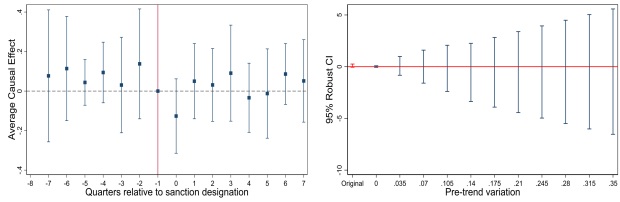
(a) Total Borrowing



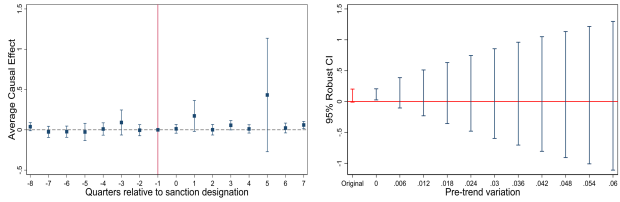
(b) Short-term Borrowing



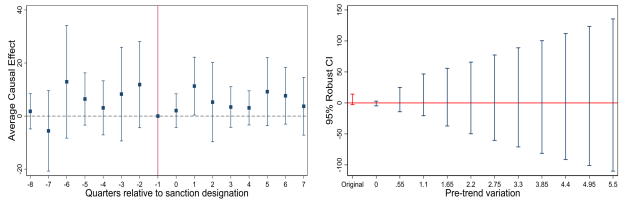
(c) Average Interest Rate



(d) Subsidy



(e) Average Tax Rate



Notes: Each marker is a coefficient estimate on an interaction term between sixteen quarter dummies and the target portfolio dummy, with the vertical segments the corresponding 95 percent confidence intervals. The red vertical line is placed in 2020Q3, the first quarter of the treatment period. All point estimates are normalized relative to the omitted period, 2020Q2. Robust confidence intervals, on the right, are estimated based on Rambachan and Roth (2023).

Table 2.E.8: Financial performance: Shielding on XPCC-subsidiaries, Covid pandemic

(a) Exclude Major Pandemic Episodes	Inventory	Capital Expenses	Total borrowing	Short-term borrowing	Avg Interest Rate	Subsidy	Avg. Tax Rate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Target × *Post*	3.215**	-0.738**	14.079*	7.097***	-0.053	0.086*	0.470
	(1.581)	(0.323)	(7.898)	(2.295)	(0.059)	(0.046)	(2.659)
Observations	7,286	7,014	4,134	6,014	5,745	6,851	5,123
Firm FE	✓	✓	✓	✓	✓	✓	✓
Sector-Quarter FE	✓	✓	✓	✓	✓	✓	✓
Mean of dependent variable	13.96	1.991	29.05	15.21	0.566	0.142	18.97
Number of firms	569	568	412	520	538	566	534
Number sanctioned	13	13	9	11	11	13	12
(b) Covid Placebo	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Target × *Covid*	0.325	-0.565	4.328	-1.680	-0.009	-0.008	0.141
	(1.584)	(0.348)	(4.213)	(2.570)	(0.044)	(0.021)	(5.239)
Observations	4,495	4,305	2,434	3,679	3,298	4,111	3,179
Firm FE	✓	✓	✓	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓	✓	✓	✓
Mean of dependent variable	14.67	1.795	31.60	16.44	0.595	0.145	19.37
Number of firms	568	566	356	504	515	554	513
Number sanctioned	13	12	7	11	11	12	10

Notes: The sample period covers sixteen quarters between 2018Q3 and 2022Q2. The *Post* indicator switches to one in 2020Q3 when financial (GloMag) sanctions were imposed on 13 XPCC-affiliated companies, the target portfolio, which are all SOEs. The control portfolio consists solely of non-sanctioned companies operating in the same sectors as those in the target portfolio. The dependent variables are indicated in the column heading. All outcome variables are scaled by the four-quarter lag of total assets, except for average interest rate and average tax rate. Standard errors (in parentheses) are clustered at the firm level; Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

Table 2.E.9: Shielding: Effects of US sanctions on politically connected firms, alternative specifications

	Total borrowing	Short-term borrowing	Avg Interest Rate	Subsidy	Avg. Tax Rate
(a) Additional SOE Interaction Term	(1)	(2)	(3)	(4)	(5)
Target × *Post*	-3.292	-5.824***	-0.012	-0.006	-2.897
	(2.706)	(1.769)	(0.059)	(0.062)	(2.658)
Connection × *Target* × *Post*	5.294***	5.348***	0.001	0.017	-0.229
	(1.840)	(1.648)	(0.062)	(0.041)	(1.702)
SOE × *Target* × *Post*	3.725*	4.636***	-0.010	0.040	2.999
	(2.201)	(1.740)	(0.040)	(0.057)	(2.170)
Observations	12,205	17,804	17,290	20,333	13,458
Firm FE	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	22.46	13.03	0.509	0.189	18.65
Number of firms	1,004	1,260	1,297	1,345	1,240
Number sanctioned	38	46	50	51	49
(b) Sample without XPCC-affiliated firms	(6)	(7)	(8)	(9)	(10)
Target × *Post*	-1.179	-2.663	-0.019	0.021	-0.468
	(2.858)	(1.899)	(0.059)	(0.048)	(1.611)
Connection × *Target* × *Post*	3.395	3.155*	0.008	-0.012	-0.963
	(2.315)	(1.885)	(0.061)	(0.041)	(1.847)
Observations	12,083	17,631	17,137	20,132	13,333
Firm FE	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	22.35	12.99	0.508	0.189	18.62
Number of firms	994	1,248	1,285	1,332	1,228
Number sanctioned	28	35	38	38	37

Notes: The sample period covers sixteen quarters between 2018Q3 and 2022Q2. The target portfolio includes firms from all types of sanction programs. The *Post* indicator switches to one in the first year sanctions are imposed on a firm. The control portfolio consists solely of non-sanctioned companies operating in the same sectors as those in the target portfolio. The dependent variable are indicated in the column heading. Political connections are defined based on the Chinese Listed Firms Personnel Database (Wang, 2020). Standard errors (in parentheses) are clustered at the firm level; Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

Chapter 3

Tracing Sanctions through the Value Chain: Evidence from the US Cotton Ban[1]

We investigate to what extent and in what way the US Cotton Ban on products originating from Xinjiang province affected Chinese exports. We find that direct exports of cotton and cotton products to the United States fell by 19% at the intensive margin and 4% at the extensive margin. While the import reduction is strongest for Chinese provinces sourcing most actively from Xinjiang, reductions in US imports from third countries correlate only very weakly with the extent to which countries source cotton inputs from China. The European Union did not impose sanctions, but its cotton-related imports from China declined significantly as well. Our analysis demonstrates that this negative spillover effect is not due to lower prices, nor to EU countries indirectly facing the ban as they want to maintain access to the US market. The most likely reason for the import decline is European firms' concerns about reputational damage associated with using cotton products sourced from China.

[1]This chapter is co-authored with Jean-François Maystadt and Johannes Van Biesebroeck. A working paper version of this chapter is available as a CEPR Discussion Paper under the same title of "Tracing Sanctions through the Value Chain: Evidence from the US Cotton Ban" (Maystadt et al., 2026) available at https://cepr.org/publications/dp21438.

3.1 Introduction

Economic sanctions have become an important tool of foreign policy. They aim to change contested policies of other nations by imposing economic costs on them, while steering clear of military intervention.[2] The use of sanctions has tripled since the 1990s (2023), including humanitarian sanctions aimed at curbing human rights violations and corruption. Sanctions have become increasingly targeted to minimize the repercussions for the population at large. Conventional economic sanctions are imposed on an entire nation, while targeted sanctions are imposed on specific individuals, businesses or organizations that are considered most responsible for the contested policies.

One example of such targeted, humanitarian sanctions is the import ban imposed by the United States in December 2020 on cotton originating from Xinjiang province. This is the largest cotton growing area in China, accounting for approximately 20 percent of global cotton production. In response to allegations of widespread repression of the Uyghur minority and the use of forced labor, the United States banned imports of all cotton and products containing cotton sourced from Xinjiang.[3] To further protest the use of forced labor in the region, it subsequently enacted the Uyghur Forced Labor Prevention Act (UFLPA) which broadened the ban to all products produced in Xinjiang, subject to a rebuttal procedure.

Despite the possibly wide-ranging implications of the US Cotton Ban, there is no evidence yet on its effectiveness. On the one hand, it is challenging for customs authorities to enforce such a region-specific policy in an industry that operates with a globally fragmented production structure (Gereffi, 1999). Its implementation requires detailed information on how the sanctioned products are embedded in local and global value chains. Hence, our first research question is whether it is possible to enforce the ban? On the other hand, the same interconnection makes it likely that downstream producers and countries operating in the same value chain are indirectly affected. It may make the sanctions less targeted, but it may also amplify their effects as it indirectly ensnares countries in between cotton-producers in Xinjiang and final consumers in the United States in the ban. The second research question then asks who is affected by the ban?

[2]Notable instances include the multi-country sanctions against Iran's nuclear program (2022) and Russia's invasion of Ukraine (Ahn and Ludema, 2020; Bělín and Hanousek, 2021; Crozet and Hinz, 2020).

[3]The region-wide ban covers all products that contain cotton grown in Xinjiang—including textiles, garments and apparel—irrespective of the location of final production, within or outside of China.

We focus on the impact of two key policy interventions that constitute the US Cotton Ban on Chinese exports: the withhold release order targeting Xinjiang-origin cotton implemented in December 2020 and the Uyghur Forced Labor Prevention Act (UFLPA) that was enacted in December 2021 and took effect in June 2022. The analysis uses product-level monthly trade flow between country pairs from UN Comtrade and similar trade flows between Chinese provinces and all country destinations from the General Administration of Customs of China. Affected cotton products are identified at the harmonized system (HS) 6-digit level of detail and control products are non-cotton textile and apparel products in the same HS 2-digit categories.

The baseline model estimates the average effect of the US sanctions on Chinese exports using a triple-differences strategy with a rich set of interactive fixed effects. It compares the evolution of Chinese exports to the United States relative to a product-destination specific baseline, controlling for product-month and destination-month interaction fixed effects that flexibly capture supply and demand shocks. The baseline estimates indicate that the Cotton Ban had a statistically and economically significant negative effect on Chinese exports of cotton-containing products to the United States. Specifically, we find a 19% decline in export value at the intensive margin and a 4% reduction in the likelihood that a product is exported (3 percentage points extensive margin effect).

These results beg the question whether this decline is large or small. It far exceeds the direct flow of goods from Xinjiang to the United States prior to the ban, but the reduction falls far short of a complete ban on imports from other Chinese provinces. Using trade information and the Chinese Input-Output table both at the provincial level, we investigate whether enforcement is aligned with input linkages between Chinese provinces and Xinjiang. We find that the export reductions line up well with the extent to which the textile sectors in various Chinese provinces source agricultural and textile inputs from Xinjiang.[4] Direct exports from Xinjiang are effectively curtailed (-82%) and the probability of direct exports falls to near zero. By far the largest other decline is in provinces in the top quartile, with a smaller, but still significant decline for provinces in the third quartile. However, this pattern does not replicate when we perform the same analysis on US imports from other countries. Depending on the specification used, there is only a weak relationship or none at all between a country's decline in US exports and the intensity of its cotton and cotton products sourcing from China. US customs seems to be able to

[4]We use predetermined input-output linkages to classify all provinces in four quartiles.

trace the cotton's origin through the Chinese value chain, but unable or unwilling to trace Chinese-sourced cotton in value chains that span third countries.

We also find indirect effects of the US Cotton Ban on exports to other destinations. Given that cotton production in Xinjiang remained stable, producers needed to find other markets.[5] However, the value of exports from China to the EU also declined (-9%), while effects at the extensive margin are negative for several countries with the magnitudes broadly in line with countries' political alignment with the United States. We investigate several possible explanations for the EU's import decline in spite of not imposing sanctions on China itself. This negative spillover effect is not due to greater buying power that could have reduced prices, nor to EU countries with a US-oriented textile sector self-imposing a ban to maintain export access to the US market. We find that the import decline is most pronounced in EU countries with the greatest human rights awareness, suggesting that the most likely explanation is European firms' concerns about reputational damage if they were to sell cotton products sourced from China. The fact that the EU import decline is largest for finished apparel products, where importers deal directly with final consumers, supports this interpretation. This pattern contrasts with the US import decline being most pronounced for raw cotton products, where the Xinjiang origin is easiest the establish.

The first literature we contribute to studies targeted sanctions. Draca et al. (2022) evaluate the effectiveness of sanctions imposed on Iran's political elite during the negotiations over its nuclear program. They find a positive response to news of negotiation breakthroughs on the returns of targeted entities on the Tehran Stock Exchange, indicative of economic incentives for the local elites to negotiate a deal in exchange for sanctions relief. A few studies look at the impact of US and EU sanctions on Russian firms following the annexation of Crimea in 2014. Ahn and Ludema (2020) find that they harmed the financial performance of targeted Russian firms, but also that strategic companies were less affected. In contrast, Nigmatulina (2023) finds no systematic negative financial effects on targeted companies, but provides evidence of substantial shielding of state-owned firms by the Russian government. Maystadt et al. (2024) study financial sanctions imposed by the US government on the Xinjiang Production and Construction Cooperation. In contrast with these studies which study sanctions targeting specific firms, we study

[5]Statistics from China's National Bureau of Statistics and the International Cotton Advisory Committee indicate that the Xinjiang share of global cotton production remained stable after 2020 at approximately 21% (Figure 3.1).

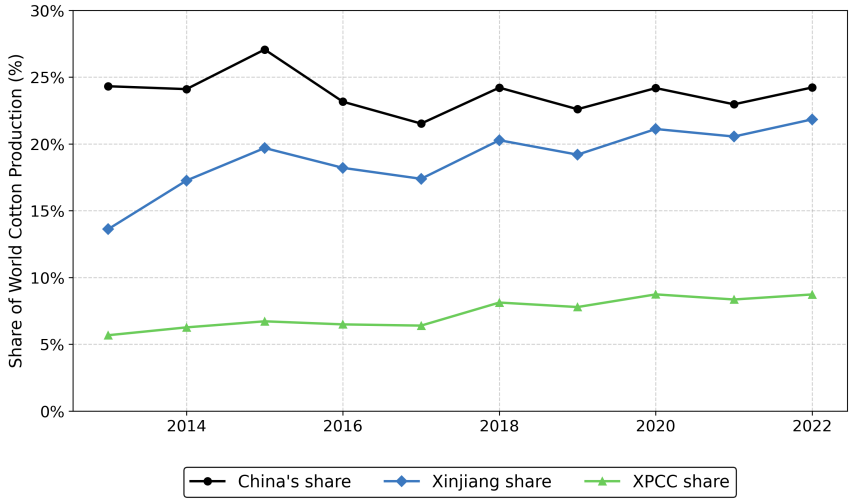
sanctions that target specific products and trace the effects through the value chain.

A second related literature studies the relationship between sanctions and trade performance. Not surprisingly, bilateral trade declines when one country imposes sanctions on another. This has been shown at the national level (e.g., Gutmann et al., 2023), firm level (e.g., Haidar, 2017), and product level (e.g., Bělín and Hanousek, 2021; Miromanova, 2023). More broadly, we contribute to the body of work exploring the connection between political conflicts and trade. Conflict with China has been very prominent and negative effects on trade have been associated with the US-China trade war (Fajgelbaum and Khandelwal, 2022), consumer boycotts (Heilmann, 2016), political conflict (Li et al., 2021), or diplomatic rows over visits by the Dalai Lama (Fuchs and Klann, 2013). Our contribution is to take global value chain (GVC) connections explicitly into account. Makioka and Zhang (2024) is one of the few studies that considers the impact of trade conflict on sourcing, which is quite natural given their focus on export restrictions. We show that GVC considerations make it more challenging to enforce sanctions, but also lead to indirect effects on third countries.

The indirect effects that we document for the EU in particular contribute to an emerging literature on political consumerism that spans economics, marketing, and political science. The existing evidence indicates that consumers are more likely to purchase from sellers who share a common political affiliation or a common position on controversial social issues (Conway and Boxell, 2024; Liaukonytė et al., 2023; McConnell et al., 2018; Schoenmueller et al., 2023). One manifestation of such preferences are consumer boycotts, but the evidence on their trade effects remains mixed. Koenig and Poncet (2022) document that French textile firms reduced sourcing from Bangladesh after the Rana Plaza collapse due to concerns over reputational damage. Our results suggest that EU firms consider the reputational risk of sourcing cotton products from Xinjiang to be sufficiently high to reduce their imports from China even in the absence of any formal EU sanctions.

The remainder of the paper is organized as follows. Section 3.2 provides background information on the implementation of the US Cotton Ban and Section 3.3 describes the data. Next, we discusses the direct effects, i.e., the impact of the ban on the export of Chinese cotton products to the United States, in Section 3.4 and the indirect effects on other countries in Section 3.5. Section 3.6 concludes.

Figure 3.1: China's share in world cotton production



Source: Compiled by authors based on information from the National Bureau of Statistics of China and International Cotton Advisory Committee.

3.2 Background

3.2.1 Cotton production in Xinjiang

Benefiting from favorable climate and policy support from the Chinese government, Xinjiang has become the main cotton production base in China (Gale and Davis, 2022).[6] Its output expanded significantly between 2010 and 2019, from 2.5 to 5 million tons per year. Its share of total Chinese production also doubled, growing from 42% to 85%, and Xinjiang alone accounted for one-fifth of global cotton production which totaled 25 million tons in 2019 (2023). Total cotton production in Xinjiang was valued at 78.8 billion RMB, or approximately 6% of Xinjiang GDP.[7] The Xinjiang Production and Construction Corps (XPCC), a paramilitary organization known for its extensive use of mechanized farming techniques, is the region's largest cotton producer (Carlson and Weaver, 2022).[8]

[6] Government support includes production subsidies for cotton cultivation and transport subsidies for textile manufacturers in other regions that source cotton from Xinjiang (US Department of Agriculture, 2025).

[7] This value is obtained by multiplying the quantity of production by the average cotton price in China.

[8] In 2019, the XPCC accounted for 41% of Xinjiang's cotton production and 34% of the total for China.

Upland cotton, which is suitable for textile and garment production, is the dominant variety cultivated in Xinjiang. Long-staple cotton, which is regarded as higher quality due to its longer fibers and used for fine fabrics, is expanding but represents only a small fraction of output. Most of the raw cotton produced in Xinjiang is processed into textiles and garments in manufacturing hubs such as Jiangsu, Zhejiang, and Shandong, while only a small share is exported in raw form (Carlson and Weaver, 2022). Through the export of Chinese cotton-related products, Xinjiang cotton is closely integrated into global supply chains.

In 2019, China's exports of cotton-related products reached a total value of 40.1 billion USD, which represented 25% of global exports. Fabrics and finished consumer goods account for 97.6% of this, and it is estimated that approximately half of all exports contain cotton originating from Xinjiang (Carlson and Weaver, 2022).[9] These products are generally not exported directly from Xinjiang, but leave China from other provinces and are sometimes re-exported via third countries before arriving at their final destinations (Lehr and Bechrakis, 2019). Complex and opaque supply chain links make it difficult for custom officials, regulators and stakeholders to trace the origin of cotton products.

3.2.2 Forced labor in Xinjiang

Since 2017, the Xinjiang government has faced accusations of repressing Uyghurs and other ethnic minorities in the region. The allegations include the establishment of detention camps and the extensive use of surveillance against minorities. In particular, the production of cotton and cotton products in Xinjiang has long been subject to international suspicion of using forced labor from ethnic minority groups, particularly the Uyghurs. These allegations have been documented extensively in media reports,[10] by non-governmental organizations (e.g., C4ADS, 2021; Lehr, 2020), and by academic researchers (e.g., Murphy et al., 2022; Zenz, 2023). The US government (Carlson and Weaver, 2022) and the United Nations (Office of the United Nations High Commissioner for Human Rights, 2022) have published the results of their investigations in official reports. The Chinese government has

[9] Trade statistics are based on the UN Comtrade database and the Xinjiang share is estimated for 2021.

[10] The *New York Times* in particular has actively reported on this issue, going back to December 16, 2018 "China's Detention Camps for Muslims Turn to Forced Labor", with major investigative journalism pieces published, for example, on April 8, 2022 "Supply Chains Widely Tainted by Forced Labor in China, Panel Is Told" and as recent as May 29, 2025 "How China Uses Work to Reshape Uyghur Identity and Control a Strategic Region".

protested the legality of the UN investigation and it has consistently denied all allegations, claiming specifically that no instances of coerced labor exist in cotton production or for any other goods produced in Xinjiang province.[11]

Despite objections from the Chinese government, concerns over forced labor practices in Xinjiang have continued to gain traction in Western countries. Human rights organizations have advocated boycotts of products linked to Xinjiang cotton. They have criticized the sourcing practices of global apparel brands and retailers,[12] and have called for formal sanctions against Xinjiang cotton.[13]

3.2.3 The US Cotton Ban

Despite repeated denials by the Chinese government, the United States imposed financial, investment and trade sanctions against Chinese entities and senior Chinese officials for human rights abuses in Xinjiang. Maystadt et al. (2024) evaluate the effects of various firm-level sanctions, but here we focus only on the trade effects of the region-wide Cotton Ban.

In September 2019, the US Customs and Border Protection (CBP) issued nine withhold release orders (WROs) to prevent importation of goods produced by Chinese entities which it considered associated with forced labor in Xinjiang. These initial actions focused on specific companies rather than imposing a broad ban.[14] Additionally, on July 1, 2020, four US government departments—State, Treasury, Commerce, and Homeland Security—jointly issued the Xinjiang Supply Chain Business Advisory, warning US firms of forced labor in the Xinjiang cotton sector and urging them to conduct enhanced due diligence.

After further negotiations with stakeholders, the United States implemented broader import sanctions targeting all cotton and cotton products originating in

[11] For instance, Yilijiang Anayiti, the spokesperson of the Xinjiang government, asserted during a press conference in Beijing on April 9, 2021 that there were no instances of forced labor in Xinjiang, and that the employment of ethnic minorities was entirely voluntary. Press release available at http://de.china-embassy.gov.cn/zt/zgxj/202104/t20210415_9045932.htm. The Chinese government also released an official rebuttal to the Office of the United Nations High Commissioner for Human Rights (2022) report available online at https://www.ohchr.org/sites/default/files/documents/countries/2022-08-31/ANNEX_A.pdf.

[12] For example, in July 2020, more than 180 organizations urged apparel brands to cease sourcing cotton from Xinjiang. Press release available at https://enduyghurforcedlabour.org/news/402-2/.

[13] For example, Human Rights Watch (2021) calls for new legislation to prohibit all imports linked to forced labor.

[14] The CBP already considered implementing a region-wide import ban in September 2020, but this proposal was opposed by the Trump administration. Instead, the CBP issued six additional WROs targeting specific entities. Reuters report is available at https://www.reuters.com/world/asia-pacific/us-pulls-back-broad-import-bans-cotton-tomatoes-chinas-xinjiang-2020-09-15/.

Xinjiang. Figure 3.2 outlines the key legislative actions in the implementation of the US Cotton Ban. The first broad-based measure was announced on December 2, 2020 when the CBP issued a WRO prohibiting the importation of all cotton and tomato products produced by the XPCC or any of the affiliated entities of its sprawling network.[15] This order authorizes the CBP to detain imports of intermediates and final goods such as textiles, garments, and apparel at US ports of entry if there is reasonable evidence that the goods were produced, in whole or in part, with XPCC-origin cotton. Detained shipments can only be released if the importer provides proof that the products were not made under such conditions. Subsequently, on January 13, 2021, the CBP issued a second, even more expansive region-wide WRO, extending the ban to all cotton and tomato products produced in Xinjiang, regardless of the producer.[16]

To further strengthen efforts against the use of forced labor in the region, one year later the United States escalated its trade restrictions by enacting the Uyghur Forced Labor Prevention Act (UFLPA). The Act was signed into law by President Biden on December 23, 2021 and took effect, with immediate enforcement, on June 21, 2022. The UFLPA differs from the WRO regime in two key respects.

First, its enforcement framework is more stringent, requiring that importers provide "clear and convincing evidence" to CBP within a 30-day period to substantiate that shipments are free from forced labor. The evidentiary requirement is significantly more demanding than the documentation standards under the WRO. Required documentation includes a detailed supply chain map delineating all involved entities, information on workforce practices at each stage (including recruitment processes, wage payments, and per-worker production output), and "credible audits" evaluating forced labor risks and remediation measures (US Customs and Border Protection, 2022).

Furthermore, the Act mandates the Forced Labor Enforcement Task Force(FLETF) to develop and implement a strategy to strengthen enforcement (US Department of Homeland Security, 2022). This strategy includes: (i) conducting a detailed assessment of the scope of imports associated with forced labor; (ii) establishing an official UFLPA Entity List and identifying high-priority sectors for enforcement; (iii) strengthening the capacity to trace and identify goods linked to forced labor;

[15]The effective date of this order was backdated to November 30, 2020. Press release available at https://www.dhs.gov/news/2020/12/02/dhs-issues-detention-order-cotton-products-made-xinjiang-production-and-construction.

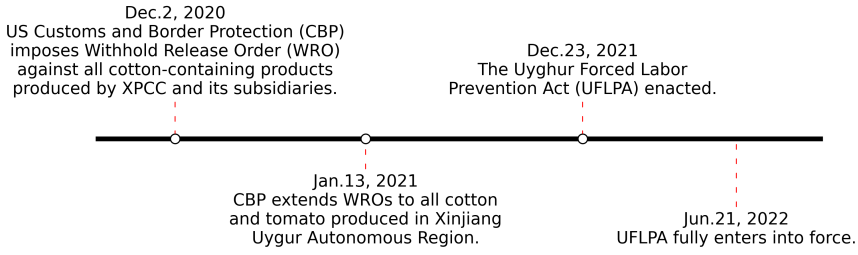
[16]Press release available at https://www.cbp.gov/newsroom/national-media-release/cbp-issues-region-wide-withhold-release-order-products-made-slave.

(iv) enhancing the legal authorities and enforcement tools at US ports of entry; (v) issuing guidance for importers to ensure compliance; and (vi) fostering interagency coordination and consultation.

Second, the coverage of the UFLPA is substantially broader. It establishes a rebuttable presumption that all goods produced wholly or partially in Xinjiang, or by entities on the UFLPA Entity List, are made with forced labor. As a result, all such shipments—extending beyond cotton and tomato products—need in principle be detained at US borders and denied entry.

Of course, enforcing such a broad ban is extremely difficult in a world where production of many goods has fragmented geographically and final goods imported from anywhere can contain inputs sourced from Xinjiang. The FLETF has designated the cotton, tomato, and poly-silicon industries as high priority sectors with dedicated enforcement plans. We therefore examine the impact of trade sanctions on products containing Xinjiang-origin cotton. We define a fixed set of cotton-related products as the targeted commodities for the entire sample period (as well as a fixed set of control products), despite the broader scope of the UFLPA. In the post-UFLPA period, we are implicitly evaluating the effect of the FLETF priority strategy. To assess the robustness of our findings, we implement alternative definitions of both targeted and control product groups in supplementary analyses.

Figure 3.2: Timeline for the imposition of the US Cotton Ban



Source: Information from the US Customs and Border Protection (2023), accessible at https://www.cbp.gov/trade/forced-labor/withhold-release-orders-and-findings.

3.3 Data

3.3.1 National trade data from UN Comtrade

For the country-level analysis, we rely on trade flows from the United Nations Comtrade database that are defined at the bilateral country level by product and month. The sample period always runs from January 2019 to December 2022. Where possible, we measure trade flows from the import side which tend to be recorded more accurately than exports.

The sample includes all products defined at the 6-digit level of the Harmonized System (HS) commodity classification that are textiles or textile products. We define them as the 801 products contained in the HS 2-digit categories from 50 to 65 (excluding 64). Following Carlson and Weaver (2022), we identify 238 cotton-related products that are sanctioned; the remaining 563 products are the controls. They include raw cotton products (5 sanctioned out of a total of 29), yarn products (49 of 150), fabrics (126 of 336), apparel products (47 of 211), and other textile products (11 of 78).[17]

In the benchmark analysis, the sample includes Chinese exports to 126 destination countries with positive imports of at least one of the sample commodities in 2019. EU member states are included as separate countries. Hong Kong and Macau are excluded as they are Special Administrative Regions of China. When we investigate the heterogeneity of effects across countries that differ in their sourcing of cotton inputs from China, we use two different samples. First, we combine exports from China with twenty additional origin countries into a four-dimensional panel. Second, we use information on US imports from 186 countries with positive textile and apparel trade, for the same period and for a subset of 798 products showing positive US imports. Appendix 3.D contains additional details.

The final sample for the benchmark analysis is a balanced panel with 4,844,448 observations in total, comprising 801 products exported to 126 destinations over a 48-month period.[18] Summary statistics for the targeted and control commodities during a 12-month period in 2019 (prior to the Cotton Ban) are presented

[17] We use the correspondence tables of the UN Statistics Division to make the product classification consistent over time. Table 3.A.1 in the Appendix shows the breakdown of treated and control products by 2-digit category. Three control products are included in both the yarn and fabrics groups.

[18] Twenty-eight destination countries stopped reporting trade flow data in the last years of the sample period. Rather than assigning no trade in these instances, we rely on export data reported by China. Treating these instances as missing data produces similar results (available upon request).

in Table 3.1, separately for the five product groups. Three quarters of the sanctioned product-month observations (2,141 of 2,856) show positive US imports and their average (monthly) value was 3.5 million USD. Positive imports in the EU and other control destinations—which tend to be much smaller markets than the United States—are less likely (37%) and on average of much lower value.

Table 3.1: Summary statistics on Chinese exports of cotton and control products (monthly, 2019)

	(1) US cotton imports		(2) EU cotton imports		(3) Control imports	
	obs.	mean	obs.	mean	obs.	mean
Panel A: ALL						
Exports (USD 1,000)	2,856	3536	74,256	128	951,588	116
Export dummy	2,856	0.75	74,256	0.37	951,588	0.37
log(Exports)	2,141	12.03	27,669	9.62	356,499	9.45
Panel B: Raw cotton						
Exports (USD 1,000)	60	1	1,560	1	34,452	9
Export dummy	60	0.18	1,560	0.15	34,452	0.05
log(Exports)	11	7.61	238	6.14	1,855	8.11
Panel C: Yarn						
Exports (USD 1,000)	588	12	15,288	5	178,200	48
Export dummy	588	0.25	15,288	0.08	178,200	0.21
log(Exports)	145	9.32	1,243	8.02	37,179	9.37
Panel D: Fabric						
Exports (USD 1,000)	1,512	237	39,312	15	399,168	91
Export dummy	1,512	0.86	39,312	0.28	399,168	0.30
log(Exports)	1,293	10.64	11,186	8.71	118,423	9.74
Panel E: Apparel						
Exports (USD 1,000)	564	15010	14,664	574	250,668	218
Export dummy	564	0.99	14,664	0.84	250,668	0.59
log(Exports)	560	15.28	12,276	10.64	147,694	9.36
Panel F: Other textile						
Exports (USD 1,000)	132	9597	3,432	116	92,664	113
Export dummy	132	1.00	3,432	0.79	92,664	0.57
log(Exports)	132	15.14	2,726	9.84	52,637	9.14

Source: UN Comtrade
Notes: The number of HS 6-digit products included in each panel are: ALL (238), Raw (5), Yarn (49), Fabric (126), Apparel (47), Other textile (11). The EU encompasses all 26 member states, with the exception of Austria, which does not provide monthly statistics. We adapt the method of Carlson and Weaver (2022) to categorize the sample commodities into five different groups. Column (3) with "Control imports" includes: (i) all commodities imported by destinations other than the United States and the EU; (ii) non-cotton containing commodities imported by the United States and the EU.

3.3.2 Provincial trade data from China Customs

Not all cotton-related exports from China were subject to sanctions, only those using Xinjiang-origin cotton. To investigate the impact of the US Cotton Ban specifically on exports from Xinjiang and from Chinese provinces that source most intensively from Xinjiang, we use information on trade flows at the provincial level. These data are collected by twenty-nine regional customs offices and reported to China's General Administration of Customs. Consistent with the UN Comtrade data, we focus on export flows between January 2019 and December 2022, to the same 126 destination countries, and of the same 796 products (for five products we observe no records in the China Customs dataset).

The resulting four-dimensional balanced panel comprises 139,612,032 product-province-destination-month observations. 5,011,355 observations, approximately 3% of the sample, show positive exports. Table 3.2 contains summary statistics in 2019 for all provinces (Panel A), for Xinjiang (Panel B), and for the top quartile of provinces according to their sourcing from Xinjiang (Panel C). The average (monthly) export value across all Chinese provinces of to-be-sanctioned products to the United States was 136,000 USD, to EU members states 4,000 USD, and for other control observations (products and/or destinations) 6,000 USD. Direct exports from Xinjiang to the United States or the EU were strikingly low, while exports from Chinese provinces that rely most on Xinjiang cotton inputs were much higher than average. A similar pattern is observed for the probability of exporting to-be-sanctioned products. The average export probability to the United States for all Chinese provinces was 16%, but only 1% for Xinjiang and 31% for top-quartile provinces.

3.3.3 Other datasets

Interprovincial Input-Output table in China. We use the Chinese interprovincial Input-Output table to identify Chinese provinces that most intensively source cotton from Xinjiang. It is compiled by Chen et al. (2023) using a trade matrix derived from Chinese VAT invoices to calibrate the multi-regional Input-Output table provided by the Development Research Center of the State Council. The resulting table provides an overview of the interprovincial supply chain linkages among 42 industrial sectors in China in 2017. In Appendix 3.B, we detail how we rank all provinces according to their intensity of cotton-related input sourcing in their textile-related sectors. This provides an approximation to the extent of

Table 3.2: Summary statistics on Chinese provincial exports of cotton and control products (monthly, 2019)

	(1) US cotton imports		(2) EU cotton imports		(3) Control imports	
	obs.	mean	obs.	mean	obs.	mean
Panel A: ALL						
Exports (USD 1,000)	82,476	136	2,144,376	4	32,676,156	6
Export dummy	82,476	0.16	2,144,376	0.03	32,676,156	0.03
log(Exports)	13,422	10.49	69,120	9.41	1,102,745	9.45
Panel B: Exports from Xinjiang						
Exports (USD 1,000)	2,844	2	73,944	0.2	1,126,764	5
Export dummy	2,844	0.01	73,944	0.00	1,126,764	0.01
log(Exports)	31	10.61	183	9.48	7,642	10.90
Panel C: Exports from Chinese provinces in 4th Quartile of Xinjiang sourcing						
Exports (USD 1,000)	19,908	318	517,608	10	7,887,348	14
Export dummy	19,908	0.31	517,608	0.07	7,887,348	0.07
log(Exports)	6,133	10.78	36,880	9.43	573,840	9.53

Source: China's General Administration of Customs, Provincial trade data
Notes: The EU encompasses all 26 member states, excluding Austria for consistency with the UN Comtrade data. Column (3) with "Control imports" includes: (i) all commodities imported by destinations other than the United States and the EU; (ii) non-cotton containing commodities imported by the United States and the EU. We use the 2017 information from the interprovincial input–output table of Chen et al. (2023) to quantify the supply-chain linkages between Xinjiang and other Chinese provinces (excluding Tibet and Beijing). Chinese provinces (other than Xinjiang) are categorized into four groups based on the total value of agricultural and textile inputs in their local textile industry that is sourced from Xinjiang. Panel C shows summary statistics for the 4th Quartile, i.e. provinces above the 75th percentile.

exposure to the US Cotton Ban for each province.

Google Trends data. We use keyword search intensity data from Google Trends to rank EU countries according to their level of interest in or awareness of human rights issues. This is accomplished by measuring the relative online search intensity for a set of "topics" (keywords) that are related to historical human rights crises from the 2010s. In Appendix 3.C, we provide further details about the methodology and the country ranking generated by the aggregate index.

3.4 Direct effects on US imports

3.4.1 Baseline specification

We estimate the direct impact of the US Cotton Ban on the exports of Chinese cotton products using the triple-differencing strategy of Frazer and Van Biesebroeck

(2010):

$$Y_{pdt} = \beta\, Ban^{US}_{pdt} + \gamma_{pd} + \gamma_{pt} + \gamma_{dt} + \epsilon_{pdt}. \tag{3.1}$$

The units of observation are product-destination-months (pdt) and the specification includes three sets of interaction fixed effects. It focuses identification on changes over time within the cross-sectional product-destination pairs, while controlling for product-month fixed effects to capture supply shocks and destination-month fixed effects as controls for demand shocks.[19] The indicator Ban^{US} takes on the value of one for US imports of cotton-related products from December 2020 onward. The coefficient β will turn negative if Chinese exports of cotton products to the United States decline after the US Cotton Ban is enacted relative to the trend for non-cotton products exported from China to the United States, further normalizing the cotton versus non-cotton trend for US exports by the same relative trend in export growth to other, non-sanctioned countries. Standard errors are clustered at the product–destination level, consistent with the level of treatment assignment.

The dependent variable Y_{pdt} is the logarithm of export volume when we study the effects at the intensive margin, and it is a binary indicator for positive export when we study the extensive margin effects. We also show robustness checks using the Poisson pseudo maximum likelihood (PPML) estimator that accounts for zeros and heteroskedasticity in the residuals.

To identify a causal effect, US sanctions should be allocated exogenously relative to the products' potential export evolution. Unlike sanctions of Chinese high-tech products that are imposed for economic reasons, the US Cotton Ban is mainly motivated by humanitarian and political considerations and can be considered exogenous (Ahn and Ludema, 2020; Nigmatulina, 2023). The comprehensive set of fixed effects capture unobserved heterogeneity that may be correlated with the allocation of sanctions.

A valid diff-in-diffs estimator requires comparable pre-sanction trends between the target and control product-destination pairs for both outcome variables (Olden and Møen, 2022). We test the parallel trends assumption by estimating the following

[19] The product-month fixed effects also remove potential spillovers from the Cotton Ban on control commodities.

event study:

$$Y_{pdt} = \sum_{t=2019m1, t\neq 2020m11}^{2022m12} \beta_t \, Ban_{pdt}^{US} + \gamma_{pd} + \gamma_{pt} + \gamma_{dt} + \epsilon_{pdt}, \tag{3.2}$$

where November 2020, the final month of the pre-treatment period, is the base period with β_t set to zero.

3.4.2 Baseline results

Table 3.3 shows the estimates of the average impact of the region-wide Cotton Ban. The estimate in column (1) indicates that exports of sanctioned commodities from China to the US decline by 19% (point estimate of -0.211 in log points) relative to the trends of the control group. This effect is significant at the 1% level. Considering that the total annual export volume of all Chinese cotton-containing products to the United States in 2019 was 10 billion USD, the aggregate reduction of Chinese exports was 1.9 billion USD per year.

The estimate in column (3) shows the extensive margin effect, i.e., the extent to which cotton-related HS 6-digit products have a reduced probability of positive exports from China to the United States after the Cotton Ban is enacted. The point estimate is again negative and statistically significant at the 1% level. The probability of any export of Chinese cotton-containing products decreased by 3 percentage points, which is a 4% reduction from the average export probability of 75% in 2019.[20]

Dynamic impact. As discussed previously, the Cotton Ban gradually tightened restrictions on Chinese cotton imports. Using the same four-year sample period, we divide the 25-month treatment window into three distinct phases: (1) December 2020–November 2021 represents the first year when the ban was implemented by a WRO of the CBP; (2) December 2021–June 2022 covers most of the second year when the UFLPA was passed, but not in force yet; and (3) July 2022-December

[20] We use monthly data to exploit richer temporal variation and trace the dynamics of the US Cotton Ban. As a robustness check, we reestimate Equation 4.1 using quarterly and annual Comtrade data. At the intensive margin, the estimated coefficients on the key interaction terms remain negative and statistically significant when using both quarterly and annual data. At the extensive margin, the effect is less precisely estimated at coarser frequencies. The quarterly estimate is -0.019 and statistically significant at the 10% level, whereas the annual estimate is close to zero and statistically insignificant.

Table 3.3: Direct impact of the Cotton Ban

	(1) log(Exports)	(2) log(Exports)	(3) Export dummy	(4) Export dummy
Ban^{US}	-0.211***		-0.030***	
	(0.044)		(0.008)	
$Ban^{US} \times first$		-0.169***		-0.018**
		(0.044)		(0.008)
$Ban^{US} \times second$		-0.182***		-0.020*
		(0.059)		(0.011)
$Ban^{US} \times UFLPA$		-0.331***		-0.067***
		(0.064)		(0.013)
Observations	1,914,465	1,914,465	4,844,448	4,844,448
Product-destination FE	✓	✓	✓	✓
Product-month FE	✓	✓	✓	✓
Destination-month FE	✓	✓	✓	✓

Notes: Estimation results for equation (3.1); the units of observation are imports from China at the product-destination-month; the sample period ranges from January 2019 to December 2022. Ban^{US} is the coefficient on the triple interaction indicator taking a value of one if a commodity contains cotton and was exported from China to the United States after the implementation of the Cotton Ban in December 2020. The three interaction terms in columns (2) and (4) represent different time periods: (1) $first$ is the period between December 2020 and November 2021, the first year of the Cotton Ban; (2) $second$ is the period between December 2021 and June 2022, the second year of the Cotton Ban, when the $UFLPA$ was enacted but not yet in force; and (3) $UFLPA$ is the period from July 2022 onward when the UFLPA is in effect. Standard errors in parentheses are clustered at the product-destination level, with ***, **, and * representing significance at the 1%, 5%, and 10% level.

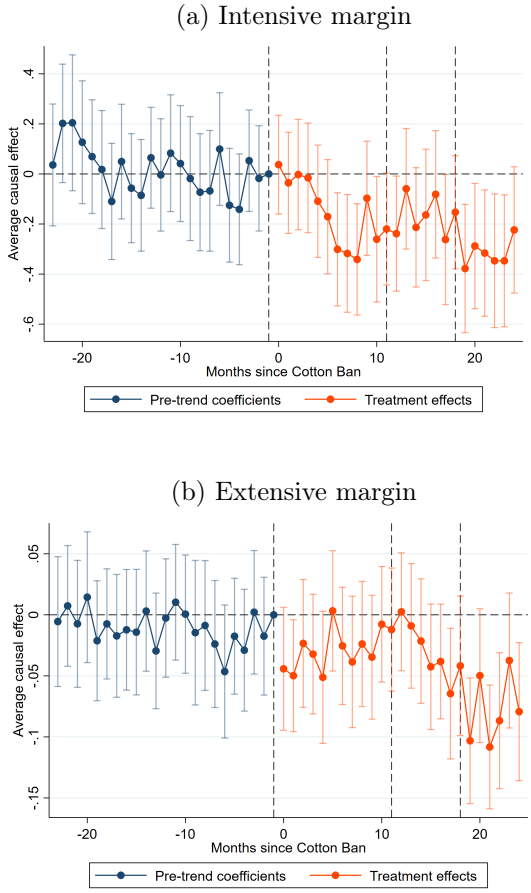
2022 is the period following the implementation of the UFLPA. We interact the Ban^{US} indicator with separate interaction effects for these three periods.

Columns (2) and (4) in Table 3.3 show the corresponding point estimates. The magnitude of the sanction effects progressively increase over time (in absolute value). At the intensive margin, the export decline grows from -16% (-0.169) in the initial year to -28% (-0.331) under the UFLPA. The gradual increase in the negative effect on the probability of Chinese exports is even more pronounced, with the point estimate tripling from -0.02 to –0.07 over the sanction period. This pattern may be attributed to adjustments of supply chains by American importers taking some time, but also to improved enforcement and greater restrictiveness of the Cotton Ban under the UFLPA.

Parallel trends. We test the parallel trends assumption for both outcome variables. The two panels of Figure 3.3 plots all estimated coefficients for 47 month-specific interaction terms with the 95% confidence intervals. There are no statistically significant export differences between Chinese exports of cotton products to the United States and the control product-destination pairs prior to the treatment period. Tests of joint-significance of all pre-sanction coefficients can be rejected both

for the export volume and the export dummy. The reduction in export volumes and export probability during the sanction period shows up in the red markers. The three periods considered in Table 3.3 are indicated by dashed vertical lines, and the gradual increase of the export decline, in particular at the extensive margin, is clearly visible. In the final period, all but one point estimate are significantly different from zero.

Figure 3.3: Event study of the direct impact on US imports



Notes: The figure shows the estimated coefficients for 47 individual interaction terms, along with the corresponding 95% confidence intervals, obtained by estimating equation (3.2). All regressions include product-destination, product-time, and destination-time fixed effects. Standard errors are clustered at the product-destination level. The vertical dashed lines indicate November 2020 (the baseline period, -1, with values normalized at 0), November 2021, and June 2022, the three months before each of the three US Cotton Ban measures are enacted.

Robustness checks. We verify the robustness of the results using a Poisson Pseudo Maximum Likelihood estimator (PPML), which is robust to the presence of zero-value observations and to heteroskedastic errors (Silva and Tenreyro, 2006). The dependent variable is the export volume in levels and results are reported in Table 3.E.1 in the Appendix. In column (1), the average point estimate for the US Cotton Ban effect remains negative and statistically significant at the 1% level and with a value of -0.105 it lies in between the effects estimated at the intensive and extensive margins. As in the baseline, the absolute magnitude of the effect progressively increased over time.[21]

Furthermore, we verify whether results are robust to using information on trade flows from China's Customs data. Results in columns (1) and (2) of Table 3.E.2 rely on the full database that has four dimensions, as total Chinese exports for each product-destination-month observation are reported separately for each Chinese province (indexed by o for origin). Note that we do not include product-destination-month fixed effects in the regression as this would be fully collinear with the Cotton Ban indicator, Ban^{US}. The point estimates measure the average effect for all Chinese provinces and are negative and statistically significant at both the intensive and extensive margin. The absolute magnitude of the export decline is lower than in the benchmark case, but we turn to heterogeneity across Chinese provinces in the next section.

Results in columns (3) and (4) of Table 3.E.2 aggregate the Chinese Customs data to the national level which generates a dataset with the same dimensions as the UN Comtrade data used for the baseline results. The main distinction is that export transactions are now measured at the export rather than the import side. The intensive margin export decline is estimated quite a bit larger at -26%, while the extensive margin results become a lot smaller and statistically insignificant.

3.4.3 Enforcement of the US Cotton Ban through the value chain

Chinese supply chain. We already established that the Cotton Ban reduced US imports of cotton-related products from China. However, the ban did not target all Chinese products, but only those made with cotton sourced from Xinjiang. Such

[21] We implement the estimator with the Stata routine "ppmlhdfe" of Correia et al. (2020). It automatically drops observations that do not contribute to the identification, e.g., destination-product pairs that never show positive exports, which explains the lower number of observations compared to the extensive margin regressions.

targeted sanctions intend to minimize the impact on other products. The bilateral country data from UN Comtrade cannot fully capture the effectiveness of Cotton Ban enforcement, which is why we now turn to China's Customs data which contain information on Chinese exports at the province level.

We expect the export decline to be strongest for direct exports from Xinjiang and to be increasing in the extent to which other provinces' textile industry sources cotton from Xinjiang. Using the interprovincial Input–Output table of Chen et al. (2023), we classify Chinese provinces into four quartiles based on their dependence on cotton-related inputs from Xinjiang.[22] Figure 3.B.1 in the Appendix shows the relevant input values for all Chinese provinces. Major textile and garment production hubs such as Guangdong, Zhejiang, and Hebei, depend heavily on cotton-related inputs from Xinjiang and are included in the top quartile $Q4$.

We then examine the effectiveness of the Cotton Ban using the following specification:

$$\begin{aligned} Y_{podt} \quad = \quad & Ban^{US}_{pdt} \times \left[\beta^{XJ} XJ_o + \sum_{k=1}^{4} \beta^{Qk} \{Qk \text{ of XJ-sourcing}\}_o \right] \\ & + \gamma_{pod} + \gamma_{pot} + \gamma_{dt}/\gamma_{pdt} + \epsilon_{podt}. \end{aligned} \tag{3.3}$$

Observations are indexed by product p, provincial origin o, export destination d, and month t. XJ is an indicator for Xinjiang province and the terms in brackets are four quartile indicators for intensity of sourcing from Xinjiang. Specification (3.3) allows the identification of five coefficients. We can make the specification even more flexible by replacing the destination-month fixed effect with product-destination-month triple-interaction fixed effects. In that case, we can only estimate four of the five coefficients, as this is now a fully saturated model at the province (origin) level, and the point estimates measure the difference with the excluded quartile $Q1$.[23]

Table 3.4 presents the estimation results for the two variations of equation (3.3). At the intensive margin, the direct effect of the sanctions is always estimated by

[22] The procedure is described in detail in Appendix 3.B. Tibet is excluded as it is also subject to US allegations of human rights abuses; Beijing is excluded as it may confound the estimation given that it is the national capital and political center, but only a small textile production center.

[23] While econometrically feasible, we do not include province–destination–month fixed effects. In that case, the effects would be only identified from the extent to which differences in the export evolution for cotton and non-cotton products within province-destination pairs are related to each province's extent of sourcing from Xinjiang. The problem with this identification strategy is that many provinces are relatively specialized in their exports to some destinations. When they export almost exclusively either cotton or non-cotton textile products, there is no identifying variation left.

far the largest specifically for Xinjiang cotton exports, and much larger than the overall effect for China from the baseline. The point estimate of -1.105 in column (1) implies that cotton exports decline by two thirds under the Cotton Ban. The point estimate of -1.732 in column (3) implies that the export level of Xinjiang is reduced by 83% relative to exports of provinces in the first quartile.[24]

Most importantly, we also find large and statistically significant declines in exports to the United States from Chinese provinces that source most cotton from Xinjiang, i.e., those in the fourth and third quartiles. The magnitudes of all effects increase monotonically with the degree of dependence on Xinjiang's inputs, indicating that the cotton ban led to larger export reductions in provinces more reliant on Xinjiang-sourced inputs.

Estimates at the extensive margin, in columns (2) and (4), are also largest for Xinjiang, but the differences with the coefficients for other provinces are notably smaller. However, the point estimate of -0.024 is extremely large compared to the mean probability of cotton-related products exported from Xinjiang to the United States which was only 1% in 2019. It suggests that the Cotton Ban stopped virtually all direct exports from Xinjiang to the United States. The export probability declined most for other provinces that are in the third quartile—not the fourth—but differences in the baseline probabilities may explain the uneven effects.

Global supply chain. We next perform a similar analysis investigating the impact on exports from a broad sample of countries. The above results indicate that US Customs are able to deter exports systematically more from Chinese provinces that rely more strongly on Xinjiang inputs. Given that Xinjiang produces one fifth of global cotton output and that the Chinese textile industry is the largest in the world, many other countries also rely directly or indirectly on cotton that originates from Xinjiang. Are US imports also systematically reduced for them?

We investigate this question symmetrically to specification (3.3). Rather than using 29 Chinese provinces as origins, we now use export information for a sample of 21 (including China) countries from the UN Comtrade dataset. All selected countries are among the 55 most important exporters of textile products to the United States, but they are chosen to make sure they vary in the extent to which

[24]Export transactions reported by Xinjiang still indicate 154 instances of cotton-related product shipments to the United States even after the implementation of the Cotton Ban, with a total value of 13 million USD. These transactions may reflect misreporting by Chinese Customs, e.g., misclassifications of product or destination, it may be goods that are detained at the US border, or transactions that are not intercepted.

Table 3.4: Direct impact of the Cotton Ban on provincial exports

	(1)	(2)	(3)	(4)
	Destination-month FE		Product-destination-month FE	
	log(Exports)	Export Dummy	log(Exports)	Export Dummy
Ban^{US} × Xinjiang	-1.105***	-0.024***	-1.732***	-0.013***
	(0.328)	(0.003)	(0.455)	(0.004)
Ban^{US} × Q4 Xinjiang sourcing	-0.271***	-0.012***	-0.750***	-0.001
	(0.041)	(0.003)	(0.278)	(0.003)
Ban^{US} × Q3 Xinjiang sourcing	-0.113**	-0.024***	-0.684**	-0.013***
	(0.049)	(0.003)	(0.279)	(0.003)
Ban^{US} × Q2 Xinjiang sourcing	-0.037	-0.012***	-0.591**	-0.001
	(0.062)	(0.002)	(0.282)	(0.003)
Ban^{US} × Q1 Xinjiang sourcing	0.640**	-0.011***		
	(0.287)	(0.002)		
Observations	4,750,327	139,612,032	4,173,657	139,612,032
Product-province-destination FE	✓	✓	✓	✓
Product-province-month FE	✓	✓	✓	✓
Destination-month FE	✓	✓		
Product-destination-month FE			✓	✓

Notes: Estimation results for equation (3.3) with Chinese provinces as origins (exporters); the units of observation are exports of China at the product-province-destination-month; the sample period ranges from January 2019 to December 2022. Ban^{US} is the coefficient on the triple interaction indicator taking a value of one if a commodity contains cotton and was exported from any Chinese province to the United States after the implementation of the Cotton Ban in December 2020. The first interaction term is a dummy for direct exports from Xinjiang province. The next four interactions are dummies for the quartile each province is assigned to in terms of sourcing agricultural and textile inputs from Xinjiang. For the results in columns (3) and (4), the average effect is absorbed by the full set of interaction fixed effects, and $Q1$ is omitted as a reference group. Standard errors in parentheses are clustered at the product-province-destination level, with ***, **, and * representing significance at the 1%, 5%, and 10% level.

they source cotton or cotton-products from China. We detail in Appendix 3.D the sample of exporters and the construction of four sourcing quartiles using the reliance on Chinese cotton inputs of each origin country by its total volume of cotton products imported from China. Countries in the top quartile are most dependent on Chinese cotton and some of their US exports will no longer be permissible under the Cotton Ban. This group includes major textile and garment producers such as Vietnam, India, and Indonesia. The indicator Ban^{US}_{pdt} now switches to one for US imports of cotton products irrespective of the exporting country, such that the (Ban^{US}_{pdt} × China) interaction equals the original Ban^{US} indicator and plays the same role as the ($Ban^{US} \times XJ$) interaction in equation (3.3).

Table 3.5 presents the corresponding results. Two observations stand out. First, at both margins, the Cotton Ban reduces the import of Chinese cotton products. These direct sanction effects are estimated larger but comparable to the baseline estimates in Table 3.3. Second, the sanctions appear to have little impact on imports from other origin countries that rely the most on Chinese cotton inputs. The

estimated effects for countries in $Q3$ and $Q4$ are statistically insignificant at the intensive margin, whereas at the extensive margin only $Q3$ and $Q1$ show a negative, statistically significant, but small effect. The estimates in column (3) which show the export evolution for the upper 3 quartiles relative to the first quartile show the expected negative signs, but point estimates are still small, especially compared to the corresponding estimates in column (3) of Table 3.4, and not statistically significant. Overall, it suggests that the Cotton Ban does not effectively restrict imports of cotton products that are made with Chinese cotton inputs but shipped from third countries.

Results in Table 3.E.3 in the Appendix show somewhat stronger effects when the sample is expanded to 27 origin countries, which include all top-30 cotton exporters to the United States that report the necessary data. While US imports of countries in $Q4$ and $Q3$ do not fall, see column (1), they do not increase like the US imports of countries in $Q1$. As a result, the normalized effects in column (3) are negative. Extensive margin effects are negative for three of the four quartiles, but the effect is stronger in $Q3$ and $Q1$ than in $Q4$, thus not in line with the intensity of sourcing from China. Finally, in Table 3.E.4 we show results based only on US imports from all 186 origin countries in a triple-difference specification. Here the extensive margin results are in line with the sourcing intensity, but the intensive margin effects not at all.

Overall, US import reductions under the Cotton Ban seem to be much less systematically related to supply chain exposure for imports from third countries, compared to imports from Chinese provinces.

3.5 Indirect effects on third country imports

3.5.1 Specification and results

Specification. Because many countries operate in the global value chain that produces textile products for the US market, the Cotton Ban may influence Chinese exports to other countries and regions as well. On the one hand, (mostly) Chinese products with Xinjiang-sourced cotton can no longer be exported to the United States and their producers will look for alternative destinations. On the other hand, textile output of other countries can no longer contain Xinjiang-sourced inputs if they are to be exported to the United States. Producers that want to retain that option may need to look for alternative cotton input sources.

Table 3.5: Direct impact of the Cotton Ban according to countries' input sourcing from China

	(1)	(2)	(3)	(4)
	Destination-month FE		Product-destination-month FE	
	log(Exports)	Export dummy	log(Exports)	Export dummy
Ban^{US}_{pdt} × China ($\equiv Ban^{US}$)	-0.368***	-0.050***	-0.460***	-0.042***
	(0.039)	(0.007)	(0.051)	(0.008)
Ban^{US}_{pdt} × Q4 China sourcing	0.024	-0.005	-0.067	0.002
	(0.029)	(0.004)	(0.046)	(0.004)
Ban^{US}_{pdt} × Q3 China sourcing	0.053	-0.011***	-0.026	-0.004
	(0.036)	(0.004)	(0.052)	(0.004)
Ban^{US}_{pdt} × Q2 China sourcing	0.056*	-0.004	-0.031	0.003
	(0.031)	(0.003)	(0.048)	(0.004)
Ban^{US}_{pdt} × Q1 China sourcing	0.091***	-0.007**		
	(0.034)	(0.003)		
Observations	9,667,684	100,964,448	8,839,862	100,964,448
Product-origin-destination FE	✓	✓	✓	✓
Product-origin-month FE	✓	✓	✓	✓
Destination-month FE	✓	✓		
Product-destination-month FE			✓	✓

Notes: Estimation results for equation (3.3) with third countries as origins (exporters); the units of observation are product-origin-destination-months; the sample period ranges from January 2019 to December 2022. Twenty-one origin countries are China, Belgium, Bulgaria, Cambodia, Canada, Colombia, Dominican Republic, France, Germany, Guatemala, India, Indonesia, Italy, Japan, Malaysia, Mexico, Nicaragua, Pakistan, Portugal, Thailand, and Vietnam. Ban^{US}_{pm} is the coefficient on the triple interaction indicator taking a value of one if a commodity contains cotton and was exported (from any origin country) to the United States after the implementation of the Cotton Ban in December 2020. The first interaction term is a dummy for direct exports from China. The next four interactions are dummies for the quartile each origin country (apart from China) is assigned to based on its total imports of all cotton products from China in 2017 and 2018. In columns (3) and (4), the average effect is absorbed by the full set of interaction fixed effects, and $Q1$ is omitted as a reference group. Standard errors in parentheses are clustered at the product-origin level, with ***, **, and * representing significance at the 1%, 5%, and 10% level.

The net effect on third countries can be estimated straightforwardly by adding a $\beta^C Ban^D_{pdt}$ term in equation (3.1), in addition to the original Ban^{US}_{pdt} indicator. The new indicator takes a value of one for cotton-related exports from China to country D during the period the Cotton Ban was in effect. The coefficient β^D captures any potential (net) indirect effects of the sanctions on country C.

Indirect effects on third countries. We report the estimates of indirect effects on Chinese exports to a select set of destination countries in Figure 3.4. It shows the point estimates with the corresponding 95% confidence interval for the intensive margin effect in panel (a) and the extensive margin effect in panel (b). For comparison purpose, we show the US estimates from Table 3.3 on the right side (in red). Countries are ordered by effect size, averaged over the two margins.

The volume of Chinese cotton exports to Russia grows significantly and China also starts exporting additional products to Russia. At the other extreme, cotton-

related exports decline significantly to the EU on both margins, even though the EU did not impose any formal sanctions itself on Chinese products. At the intensive margin, no other point estimate is significantly different from zero, but the probability that China exports a cotton-containing product also declines for the UK, Canada, Brazil, and Japan.

The ordering of countries in Figure 3.4 seems to reflect not only economic integration, but is relatively similar to countries' geopolitical alignment, i.e, whether countries are more likely to side with China or the United States. For example, with the exception of Russia, the ordering is similar to the extent that countries' voting at the United Nations General Assembly aligns with China or the United States. Between 2017 and 2018, that showed the following similarity order: China, South Africa, India, Brazil, Mexico, Russia, Japan, EU, Canada, UK, United States.

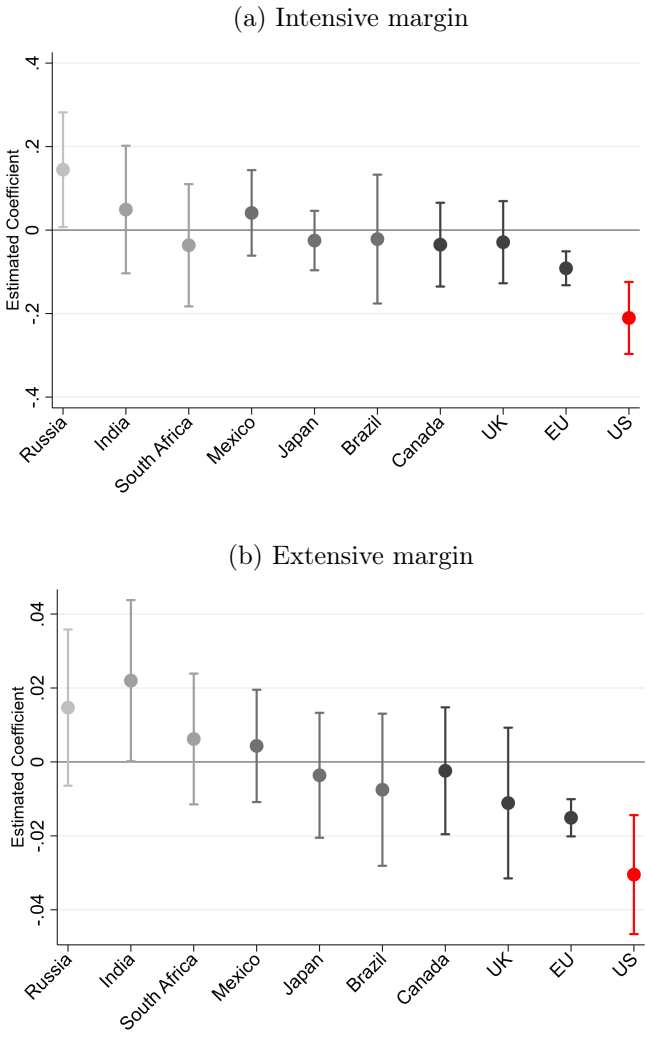
Indirect effects on the EU. We zoom in on the indirect effects of the US Cotton Ban on European imports of Chinese cotton-containing products. The estimates in columns (1) and (3) of Table 3.6 replicate the EU effects shown in Figure 3.4. The export reductions for the EU are, respectively, 60% and 56% smaller than for the United States, but highly significant. Even more than for the United States, the effects grow over time as the UFLPA is voted and then takes effect. The third coefficient is on both margins two to three times larger (in absolute value) than the immediate impact.

The event study for the indirect effects on EU imports, in Figure 3.5, indicates that the parallel trends assumption for the pre-treatment period is satisfied. The gradually increasing effect in the post-sanctions period also shows up very clearly, especially at the extensive margin.

Robustness checks We conduct a range of sensitivity checks and the results prove highly robust, i.e., consistently showing a significant decline of EU imports from China that is smaller than the direct effects on US imports (full results available upon request).

First, we estimate with a broader control group that includes non-textile and non-garment products. Second, we use a narrower control group that omits non-textile products that are ever exported directly from Xinjiang to the United States, because all Xinjiang products are sanctioned in the last seven months. Third, we rely on a more limited set of 98 destination countries as controls, because only they report any textile imports from China in every month throughout the sample

Figure 3.4: Effect of the US Cotton Ban on third country imports of Chinese cotton products



Notes: Point estimates and 95% confidence intervals of the β^D coefficient that captures the indirect effect of the US Cotton ban on imports in country D of cotton-containing products from China. Each coefficient is estimated using a separate regression for each destination, which always also incorporates an indicator for the direct effect on US imports and three sets of interaction fixed effects.

period.

Next, we use alternative definitions for the treatment group. First, we aggregate all EU imports into a single entity which generated a remarkably similar, but less

Table 3.6: Indirect impact of the Cotton Ban: Chinese exports to the EU

	(1) log(Exports)	(2) log(Exports)	(3) Export dummy	(4) Export dummy
Ban^{US}	-0.234***		-0.034***	
	(0.044)		(0.008)	
$Ban^{US} \times first$		-0.182***		-0.020**
		(0.045)		(0.008)
$Ban^{US} \times second$		-0.208***		-0.024**
		(0.060)		(0.011)
$Ban^{US} \times UFLPA$		-0.372***		-0.072***
		(0.065)		(0.013)
Ban^{EU}	-0.092***		-0.015***	
	(0.021)		(0.003)	
$Ban^{EU} \times first$		-0.051**		-0.010***
		(0.021)		(0.003)
$Ban^{EU} \times second$		-0.103***		-0.016***
		(0.027)		(0.003)
$Ban^{EU} \times UFLPA$		-0.160***		-0.023***
		(0.030)		(0.004)
Observations	1,914,465	1,914,465	4,844,448	4,844,448
Product-destination FE	✓	✓	✓	✓
Product-month FE	✓	✓	✓	✓
Destination-month FE	✓	✓	✓	✓

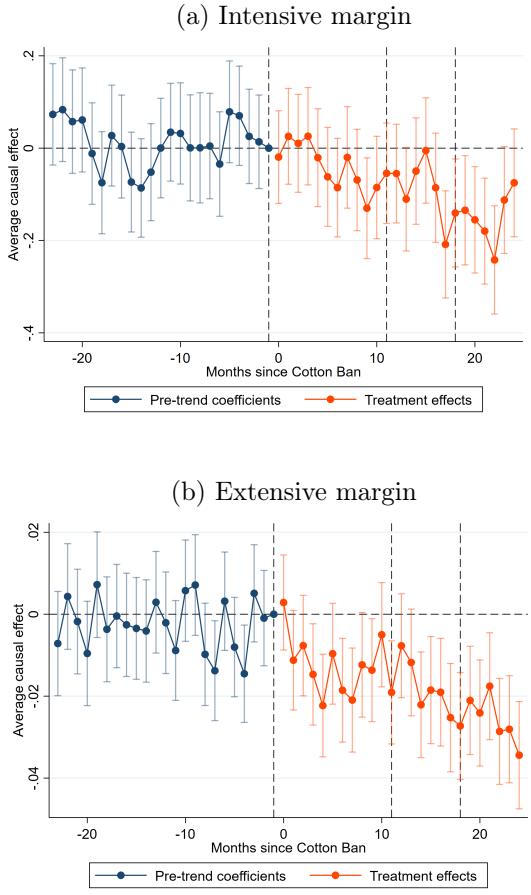
Notes: Estimation results for equation (3.1); the units of observation are imports from China at the product-destination-month; the sample period ranges from January 2019 to December 2022. Ban^{US} is the coefficient on the triple interaction indicator taking a value of one if a commodity contains cotton and was exported from China to the United States after the implementation of the Cotton Ban in December 2020. Ban^{EU} is a comparable indicator, but for Chinese imports into any of the 26 EU countries; the sample omits Austria which does not provide monthly trade information and the UK is not included in the EU. The three interaction terms in columns (2) and (4) represent different time periods: (1) $first$ is the period between December 2020 and November 2021, the first year of the Cotton Ban; (2) $second$ is the period between December 2021 and June 2022, the second year of the Cotton Ban, when the $UFLPA$ was enacted but not yet in force; and (3) $UFLPA$ is the period from July 2022 onward when the UFLPA is in effect. Standard errors in parentheses are clustered at the product-destination level, with ***, **, and * representing significance at the 1%, 5%, and 10% level.

precise, point estimate of -0.101*** at the intensive margin. However, the estimated coefficient at the extensive margin becomes near zero. Second, using a more narrow definition of 195 instead of 238 treated commodities, following Carlson and Weaver (2022), barely changes the results.

Finally, the PPML estimate for the coefficient on Ban^{EU} is -0.072**, which is statistically significant only at the 5% level, and approximately half as large as the -0.129 direct effect on Ban^{US}.

Note:

Figure 3.5: Event study of the indirect impact on EU imports



Notes: The figure shows the estimated coefficients for 47 individual interaction terms, along with the corresponding 95% confidence intervals, for the indirect effects on EU imports of cotton-related products that fall under the US Cotton Ban. All regressions also include 47 terms capturing the direct effect on US imports, as well as three sets of interaction fixed effects. Standard errors are clustered at the product-destination level. The vertical dashed lines indicate November 2020 (the baseline period, −1, with values normalized at 0), November 2021, and June 2022, the three months before each of the three US Cotton Ban measures are enacted.

3.5.2 Potential explanations

The indirect effects of the US Cotton Ban cannot be attributed to EU policy interventions, as no formal sanctions targeting Chinese commodities were implemented by the EU during the sample period. Supply-side factors, such as technological shocks affecting Chinese production, are controlled for through product-month fixed effects and, if anything, would lead us to expect increased EU imports through trade

diversion. We explore three demand-side mechanisms that could potentially explain the import decline.

Reduced market power (of China). The US Cotton Ban lowered demand for Chinese cotton-containing products in the United States and in countries exporting final products to the United States. Given the size of the US market, this would reduce any market power Chinese exporters might have in other destinations and could put downward pressure on prices. If quantities adjust only slowly, lower prices could reduce the value of exports to other destination markets, such as the EU.

This can be straightforwardly tested by estimating equation (3.1) separately using log price and log quantity as dependent variables. Results in Table 3.7 clearly indicate that this mechanism cannot explain the EU import decline. The indirect effect is entirely driven by a reduction in quantity with no significant effect of sanctions on Chinese export prices.

Table 3.7: Indirect impact of the Cotton Ban: Price versus quantity

	(1) log(Exports)	(2) log(Price)	(3) log(Quantity)
Ban^{US}	-0.234***	0.030	-0.253***
	(0.044)	(0.024)	(0.055)
Ban^{EU}	-0.092***	0.000	-0.092***
	(0.021)	(0.010)	(0.024)
Observations	1,914,465	1,866,363	1,866,364
Product-destination FE	✓	✓	✓
Product-month FE	✓	✓	✓
Destination-month FE	✓	✓	✓

Notes: Estimation results for equation (3.1) using three different dependent variables. The units of observation are imports from China at the product-destination-month; the sample period ranges from January 2019 to December 2022. Ban^{US} is the coefficient on the triple interaction indicator taking a value of one if a commodity contains cotton and was exported from China to the United States after the implementation of the Cotton Ban in December 2020. Ban^{EU} is a comparable indicator, but for Chinese imports into any of the 26 EU countries. The sample omits Austria which does not provide monthly trade information and the UK is not included in the EU. Standard errors in parentheses are clustered at the product-destination level, with ***, **, and * representing significance at the 1%, 5%, and 10% level.

Export potential to the United States. The US Cotton Ban requires all firms that export to the United States, not only Chinese firms, to provide sufficient evidence that none of their own cotton inputs or those of their suppliers are sourced from Xinjiang. As we discussed, the burden of proof increased substantially under the UFLPA. Faced with uncertainty regarding the traceability of cotton origins within Chinese supply chains, European firms, particularly those relying most on

the US export market, may opt to reduce imports of cotton-containing products from China.[25] Both the need to avoid Xinjiang-origin cotton and the mere cost of compliance with the US Cotton Ban, could motivate reduced imports from China.

To empirically assess whether the re-exporting channel is a plausible explanation for the negative indirect effect, we divide all EU countries in two groups according to the extent their exports were directed to the United States prior to the Cotton Ban. Figure 3.D.2 in Appendix 3.D shows the full ranking and the six EU countries in the top quartile of US exposure, i.e., Portugal, Italy, Romania, and Malta, Lithuania, Latvia, and Bulgaria are categorized as "High" exporters to the US.

We incorporate an additional interaction term ($Ban^{EU} \times$ High US exports) in equation (3.1) to capture any incremental effect of the Cotton Ban on these countries. Columns (1) and (3) of Table 3.8 show an unexpected positive effect at the intensive margin, and no statistically significant effect at the extensive margin. The EU import decline of Chinese cotton-containing products is clearly not driven by countries most reliant on the US export market.

This finding is not altogether unexpected. It is in line with the results in Table 3.5 where we also did not find lower US imports from countries that sourced cotton inputs most intensely from China. A robustness check verifying whether a pattern exists between US imports and specifically EU countries' reliance on cotton imports from China, does not support this mechanism either.

Reputational concerns. While allegations of human rights violations in Xinjiang have been circulating since 2017, the US Cotton Ban could make the connection between Xinjiang cotton and the use of forced labor more salient for European consumers. Some consumers may become more conscientious about the origins of the products they purchase even without a formal ban. International non-governmental organizations have been instrumental in lobbying for the Cotton Ban in the United States. In Europe they did not succeed in getting formal sanctions approved, but they still appeal directly to consumers. Therefore, continuing to sell products made with cotton from China in markets with a heightened awareness of human rights issues could pose a risk to a company's reputation and brand valuation.

We examine this mechanism by leveraging variation across EU countries in awareness about human rights issues. We utilize Google Trends data to construct

[25] These incentives extend to firms that wish to maintain uniform product content across all consumer markets they serve.

such an awareness index for all EU countries, based on search activity surrounding historical human rights crises.[26] Details of the calculations are provided in Appendix 3.C and Figure 3.C.1 shows the results, with Germany, the Netherlands, Finland and Sweden showing the highest values. Among the nineteen EU countries that we can rank, those with a value above the median are classified as "High HR awareness."[27]

Estimation results with an additional interaction term for EU countries with high human rights awareness are also in Table 3.8. At the intensive margin, the coefficient of interest is estimated very precisely and equal to -0.153*** in column (2). With the interaction term included, the baseline effect on Ban^{EU} shrinks almost to zero and loses statistical significance. It implies that the reduction in EU imports is driven entirely by countries with greater attention to human rights issues. This finding is consistent with Koenig and Poncet (2022) who also find that firms reduce sourcing from origin countries that pose reputational risks. However, this pattern does not replicate in the extensive margin results which show a small positive effect on the interaction term. This may be explained with reputational concerns chiefly affecting the behavior of large firms which dominate the impact at the intensive margin, but not at the extensive margin. Taken together, this finding implies that reputational risks drove firms to adjust import volumes rather than to exit the market entirely.

In Figure 3.6 we present one final piece of evidence that is consistent with the importance of reputational concerns. We classify all sample commodities into five subgroups and estimate the direct and indirect effects for the United States and the EU separately by product group. The point estimates for the direct impact on US imports (in red) are larger (in absolute value) for less processed cotton products. This is especially apparent at the extensive margin. It is intuitive that effects on imports that fall directly under the Cotton Ban legislation are strongest for products where the legal ban is easiest to enforce, such as raw, unprocessed cotton.

The indirect impact on EU imports (in blue) shows the reverse pattern. Effects at the intensive margin, which are dominated by large firms, are strongest for apparel and only slightly smaller for fabric. In contrast, there is no significant impact on yarn or raw cotton, and those point estimates are even positive. This pattern is consistent with indirect effects stemming from the reputation mechanism. In the

[26] We selected human right crises not involving China to avoid a possible endogeneity problem as anti-Chinese sentiments might affect trade with China, independently from the US Cotton Ban.

[27] Due to data limitations, we cannot calculate the index for seven EU countries. Rather than omit them from the regression, we set the indicator for "High HR awareness" to zero for them.

EU, products closer to consumers see a larger decline; in the US, products that are less processed, and where Xinjiang origin is likely to be easier to detect, see a larger decline.

Table 3.8: Indirect impact of the Cotton Ban: Re-export versus reputation considerations

	(1) log(Exports)	(2) log(Exports)	(3) Export dummy	(4) Export dummy
Ban^{US}	-0.234***	-0.234***	-0.034***	-0.034***
	(0.044)	(0.044)	(0.008)	(0.008)
Ban^{EU}	-0.118***	-0.031	-0.015***	-0.019***
	(0.023)	(0.025)	(0.003)	(0.003)
Ban^{EU} × High US exports	0.127***		-0.001	
	(0.042)		(0.005)	
Ban^{EU} × High HR aware		-0.153***		0.012**
		(0.036)		(0.005)
Observations	1,914,465	1,914,465	4,844,448	4,844,448
Product-destination FE	✓	✓	✓	✓
Product-month FE	✓	✓	✓	✓
Destination-month FE	✓	✓	✓	✓

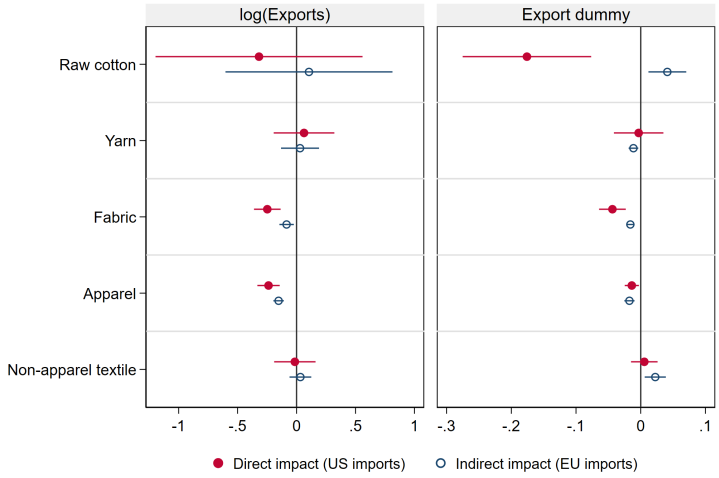
Notes: Estimation results for equation (3.1) with additional interaction terms; the units of observation are imports from China at the product-destination-month; the sample period ranges from January 2019 to December 2022. Ban^{US} is the coefficient on the triple interaction indicator taking a value of one if a commodity contains cotton and was exported from China to the United States after the implementation of the Cotton Ban in December 2020. Ban^{EU} is a comparable indicator, but for Chinese imports into any of the 26 EU countries. The sample omits Austria which does not provide monthly trade information and the UK is not included in the EU. The interaction term "High US exports" is an indicator that equals one for country-product combinations where the EU country has a US export share for that specific cotton-containing product that exceeds the 75th percentile (across EU countries). The "High HR search" indicator equals one for EU countries with a relative Google keywords search intensity for human rights crises that exceeds the median (across EU countries). Standard errors in parentheses are clustered at the product-destination level, with ***, **, and * representing significance at the 1%, 5%, and 10% level.

3.6 Conclusion

There has been a notable increase in the use of targeted sanctions, but the impact of such measures remains underexplored, particularly in the context of global value chains. We examine the region-wide ban on the imports of Xinjiang-origin cotton, implemented by the United States in December 2020, as a case study to provide new empirical evidence on the effectiveness of this policy tool.

Two findings stand out. First, it is challenging to implement such a targeted policy in a world with internationally fragmented production. When processed goods arrive at US ports, US customs needs to ascertain whether they contain Xinjiang-origin cotton or not. We find that the heterogeneity in the US import decline across Chinese provinces is correlated with provinces' reliance on Xinjiang-

Figure 3.6: Heterogeneous impact of the Cotton Ban by product category



Notes: Coefficient estimates with 95% confidence intervals from separate regressions for each of the five product categories and both dependent variables. The sample period ranges from January 2019 to December 2022. Regressions include indicators for both the direct (US) and indirect (EU) effects and three sets of interaction fixed effects.

origin cotton, which we verify using China's interprovincial Input-Output table. However, we fail to detect a systematic correlation for the change in US imports across countries, even though countries' reliance on Chinese-origin cotton also varies greatly.

Second, while the policy consists of targeted sanctions, sourcing links in global value chains potentially lead to spillover effects on other countries. While this can enhance the impact of the sanctions, as producers in other countries indirectly help enforce the US Cotton Ban, it may also spread the costs to producers in other countries. We investigate specifically for the EU which factors best explain the observed decline of cotton-related imports from China. Consumer-facing firms turning away from China in order to avoid potential damage to their reputation seems to be even more important than global value chain connections.

Our analysis also suggests a few areas for future research. While our identification strategy isolates the direct and spillover effects of the ban on Chinese exports, it does not speak to broader general-equilibrium effects. In particular, we do not observe how Chinese authorities and firms adjust along other margins, e.g., they might reduce Chinese own cotton imports, redirect Xinjiang-linked cotton products to domestic markets, shift sourcing between provinces, or use fiscal and industrial policies

to cushion affected producers. These questions are important for understanding the full welfare and policy implications of the sanctions. While we document indirect effects of the Cotton Ban on Chinese exports to other countries, we do not explicitly analyze trade diversion and how sanctions lead to a reorganization of the global textile value chain, e.g., which countries gain? We also do not explore the potential consequences of the Cotton Ban for the United States itself, the country imposing the sanctions. Specifically, the adjustment in global value chains could yield both positive and negative economic outcomes for American firms and consumers. To investigate these issues, a more structural model-based approach is likely to be required.

References

Ahn, D. P., & Ludema, R. D. (2020). The sword and the shield: The economics of targeted sanctions. *European Economic Review*, *130*, 103587.

Bělín, M., & Hanousek, J. (2021). Which sanctions matter? Analysis of the EU/Russian sanctions of 2014. *Journal of Comparative Economics*, *49*(1), 244–257.

C4ADS. (2021). *Long shadows: How the global economy supports oppression in Xinjiang.* [Washington, DC]. https://c4ads.org/wp-content/uploads/2021/08/LongShadows-Report.pdf

Carlson, D., & Weaver, M. (2022). *Disentangling the Knot: Identifying US and global exposure to Xinjiang cotton.* Office of Industries, US International Trade Commission. https://www.usitc.gov/publications/332/working_papers/disentangling_the_knot_final_102522-compliant.pdf

Chen, Q., Gao, Y., Pan, C., Xu, D., Cai, K., Guan, D., He, Q., Li, S., Liu, W., & Meng, B. (2023). An interprovincial input–output database distinguishing firm ownership in China from 1997 to 2017. *Scientific Data*, *10*(1), 293.

Conway, J., & Boxell, L. (2024). *Consuming values* [Available at SSRN 4855718].

Correia, S., Guimarães, P., & Zylkin, T. (2020). Fast poisson estimation with high-dimensional fixed effects. *The Stata Journal*, *20*(1), 95–115.

Crozet, M., & Hinz, J. (2020). Friendly fire: The trade impact of the Russia sanctions and counter-sanctions. *Economic Policy*, *35*(101), 97–146.

Draca, M., Garred, J., Stickland, L., & Warrinnier, N. (2022). On target? Sanctions and the economic interests of elite policymakers in Iran. *Economic Journal*, *133*(649), 159–200.

Fajgelbaum, P. D., & Khandelwal, A. K. (2022). The economic impacts of the US–China trade war. *Annual Review of Economics*, *14*(1), 205–228.

Frazer, G., & Van Biesebroeck, J. (2010). Trade growth under the African growth and opportunity act. *Review of Economics and Statistics*, *92*(1), 128–144.

Fuchs, A., & Klann, N.-H. (2013). Paying a visit: The Dalai Lama effect on international trade. *Journal of International Economics*, *91*(1), 164–177.

Gale, F., & Davis, E. (2022). *Chinese cotton: Textiles, imports, and Xinjiang* [USDA Economic Research Report No. 307].

Gereffi, G. (1999). International trade and industrial upgrading in the apparel commodity chain. *Journal of International Economics*, *48*(1), 37–70.

Gutmann, J., Neuenkirch, M., & Neumeier, F. (2023). The economic effects of international sanctions: An event study. *Journal of Comparative Economics*, *51*(4), 1214–1231.

Haidar, J. I. (2017). Sanctions and export deflection: Evidence from Iran. *Economic Policy*, *32*(90), 319–355.

Heilmann, K. (2016). Does political conflict hurt trade? Evidence from consumer boycotts. *Journal of International Economics*, *99*, 179–191.

Human Rights Watch. (2021). *Break their lineage, break their roots: China's crimes against humanity targeting Uyghurs and other Turkic muslims* [Accessed: 2025-06-21]. %5Curl%7Bhttps://www.hrw.org/report/2021/04/19/

break-their-lineage-break-their-roots/chinas-crimes-against-humanity-targeting%7D

Koenig, P., & Poncet, S. (2022). The effects of the Rana Plaza collapse on the sourcing choices of French importers. *Journal of International Economics*, *137*, 103576.

Lehr, A. (2020). *Addressing forced labor in the Xinjiang Uyghur Autonomous Region*. Center for Strategic & International Studies Briefs. https://csis-website-prod.s3.amazonaws.com/s3fs-public/publication/200730_Lehr_XinjiangUyghurForcedLabor_brief_FINAL_v2.pdf

Lehr, A. K., & Bechrakis, E. M. (2019). *Connecting the dots in Xinjiang: Forced labor, forced assimilation, and western supply chains* (Report of the CSIS Human Rights Initiative). Center for Strategic and International Studies. Retrieved June 22, 2025, from https://csis-website-prod.s3.amazonaws.com/s3fs-public/publication/Lehr_ConnectingDotsXinjiang_interior_v3_FULL_WEB.pdf

Li, Y., Jian, Z., Tian, W., & Zhao, L. (2021). How political conflicts distort bilateral trade: Firm-level evidence from China. *Journal of Economic Behavior & Organization*, *183*, 233–249.

Liaukonytė, J., Tuchman, A., & Zhu, X. (2023). Frontiers: Spilling the beans on political consumerism: Do social media boycotts and buycotts translate to real sales impact? *Marketing Science*, *42*(1), 11–25.

Makioka, R., & Zhang, H. (2024). The impact of export controls on international trade: Evidence from the Japan–Korea trade dispute in semiconductor industry. *Journal of the Japanese and International Economies*, *74*, 101336.

Maystadt, J.-F., Tsang, K., & Van Biesebroeck, J. (2026). Tracing sanctions through the value chain: Evidence from the US cotton ban. *CEPR Discussion Paper*, (21438).

Maystadt, J.-F., Tsang, K., Van Biesebroeck, J., & Warrinnier, N. (2024). Targeting the corps: The incidence of humanitarian sanctions on firm performance in China. *CEPR Discussion Paper*, (18790).

McConnell, C., Margalit, Y., Malhotra, N., & Levendusky, M. (2018). The economic consequences of partisanship in a polarized era. *American Journal of Political Science*, *62*(1), 5–18.

Miromanova, A. (2023). The effectiveness of embargoes: Evidence from Russia. *World Economy*, *46*(4), 906–940.

Morgan, T. C., Syropoulos, C., & Yotov, Y. V. (2023). Economic sanctions: Evolution, consequences, and challenges. *Journal of Economic Perspectives*, *37*(1), 3–29.

Murphy, L., Elimä, N., & Tobin, D. (2022). *Until nothing is left: China's settler corporation and its human rights violations in the Uyghur Region. A report on the Xinjiang Production and Construction Corps* [Sheffield Hallam University. Helena Kennedy Centre for International Justice]. https://www.shu.ac.uk/-/media/home/research/helena-kennedy-centre/projects/until-nothing-is-left-docs/murphy-elima-tobin----until-nothing-is-left.pdf

Nigmatulina, D. (2023). *Sanctions and misallocation. How sanctioned firms won and Russia lost* [CEP Discussion Paper No. 1886].

Office of the United Nations High Commissioner for Human Rights. (2022). *OHCHR assessment of human rights concerns in the Xinjiang Uyghur Autonomous Region, People's Republic of China*. https://www.ohchr.org/sites/default/files/documents/countries/2022-08-31/22-08-31-final-assesment.pdf

Olden, A., & Møen, J. (2022). The triple difference estimator. *Econometrics Journal*, *25*(3), 531–553.

Schoenmueller, V., Netzer, O., & Stahl, F. (2023). Frontiers: Polarized America: From political polarization to preference polarization. *Marketing Science*, *42*(1), 48–60.

Silva, J. S., & Tenreyro, S. (2006). The log of gravity. *Review of Economics and statistics*, *88*(4), 641–658.

US Customs and Border Protection. (2022). *Uyghur Forced Labor Prevention Act. Operational guidance for importers* [CBP Publication No. 1793-0522]. https://www.cbp.gov/sites/default/files/assets/documents/2022-Jun/CBP_Guidance_for_Importers_for_UFLPA_13_June_2022.pdf

US Department of Agriculture. (2025). *Cotton and products annual* (tech. rep. No. CH2025-0087). USDA Foreign Agricultural Service. https://apps.fas.usda.gov/newgainapi/api/Report/DownloadReportByFileName?fileName=Cotton+and+Products+Annual_Beijing_China+-+People%27s+Republic+of_CH2025-0087.pdf

US Department of Homeland Security. (2022). *Strategy to prevent the importation of goods mined, produced, or manufactured with forced labor in the People's Republic of China* [Office of Strategy, Policy and, Plans. Report to Congress, June 17, 2022]. https://www.dhs.gov/sites/default/files/2022-06/22_0617_fletf_uflpa-strategy.pdf

Voora, V., Bermudez, S., Farrell, J. J., Larrea, C., & Luna, E. (2023). *Global market report: Cotton prices and sustainability* (tech. rep.). International Institute for Sustainable Development. https://www.iisd.org/system/files/2023-01/2023-global-market-report-cotton.pdf

Zenz, A. (2023). Coercive labor in the cotton harvest in the Xinjiang Uyghur Autonomous Region and Uzbekistan: A comparative analysis of state-sponsored forced labor. *Communist and Post-Communist Studies*, *56*(2), 1–32.

Appendix 3.A Sample of commodities

Table 3.A.1: Number of products in the sample (treated products in parenthesis)

HS 2-digit	Description	Raw	Yarn	Fabrics	Apparel	Other textiles
50	Silk	3	2	4	0	0
51	Wool, fine or coarse animal hair; horsehair yarn and woven fabric	12	15	11	0	0
52	Cotton	5 (5)	49 (49)	70 (70)	0	0
53	Vegetable textile fibres; paper yarn and woven fabrics of paper yarn	9	7	7	0	0
54	Man-made filaments	0	36	34 (3)	0	0
55	Man-made staple fibres	0	24	84 (31)	0	0
56	Wadding, felt and nonwovens, special yarns; twine, cordage, ropes and cables and articles thereof	0	14	17 (1)	0	0
57	Carpets and other textile floor coverings	0	0	0	0	23
58	Fabrics; special woven fabrics, tufted textile fabrics, lace, tapestries, trimmings, embroidery	0	0	37 (9)	0	0
59	Textile fabrics; impregnated, coated, covered or laminated; textile articles of a kind suitable for industrial use	0	0	24	0	0
60	Fabrics; knitted or crocheted	0	0	44 (12)	0	0
61	Apparel and clothing accessories; knitted or crocheted	0	0	0	106 (25)	0
62	Apparel and clothing accessories; not knitted or crocheted	0	0	0	104 (22)	0
63	Textiles, made up articles; sets; worn clothing and worn textile articles; rags	0	3	4	1	47 (11)
65	Headgear and parts thereof	0	0	0	0	8
TOTAL		29(5)	150 (49)	336 (126)	211 (47)	78 (11)

Source: World Integrated Trade Solution (WITS) of the World Bank (2024), accessible at https://wits.worldbank.org/trade/country-byhs6product.aspx?lang=en.
Notes: Products are defined at the HS 6-digit level. The number of cotton-containing commodities are in parentheses. We use the method of Carlson and Weaver (2022) to categorize 801 commodities at the HS 6-digit level into five groups. Three control commodities in 63 are classified as both *Yarn* and *Fabrics*.

Appendix 3.B Province ranking based on intensity of input sourcing from Xinjiang

We use the supply chain links between Chinese provinces and Xinjiang observed in the interprovincial Input-Output table of Chen et al. (2023) to classify provinces into four groups. For each province, we calculate for the two sectors of 'Manufacture of textiles' and 'Manufacture of textile wearing apparel, footwear, leather, fur, feather and its products' how much of their inputs come from the Xinjiang sector of 'Farming, forestry, animal production and fishery' or from either of the two Xinjiang sectors mentioned above.

A share-based measure may understate exposure in large, diversified textile provinces and produces counterintuitive rankings, such as Inner Mongolia and Tianjin appearing highly Xinjiang-dependent. We therefore use absolute input values, which better align with China's main cotton textile and garment production centers, while acknowledging possible scale effects.

Results in Table 3.B.1 illustrate that the bulk of output from the first two sectors is used as input in the textile sectors of Xinjiang or other Chinese provinces, but only a small fraction of the third sector.

Naturally, these broadly defined categories will encompass some non-cotton inputs from Xinjiang, but we do not observe supply linkages at a more detailed level of industry or product aggregation. From the Xinjiang Statistical Yearbook we know that cotton accounts for 24% of total output in the Xinjiang agricultural sector and we have multiplied the inputs from that sector by 0.24 before showing them in Table 3.B.1 and adding them to the inputs of the other two sectors.

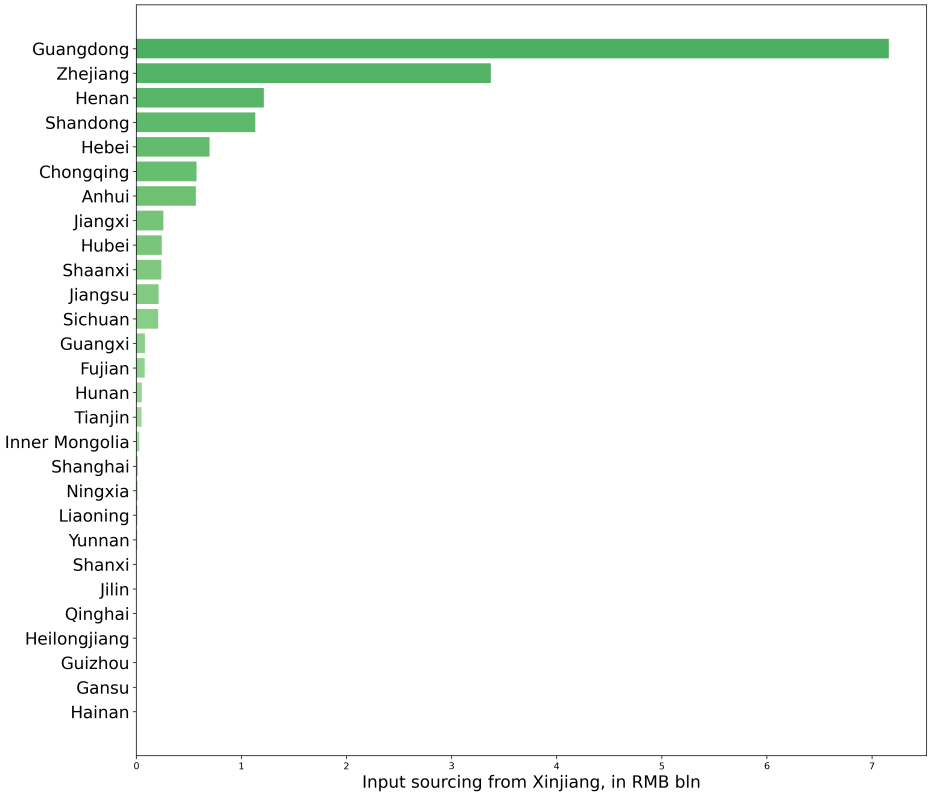
We rank all Chinese provinces according to their total sourcing from Xinjiang, shown in Figure 3.B.1, and categorize them into four quartiles based on that index.

Table 3.B.1: Destination of the output of cotton-related sectors in Xinjiang

	(1)	(2)	(3)	(4)	(5)	(6)
	Intermediate use		Domestic consumption		Exports	Total
(in millions, RMB)	Xinjiang	Others	Xinjiang	Others		
Xinjiang Agricultural	23,167	16,927	11,520	17,794	663	70,071
	(33%)	(24%)	(16%)	(25%)	(1%)	(100%)
Xinjiang Textile	6,547	19,747	1,105	1,078	10,506	38,983
	(17%)	(51%)	(3%)	(3%)	(27%)	(100%)
Xinjiang Garment	249	0.3	1,152	0.2	3,820	5,222
	(5%)	(0%)	(22%)	(0%)	(73%)	(100%)

Source: Calculations based on China interprovincial input–output (IPIO) database for 2017 of Chen et al. (2023).
Notes: Columns (1)-(5) show the value and share of output of sector i in Xinjiang that is used as intermediate input or domestic consumption (consumption + investment) in Xinjiang or any other Chinese province, or is exported.

Figure 3.B.1: Chinese provinces ranked by cotton-related input sourcing from Xinjiang



Source: Calculations based on China interprovincial input–output database 2017 of Chen et al. (2023).
Notes: Values indicate the cotton-related intermediate inputs (3 input-output sectors) from Xinjiang that are used in textile-related sectors (2 input-output sectors) of each province. Tibet and Beijing are excluded from the analysis.

Appendix 3.C Country ranking based on attention to human rights crises

Google Trends makes information regarding the online search activity for a broad range of keywords available to researchers by country and time period. It does not release the absolute number of searches, but employs a two-step normalization process. First, the daily search volume for each keyword is adjusted by the total daily search volume across all keywords in a country. Second, the results are further normalized by the maximum daily search volume within a sample period. If a search term consists of two keywords, the normalization is relative to the maximum daily search intensity of either of the two keywords. The resulting relative search intensity for each keyword or group of keywords falls within a range of 0 to 100.

We collect data on the relative search intensity corresponding to several historical human rights crises that occurred in the 2010s. Each of these crises occurred in different regions and have drawn condemnation from the European Parliament. Rather than working with individual keywords or search terms, we identify so-called "topics" associated with an incident. Topics are conceptual grouping of related terms, that even include words in different languages. They are classified by Google Trends and are the building blocks we work with. This approach facilitates capturing the intensity of search over all relevant search terms associated with an event. In some cases we pick "ethnic groups", which is a group of related keywords much like a topic and is also defined by Google Trends. In a time period surrounding human rights violations that involve a certain ethnic group, we assume the search activity in EU countries pertain to that specific event. We select "Traffic (Topic)" as a control search topic to normalize the average search intensity over topics separately for each EU country.

The first three lines in Table 3.C.1 lists three human rights crises that we could clearly associate with a Google Trends-defined topic and that occurred prior to our study period. They include the attacks on schools and the kidnappings of schoolgirls by the jihadist militant group Boko Haram in Nigeria, the Yazidi genocide and refugee crisis caused by Islamic State in Northern Iraq, and the genocide against the Rohingya people by the army in Myanmar.

We calculate a search intensity index for each EU country by averaging the 2-

Table 3.C.1: Keywords for selected human right crises

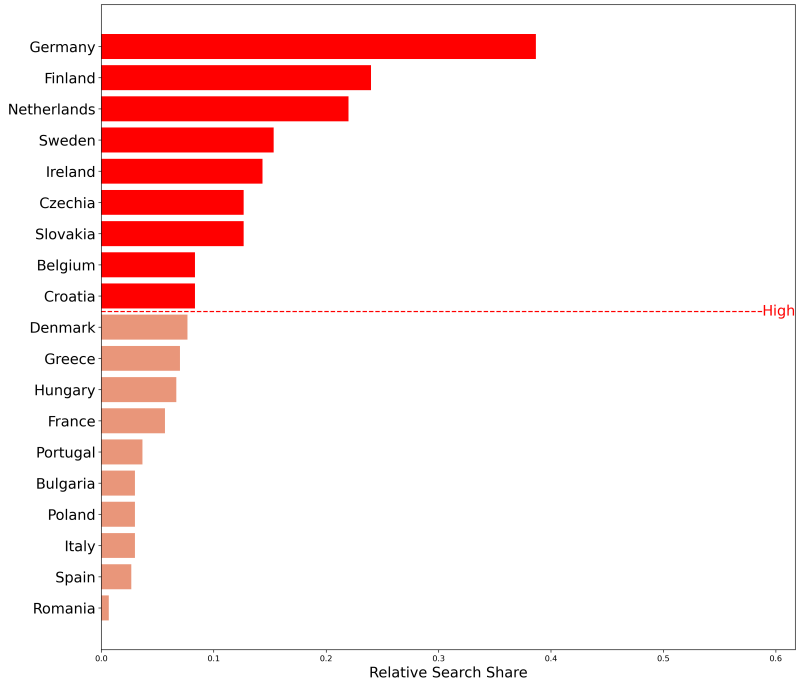
Human rights crisis	Google Trends topic	Peak of global search	2-month search period
Boko Haram kidnappings	Boko Haram (Topic)	May 12, 2014	April 13 – June 11
Yazidi genocide	Yazidis (People)	Aug 10, 2014	July 12 – Sept. 9
Rohingya genocide	Rohingya people (Ethnic group)	Sep 6, 2017	Aug. 8 – Oct. 6
2019 Turkish offensive into north-eastern Syria	Kurds (Ethnic group)	Oct 10, 2019	Sept. 11 – Nov. 9
Persecution of Uyghurs in China	Uyghurs (Ethnic group)	Dec 22, 2019	Nov. 23 – Jan. 21

Source: "Topic" definitions and date of peak global search are obtained from the Google Trends database.

month relative search share (against "Traffic"). Search data for all three crises in Cyprus, Estonia, Latvia, Lithuania, Luxembourg, Malta, and Slovenia are labeled as low volumes searches by Google Trends and we do not calculate the index for them. Figure 3.C.1 shows the ranking across EU countries and the dividing line for the median index.

Robustness checks using search volumes over a single month or using two additional crises, including the Chinese treatment of Uyghur minorities in Xinjiang, yield a very similar ranking.

Figure 3.C.1: European countries ranked by relative online search intensity for human rights crises



Notes: The search intensity regarding three human rights crises is calculated using data from Google Trends. These crises are (1) the Boko Haram kidnappings in 2014, (2) the Yazidi genocide in 2014; and (3) the Rohingya genocide in 2017. The two-month relative search intensities on each of these three Google Trends-generated "topics" is normalized by the "Traffic" topic in each EU country. Seven EU countries are excluded because Google designates them as having too low search volume. Furthermore, Austria is not included in our sample.

Appendix 3.D Country rankings using trade data

There are two ways for countries to be highly exposed to the US Cotton Ban. First, countries can source lots of cotton products from China, or second, countries can export lots of cotton products to the United States.

We first investigate whether the reduction of US imports is heterogeneous according to exporters' sourcing from China. In the analysis reported in Table 3.5, we extend the baseline three-dimensional balanced panel of Chinese exports with comparable information for 20 additional origin countries. Panel A of Table 3.D.1 reports these countries' rankings as importers of cotton products from China and as exporters of cotton products to the United States in 2017 and 2018. The countries are chosen to all have high US export exposure, but differing in their reliance on Chinese imports. They range from the 1st to the 55th largest exporter (to the United States) and from the 2nd to the 76th largest importer (from China).

In a robustness check reported in Table 3.E.3, we add six additional origin countries to the four-dimensional sample. They are reported in Panel B of Table 3.D.1. The full sample of 26 countries used in that analysis covers all top-30 cotton exporting countries (to the United States) that have complete monthly trade data over the sample period.

In a second robustness check, we estimate a triple-difference specification similar to the baseline analysis. In this case, we construct a three-dimensional balanced panel of US imports from all origins, rather than Chinese exports to all destinations. We calculate the total import value of cotton-related products from China for each of 207 origin countries in 2017 and 2018. For 33 origin countries that do not report annual import data in UN Comtrade, we rely on the Chinese export data. Countries are categorized into four quartiles according to their extent of cotton sourcing from

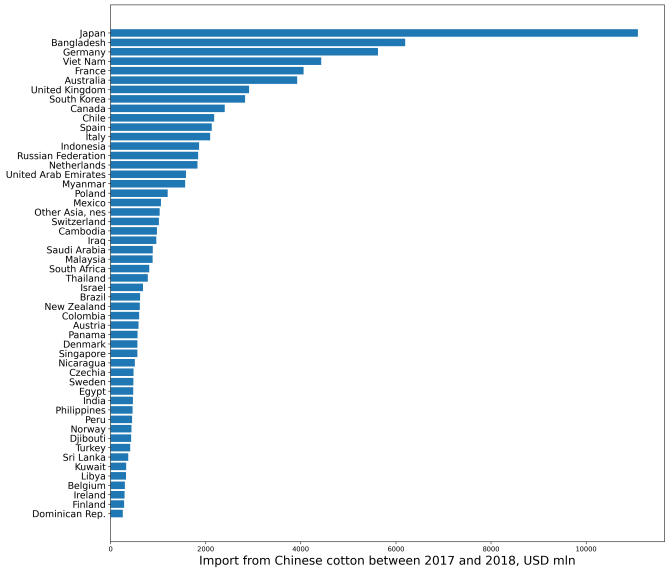
Table 3.D.1: Rankings of origin countries

Origin	Ranking as importer from China	Ranking as exporter to US	Origin	Ranking as importer from China	Ranking as exporter to US
Panel A: Twenty selected origin countries (used in Table 3.5)					
Belgium	41	55	Indonesia	13	5
Bulgaria	76	47	Italy	12	16
Cambodia	21	8	Japan	2	28
Canada	9	26	Malaysia	22	21
Colombia	28	29	Mexico	19	6
Dominican Rep.	44	17	Nicaragua	32	9
France	5	33	Pakistan	57	4
Germany	3	44	Portugal	63	23
Guatemala	60	12	Thailand	24	19
India	36	2	Vietnam	4	1
Panel B: Six additional origin countries (added in Table 3.E.3)					
Egypt	35	18	Philippines	37	22
El Salvador	49	10	South Korea	8	24
Mauritius	55	27	Turkey	40	13

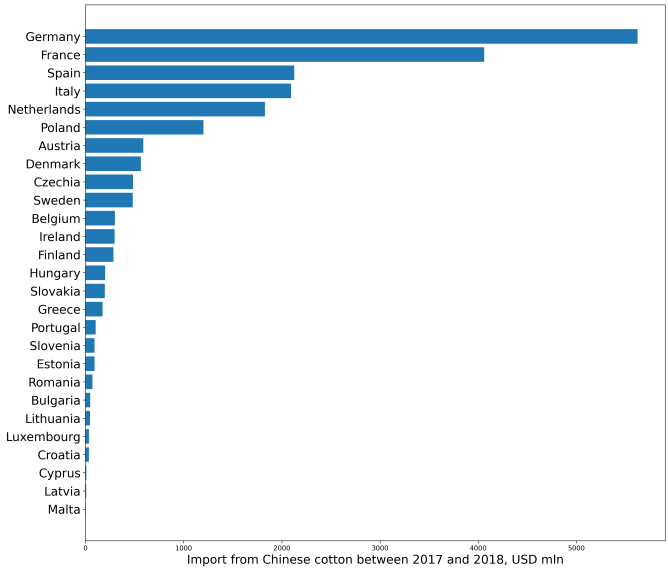
Note: Panel A displays the 20 origin countries (in addition to China) used for the analysis reported in Table 3.5 and Panel B the six additional countries added to the sample for the analysis in Table 3.E.3. The rankings are calculated based on imports and exports of cotton products in 2017 and 2018.

Figure 3.D.1: Country rankings by cotton input sourcing from China

(a) All countries (4th quartile)



(b) EU countries



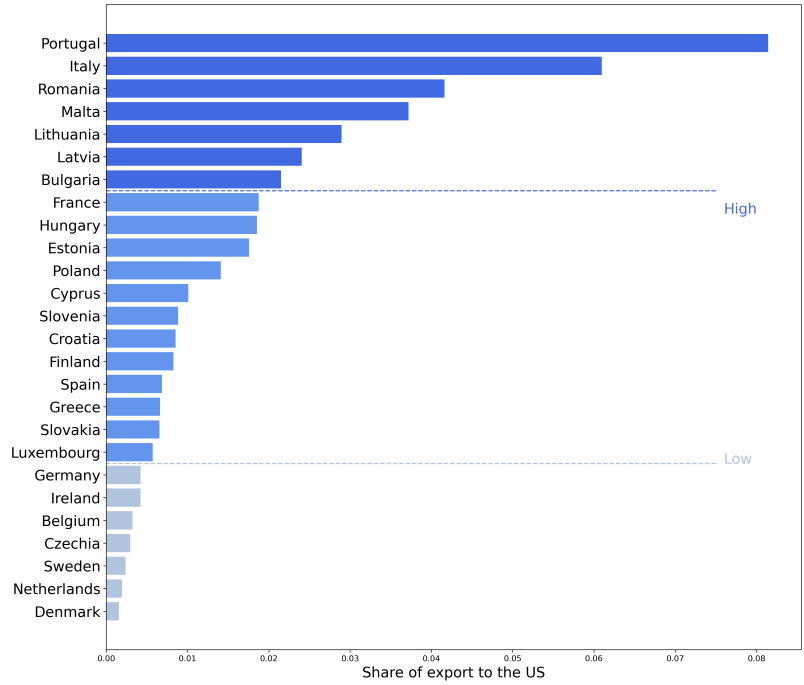
Source: Calculations based on UN Comtrade data.

Notes: Import values of cotton and cotton products from China in 2017 and 2018. Austria is excluded.

China and origin countries in the top quartile are shown in panel (a) of Figure 3.D.1. A similar ranking based only on EU countries is shown in panel (b).

Second, we also investigate heterogeneity of the indirect effects of the Cotton ban. Countries with higher own exports of cotton-related products to the United States may protect their access to the large US market by changing to alternative sources of cotton-related inputs than China. Specifically for EU countries, we measure what fraction of each country's exports of cotton textiles and garments (excluding raw cotton) in 2017 and 2018 are directed to the United States. A higher ratio reflects stronger dependence on the US market and, consequently, greater exposure to the indirect effects of the Cotton Ban. Austria is excluded from the sample, while for Luxembourg the measure is based only on 2018, as only 10 observations are reported in 2017. The ranking is shown in Figure 3.D.2 and it is used to classify EU countries in three terciles, as indicated.

Figure 3.D.2: EU countries ranked by cotton exports to the United States



Source: Calculated based on UN Comtrade

Notes: Share of US exports in total exports of cotton-containing textiles and garments for 2017 and 2018. Austria is excluded.

Appendix 3.E Additional results

Table 3.E.1: Direct impact of the Cotton Ban: PPML estimator

	(1) Exports	(2) Exports
Ban^{US}	-0.105***	
	(0.031)	
$Ban^{US} \times first$		-0.052
		(0.037)
$Ban^{US} \times second$		-0.092*
		(0.041)
$Ban^{US} \times UFLPA$		-0.224***
		(0.046)
Observations	3,647,662	3,647,662
Destination-Product FE	✓	✓
Product-Month FE	✓	✓
Destination-Month FE	✓	✓

Note: Results estimated with Poisson Pseudo Maximum Likelihood, using the level of exports as dependent variable. Ban^{US} is an indicator that equals one if a commodity contains cotton, the export destination is the United States, and the Cotton Ban is in effect (from December 2020 onward). $first$ is an interaction dummy for the period between December 2020 and November 2021, $second$ for the period between December 2021 and June 2022, and $UFLPA$ for the period from July 2022 onward. Standard errors, clustered at product-destination level, are reported in parentheses with ***, **, and * represent significance at the 1%, 5%, and 10% level, respectively.

Table 3.E.2: Direct impact of the Cotton Ban: Replication with China's Customs data

	(1)	(2)	(3)	(4)
	Province-level data (*podt*)		National (aggregated) data (*pdt*)	
	log(Exports	Export dummy	log(Exports)	Export dummy
Ban^{US}	-0.169***	-0.015***	-0.296***	-0.002
	(0.030)	(0.002)	(0.062)	(0.010)
Observations	4,750,327	139,612,032	1,469,040	4,814,208
Product-province-destination FE	✓	✓		
Product-destination FE			✓	✓
Destination-Month FE	✓	✓	✓	✓
Product-province-month FE	✓	✓		
Product-month FE			✓	✓

Note: Results are based on the provincial-level data from China's Customs. In columns (1) and (2), the units of observations are product-province-destination-months and standard error are clustered at the product-province-destination level. In columns (3) and (4), the data is aggregated to the national level across provinces and it uses the same triple-difference estimator of the benchmark analysis. Standard errors are clustered at the product-destination level. Clustered standard errors are reported in parentheses with ∗∗∗, ∗∗, and ∗ represent significance at the 1%, 5%, and 10% level, respectively.

Table 3.E.3: Direct impact of the Cotton Ban according to countries' input sourcing from China: Robustness check using 27 origin countries

	(1)	(2)	(3)	(4)
	Destination-month FE		Product-destination-month FE	
	log(Exports)	Export dummy	log(Exports)	Export dummy
Ban^{US}_{pdt} × China ($\equiv Ban^{US}$)	-0.379***	-0.049***	-0.475***	-0.042***
	(0.039)	(0.007)	(0.049)	(0.008)
Ban^{US}_{pdt} × Q4 China sourcing	0.003	-0.006**	-0.099**	0.000
	(0.024)	(0.003)	(0.041)	(0.004)
Ban^{US}_{pdt} × Q3 China sourcing	0.014	-0.009***	-0.047	-0.002
	(0.044)	(0.003)	(0.057)	(0.004)
Ban^{US}_{pdt} × Q2 China sourcing	0.125***	0.002	0.019	0.004
	(0.028)	(0.003)	(0.044)	(0.004)
Ban^{US}_{pdt} × Q1 China sourcing	0.081**	-0.007***		
	(0.031)	(0.002)		
Observations	11,326,141	129,800,448	10,577,972	129,800,448
Product-origin-destination FE	✓	✓	✓	✓
Product-origin-month FE	✓	✓	✓	✓
Destination-month FE	✓	✓		
Product-destination-month FE			✓	✓

Note: Estimation results for equation (3.3) with third countries as origins (exporters); the units of observation are product-origin-destination-months; the sample period ranges from January 2019 to December 2022. Twenty-seven origin countries are China, Belgium, Bulgaria, Cambodia, Canada, Colombia, Dominican Republic, Egypt, El Salvador, France, Germany, Guatemala, India, Indonesia, Italy, Japan, Malaysia, Mauritius, Mexico, Nicaragua, Pakistan, Philippines, Portugal, South Korea, Turkey, Thailand, and Vietnam. This includes six more countries than in Table 3.5, now covering all top-30 exporters to the United States that report a complete monthly trade data for the full sample period. The first interaction term is a dummy for direct exports from China. The next three interactions are dummies for the quartile each origin country (apart from China) is assigned to based on its total imports of raw cotton, cotton yarns, and cotton fabrics from China over the 2017–2018 period. The results in columns (3) and (4) exclude raw cotton imports from the sample. Note that the average effect is absorbed by the full set of interaction fixed effects, and $Q1$ is omitted as a reference group. Standard errors in parentheses are clustered at the product-origin level, with ***, **, and * representing significance at the 1%, 5%, and 10% level.

Table 3.E.4: Direct impact of the Cotton Ban according to countries' input sourcing from China: Triple-difference analysis using only US imports

	(1)	(2)	(3)	(4)
	All		Excluding Raw Materials	
	log(Exports)	Export dummy	log(Exports)	Export dummy
Ban^{US}_{pt} × China ($\equiv Ban^{US}$)	-0.243**	-0.037***	-0.245**	-0.033***
	(0.121)	(0.008)	(0.121)	(0.008)
Ban^{US}_{pt} × Q4 China sourcing	0.049	-0.007***	0.049	-0.007***
	(0.116)	(0.001)	(0.116)	(0.001)
Ban^{US}_{pt} × Q3 China sourcing	0.036	-0.002*	0.036	-0.002**
	(0.117)	(0.001)	(0.117)	(0.001)
Ban^{US}_{pt} × Q2 China sourcing	0.036	-0.000	0.036	-0.001
	(0.124)	(0.001)	(0.124)	(0.001)
Observations	611,433	7,124,544	608,390	6,865,632
Product-origin FE	✓	✓	✓	✓
Product-month FE	✓	✓	✓	✓
Origin-month FE	✓	✓	✓	✓

Note: Estimation results for equation (3.3) with third countries as origins (exporters); the units of observation are US imports by product-origin-months; the sample period ranges from January 2019 to December 2022. Ban^{US}_{pm} is a dummy taking a value of one if a commodity contains cotton and was exported (from any origin country) to the United States after the implementation of the region-wide cotton ban in December 2020. The first interaction term is a dummy for direct exports from China. The next three interactions are dummies for the quartile each origin country (185 countries apart from China) is assigned to based on its total imports cotton products from China over the 2017–2018 period. The results in columns (3) and (4) exclude raw cotton imports from the sample. Note that the average effect is absorbed by the full set of interaction fixed effects, and $Q1$ is omitted as a reference group. Standard errors in parentheses are clustered at the product-origin level, with ∗∗∗, ∗∗, and ∗ representing significance at the 1%, 5%, and 10% level.

Chapter 4

Mining the Energy Transition: Chinese Critical Mineral Investment and Protest in sub-Saharan Africa

China has become a major investor in critical mineral extraction worldwide. This paper examines how Chinese-owned mining projects affect local protest dynamics across fifteen sub-Saharan African countries from 2004 to 2023. Drawing on a novel geocoded panel of Chinese mining investments and a triple-differences identification strategy, the analysis finds that the opening of Chinese-owned mines increases the likelihood of local protest by 17 percentage points and raises the number of protest events by 57% relative to comparable non-Chinese operations. The effects are substantially weaker in countries with stronger democratic institutions and higher-quality governance. I find no evidence that Chinese mining increases corruption or reduces local economic well-being. Instead, satellite-based measures show that Chinese operations increase air pollution and reduce vegetation cover, indicating that environmental degradation is an important consequence of these investments. Chinese-owned mining is also associated with a higher prevalence of pollution-related illnesses and more environmentally motivated protests. Taken together, the findings suggest that environmental degradation is a key mechanism linking Chinese mining investment to local protest.

4.1 Introduction

Rising global demand for technologies underpinning the energy transition has triggered a surge in investment in critical mineral extraction.[1] As part of its strategy to support domestic clean energy industries and secure access to strategic mineral inputs, China has adopted policies and mobilized state-backed financing to facilitate overseas investment in the sector. This policy-supported expansion has been particularly pronounced in sub-Saharan Africa. From almost no presence in the early 2000s, Chinese-owned and joint-venture projects now account for roughly one-fifth of all critical-mineral mines in the region.[2] The growing presence of Chinese mining companies has attracted increasing international scrutiny. A growing body of reports documents tensions between Chinese operators and host communities, often linked to environmental degradation, corruption, and limited economic benefits for local workers (e.g., RAID, 2021; Babatunde et al., 2025). In some cases, these grievances have culminated in protests against Chinese mining operations. Yet systematic empirical evidence on the effects of Chinese mining investment on protest in host communities remains limited.

There are reasons to expect Chinese-operated mines to generate stronger negative externalities and thus greater resistance than mines run by other firms, even though the adverse effects brought by mining activities are not unique to the Chinese ownership. China's home-country regulations governing overseas mining activities are largely non-binding (Links et al., 2021). Coupled with weak social pressure in China to enforce higher standards operating abroad (Sändig et al., 2024), this reduces firms' incentives to exceed often porous host country regulatory requirements. Moreover, the management culture of Chinese mining firms often emphasizes primary on production targets and involves limited engagement with local stakeholders (Lee, 2018). As a result, when weak Chinese oversight meets weak local enforcement, Chinese mining projects are more susceptible to lower environmental and social safeguards. This increases local exposure to the costs of extraction, exacerbates grievances, and raises the likelihood of protest.

This paper investigates whether Chinese outward foreign direct investment (OFDI) in critical mineral mining leads to increased local protest in fifteen sub-Saharan

[1] There is no single, universally accepted definition of a critical mineral, as what qualifies as "critical" depends on a country's strategic priorities and the vulnerability of its supply chains. Throughout this paper, I define critical minerals as non-fuel minerals and metals that constitute essential inputs into energy-transition technologies.

[2] Author's calculations based on hand-collected project-level data and information from the S&P SNL Metals & Mining database.

African countries. The analysis draws on a newly constructed panel dataset combining geocoded protest data from the Armed Conflict Location and Event Data Project (ACLED) and a unique project-level dataset on Chinese investments in critical mineral mines worldwide. The Chinese investment dataset, constructed from corporate press releases, regulatory filings, and industry reports, identifies 74 Chinese-owned operating critical mineral extraction projects in sub-Saharan Africa and records each project's location, acquisition year, and development stage. To identify the effect of Chinese ownership, we integrate the panel with the S&P SNL Metals & Mining database and construct a comparison group of otherwise similar non-Chinese mines.

The empirical strategy isolates the effect of Chinese ownership by utilizing a triple-differences design. Unit of measurement is sub-national grid cell of $0.5° \times 0.5°$(approximately 55km $\times$ 55km at the equator). The sample consists of 3,057 grid cells across fifteen sub-Saharan African countries. Identification compares changes in local protest activity before and after the onset of Chinese mining operations relative to changes in cells with non-Chinese mining, while netting out trends in cells without any mining activity. To mitigate potential confounding factors, the specification includes cell and country-year fixed effects, as well as controls for annual local weather shocks. Event-study shows no evidence of differential pre-trends in protest before Chinese mines commence, supporting the validity of the parallel trends assumption. Potential selection bias from the non-random placement of Chinese projects also appears limited.

The results provide significant evidence that Chinese critical mineral extraction exerts significantly stronger impacts on local protest activity than comparable projects operated by non-Chinese firms. Particularly, point estimates on the differential effect of Chinese ownership are positive and statistically significant at the 1% level. The opening of a Chinese-operated mine increases the probability of local protest by 17 percentage points and raises the number of protests by 57%, relative to comparable non-Chinese projects. The results are robust across a variety of sensitivity analyses, including the use of heterogeneity-robust estimators, alternative model specifications, different samples, and alternative treatments of standard errors. In addition, permutation tests confirm that the estimated effects are highly unlikely to have arisen by chance.

Local governance can play a crucial role in mitigating the adverse effects of resource extraction (Hodler et al., 2023a). In particular, when Chinese mining firms are more adapted to local regulatory environments, stronger governance in

the host country can more effectively constrain their practices and reduce potential negative impacts. Using country-level democracy scores from the Polity V Project and governance measures from the World Governance Indicators, I show that areas located in countries with higher levels of democracy, stronger control of corruption, and greater regulatory quality experience significantly smaller increases in protest following the opening of a Chinese-operated mine. These results suggest that strong institutions can mitigate the disruptive effects of Chinese investment by enhancing transparency, regulatory enforcement, and responsiveness to community concerns.

Lastly, I investigate the mechanisms underlying the protest-amplifying effect of Chinese mining investment, combining cell-level satellite measures with individual-level data from Afrobarometer and the Demographic and Health Surveys. I consider three potential channels: environmental degradation, corruption, and local economic well-being. The evidence points consistently to environmental degradation. The opening of a Chinese-operated mine increases local $PM_{2.5}$ concentrations, reduces vegetation cover as measured by NDVI, and accelerates deforestation. These patterns accord with reports of pollution and ecological damage surrounding Chinese mining operations (e.g. Associated Press, 2025). Children living near Chinese mining sites also experience worse health outcomes, including higher rates of underweight, cough, diarrhea, and fever, consistent with greater exposure to environmental contamination. Moreover, Chinese mining is associated with more environment-related protests, and its effect on protest is stronger in areas facing greater environmental risk. Taken together, these findings identify environmental degradation as a key channel through which Chinese mining investment generates local resistance. By contrast, I find little evidence that Chinese-operated mines systematically increase perceived corruption or reduce household economic well-being.

First, this paper contributes to the emerging literature on geoeconomics. Recent studies examine the consequences of geoeconomic instruments such as international sanctions (Ahn and Ludema, 2020; Maystadt et al., 2024), investment restrictions (Frattaroli, 2020; Eichenauer and Wang, 2024), and foreign aid (Nunn and Qian, 2014; Dreher et al., 2022). By contrast, theoretical and empirical evidence remains limited on the consequences of state-directed capital flows into strategically important sectors (Mohr and Trebesch, 2025). This paper provides new evidence that China's policy-supported investment in critical-mineral extraction can generate substantial social instability in host communities. More broadly, the findings show that industrial policies motivated by geopolitical and supply-chain considerations can have political-economy consequences not only for the countries adopting

them and their strategic rivals, but also for the developing countries that receive the resulting investments.

Second, the paper contributes to the literature on the local consequences of natural-resource extraction. Existing research shows that mining affects the environment (Von der Goltz and Barnwal, 2019; Girard et al., 2025), local economic activity (Aragón and Rud, 2013; Bazillier and Girard, 2020), human capital (Mejía, 2020; Agüero et al., 2021), health (Benshaul-Tolonen, 2019; Von der Goltz and Barnwal, 2019), and conflict (Maystadt et al., 2014; Stoop et al., 2019; Hodler et al., 2023b). Much of the subnational evidence, however, is drawn from single-country settings. Building on cross-country studies showing that mineral-price shocks and the opening of new mines can intensify local conflict (Berman et al., 2017; Christensen, 2019), this paper shows that mine ownership is an important source of heterogeneity: Chinese-operated mines generate significantly more protest than comparable non-Chinese mines. This finding complements earlier studies that document limited differences in mining outcomes across ownership types (Christensen, 2019; Bezzola et al., 2022). The results also speak directly to concerns that rising demand for transition minerals may generate a "critical-mineral resource curse" in developing economies with weak institutions (Arezki and van der Ploeg, 2025; Couttenier and Rohner, 2025; Vanden Eynde and Vargas, 2025). In line with this concern, I show that large-scale critical-mineral extraction can increase local social instability, while stronger democratic institutions and higher-quality governance attenuate these adverse effects.

Third, the paper contributes to the literature on China's global economic engagement. Existing research has largely examined China's international influence through trade and foreign aid. Rising import competition from China has disrupted labor markets and affected political and social outcomes in importing countries (e.g. Autor et al., 2013), while Chinese aid has been linked to a range of economic and political outcomes in recipient countries (e.g. Dreher et al., 2021; Iacoella et al., 2021). Evidence on the local consequences of Chinese outward foreign direct investment remains comparatively limited. Important exceptions include Kim-Leffingwell et al. (2024), who links Chinese OFDI to anti-China protest at the country level, and Wang et al. (2022) and McCauley et al. (2022), who examine how Chinese-financed projects affect public attitudes and local politics. Leveraging high-resolution spatial and temporal information on mine locations, ownership, and operational timelines, this paper provides new causal evidence that Chinese mining investment increases local unrest. It further identifies environmental degradation as the primary mech-

anism, contrasting with earlier studies that emphasize employment displacement as a principal adverse consequence of Chinese OFDI (Wegenast et al., 2019; Yang, 2022).

The remainder of the paper is organized as follows. Section 4.2 provides background information on Chinese outward investment in critical mineral mining and its relationship with local protest. Section 4.3 describes the data used in the analysis. Section 4.4 outlines the empirical strategy. Section 4.5 reports the main results, robustness checks, and heterogeneity analyses. Section 4.6 examines the mechanisms. Finally, Section 4.7 concludes.

4.2 Background

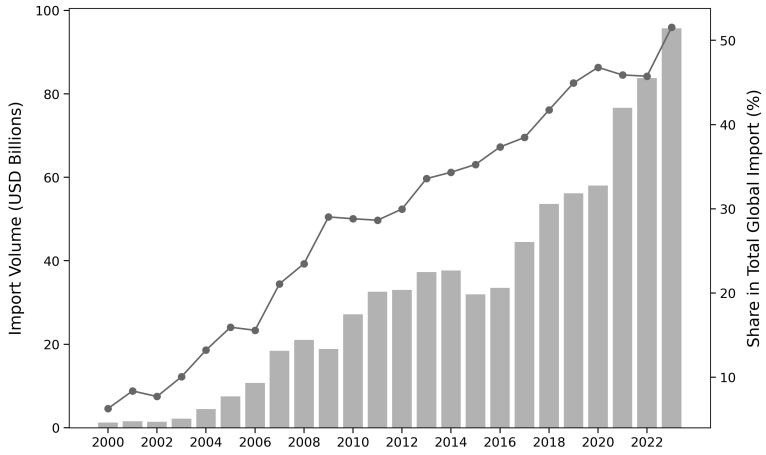
4.2.1 Chinese Investment in Transition Minerals in sub-Saharan Africa

China has established a dominant position in the midstream and downstream segments of the global energy transition value chain. Ranging from mineral refinement and green technology equipment manufacturing, Chinese firms have already dominated a major share in global production.[3] However, these sectors are highly mineral-intensive, relying heavily on inputs such as copper, cobalt, lithium, and zinc (Kim et al., 2021). China's domestic supply cannot meet this rising demand and therefore increasingly relies on external sources. Accordingly, as shown in Figure 4.1, the total value of China's critical mineral imports increased from approximately USD 1 billion in 2000 to nearly USD 96 billion in 2023, accounting nearly half of global imports. This growing reliance on external supply poses considerable supply-chain risks for China (Yuan et al., 2024).

To strengthen its access to upstream resources, Beijing has actively promoted overseas investment in mineral extraction (International Energy Agency, 2025). Since the early 2000s, Chinese state policy banks and commercial banks have extended resource-backed loans to support the acquisition and development of foreign mining assets (e.g., Marysse and Geenen, 2009; Bräutigam and Gallagher, 2014).

[3]China refines roughly one-third of the world's nickel, over half of its lithium and cobalt, and nearly all of its rare earth elements (Kim et al., 2021). Moreover, China produces more than 70% of global solar photovoltaic (PV) modules, 60% of wind turbines, and approximately 60% of electric vehicles. See the IEA's Solar PV Global Supply Chains report (https://www.iea.org/reports/solar-pv-global-supply-chains/executive-summary), Reuters (https://www.reuters.com/business/energy/chinas-dominance-wind-turbine-manufacturing-2024-04-10/), and CNN (May 2, 2025).

Figure 4.1: Chinese Import of Critical Minerals



Note: Own calculations based on information from the BACI dataset (Gaulier and Zignago, 2010). The line represents the Chinese share in the total world's import of the critical minerals.

This approach was institutionalized in the National Plan for Mineral Resources (2016–2020), which explicitly encouraged investment in critical minerals both domestically and abroad (Zhou et al., 2025). Consistent with this mandate, Escobar et al. (2025) estimate that Chinese state-owned banks and official agencies provided roughly USD 57 billion in loans and aid between 2000 and 2021 to facilitate the global expansion of Chinese mining enterprises. These policy and financial supports have enabled Chinese firms to rapidly expand their critical mineral portfolios worldwide.

Consequently, China's OFDI in mining has expanded sharply. According to data from the National Bureau of Statistics of China, OFDI stocks in the mining sector increased nearly eightfold between 2008 and 2023, from 23 USD billion to 193 USD billion (see Figure 4.2).[4] Moreover, based on the author's calculations, Chinese firms were involved in 206 critical mineral projects worldwide by 2023, up from only 9 in 2003 (see Figure 4.3).[5]

Sub-Saharan Africa has emerged as a key destination for these investments, owing to the region's abundant reserves of critical minerals. By 2023, 102 critical mineral mines, which are either under construction or in operation, in the region had

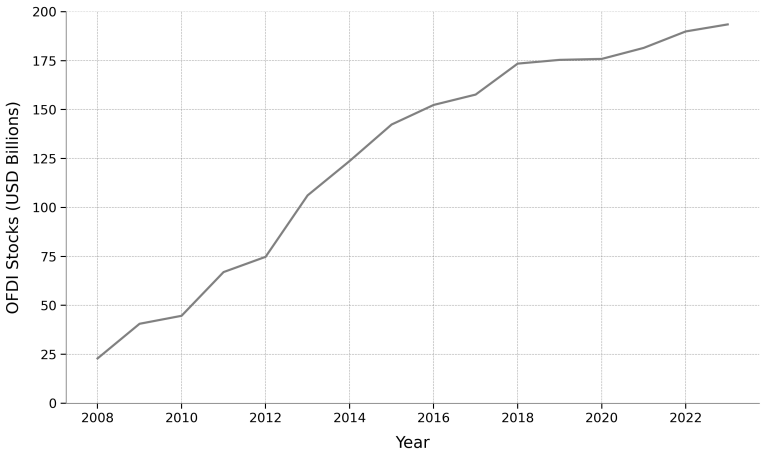
[4]Data from National Bureau of Statistics of China (2025) reflect aggregate mining investment, which may include non-critical minerals; Figure 4.2 should therefore be interpreted with caution.

[5]Author's calculations include only extraction projects that are under construction or in operation.

Chinese ownership, accounting for roughly one-fifth of all such mines.[6] Of these, 84 were operational across sixteen countries, with the Democratic Republic of the Congo (hereafter DRC), Guinea, South Africa, and Zambia are the main recipients (Figure 4.A.3).[7]

Chinese state-owned enterprises (SOEs) play a central role in this overseas mining expansion. Of the Chinese-owned projects in sub-Saharan Africa, about 66 are owned by SOEs, including 37 by Chinese central SOEs.

Figure 4.2: Chinese Outward FDI Stocks in Mining Sector



Note: Own calculations based on information from the National Bureau of Statistics of China (National Bureau of Statistics of China, 2025). The official source does not provide a value for 2022. I estimate the 2022 value by adding the 2022 OFDI flow reported by the NBS to the 2021 stock value.

4.2.2 Chinese Operation and Protest in sub-Saharan Africa

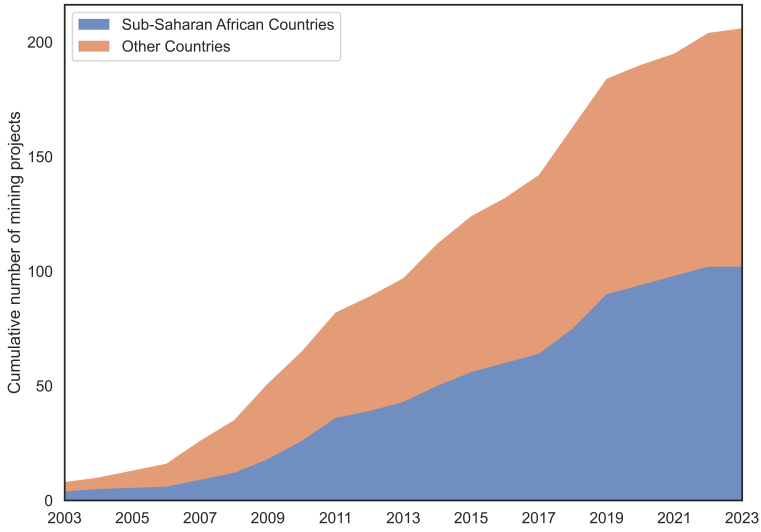
China has long framed its engagement with sub-Saharan Africa as a mutually beneficial partnership. At successive Forum on China–Africa Cooperation (FOCAC) summits, Chinese leaders have emphasized the "win–win" nature of China–Africa economic cooperation.[8] Despite this official discourse, evidence on the local effects of

[6] Author's calculations based on S&P SNL Metals and Mining data, focusing on extraction projects either under construction or in operation.

[7] The regression analysis focuses on 74 mines across fifteen countries that were Chinese-owned and operational prior to 2023. This restriction ensures consistency with the use of a one-year lagged treatment variable.

[8] Hu Jintao's 2012 FOCAC speech is available at https://www.focac.org/eng/zywx_1/zyjh/201207/t20120719_7933601.htm . President Xi's keynote speeches at FOCAC (Johannesburg 2015; Beijing 2018) and the China–Africa Leaders' Dialogue in 2021 reiterate this narrative; see

Figure 4.3: Evolution of Chinese-owned Critical Mineral Mining Operations



Note: This figure is compiled by the author.

Chinese mining investment is mixed. While some studies document developmental benefits, such as infrastructure provision, employment, and economic growth (e.g., Brautigam et al., 2019; de Freitas, 2023; Egbetokun, 2024), others link Chinese-operated projects to rising social unrest in host communities (RAID, 2009; RAID, 2021; Sabbe, 2022).

These heightened tensions between Chinese firms and local communities are closely linked to the dual regulatory gap governing Chinese overseas mining. On the home-country side, China's economic engagement with sub-Saharan Africa is shaped by the principle of non-interference, which largely delegates environmental and social oversight to host governments. During the period under study, China lacked binding laws requiring overseas extractive firms to comply with environmental, social, or community-consultation standards (Inclusive Development International, 2022); instead, firms operated under non-binding guidelines with limited enforcement (Gallagher and Qi, 2018; Links et al., 2021; Chen, 2025). On the host-country side, regulatory agencies often lack the capacity or political autonomy to effectively enforce environmental protection, labor standards, and compensation

http://en.cabc.org.cn/?c=policys&a=view&id=33, http://www.xinhuanet.com/english/2018-09/03/c_129946189.htm , and http://www.focac.org.cn/eng/zywx_1/zyjh/202201/t20220124_10632446.htm.

requirements. This creates an institutional gap in which Chinese firms face weak accountability from both the sending and receiving countries. Limited scrutiny by Chinese media and civil society (Mol, 2011; Sändig et al., 2024), together with management practices that prioritize production targets over sustained community engagement (Lee, 2018), further reduces incentives to internalize local costs. As a result, Chinese-owned mines may be more susceptible than other mines to operating with inadequate environmental and social safeguards, exposing local communities to uncompensated harms and increasing the likelihood of social unrest.

Chinese mining activities have frequently been associated with weak environmental management, pollution incidents, and contamination risks that can directly affect local livelihoods and health (Shinn, 2016; Sun and Tang, 2016). These environmental harms may be compounded by weak governance around Chinese overseas mining, including limited enforcement of foreign bribery rules and opaque contracting practices (Ferguson, 2017; Sun et al., 2017). Such practices can reinforce the corruption-enhancing effects of mining and weaken institutional trust (Ross, 2015; Knutsen et al., 2017). Finally, limited transparency around mining contracts and expected local benefits may inflate community expectations and exacerbate economic grievances when promised gains fail to materialize (Christensen, 2019; Bennon and Fukuyama, 2022).

Although anecdotal evidence and case studies suggest that Chinese-operated mines generate greater social tensions and more frequent protest, systematic empirical evidence remains limited. This paper addresses this gap by examining whether Chinese-owned critical mineral mines amplify protest relative to otherwise similar non-Chinese mines and to regions without mining activity.

4.3 Data

The dataset is a balanced panel of 3,057 subnational grid cells, each spanning $0.5^\circ \times 0.5^\circ$ latitude and longitude (approximately 55×55 km at the equator), across 15 sub-Saharan African countries, between 2004 and 2023.[9] All countries host at least one operating Chinese-owned transition mineral mines as of the sample period.[10] Rather than administrative boundaries, fixed grid cells are the measurement unit to avoid endogeneity arising from the alignment of political borders with conflict

[9] Administrative boundaries are from the geoBoundaries Global Database of Political Administrative Boundaries (Runfola et al., 2020).

[10] Republic of the Congo is excluded from the analysis because it does not host any operating non-Chinese mines.

dynamics (Berman et al., 2017). Figure 4.4 shows the sample of grid cells and the spatial distributions of local protests and critical mineral mines.

4.3.1 Conflict Data

Protest events are measured using data from the ACLED. It compiles detailed information on conflict events from diverse sources, including media reports, NGOs, international organizations, and local actors. A key advantage of ACLED is that it records a wide spectrum of events without imposing a fatality threshold, thereby including incidents that do not involve high levels of violence.

The analysis is restricted to protest and riot events with the highest spatial precision in ACLED. Across the sample countries, 36,033 such events are recorded during the study period. Two annual cell-level measures of protest activity are constructed: (1) a binary indicator for protest incidence and (2) the count of protest events per cell–year. The main specification applies the inverse hyperbolic sine (IHS) transformation of protest counts to address skewness and zeros, with alternative functional forms examined in robustness checks.

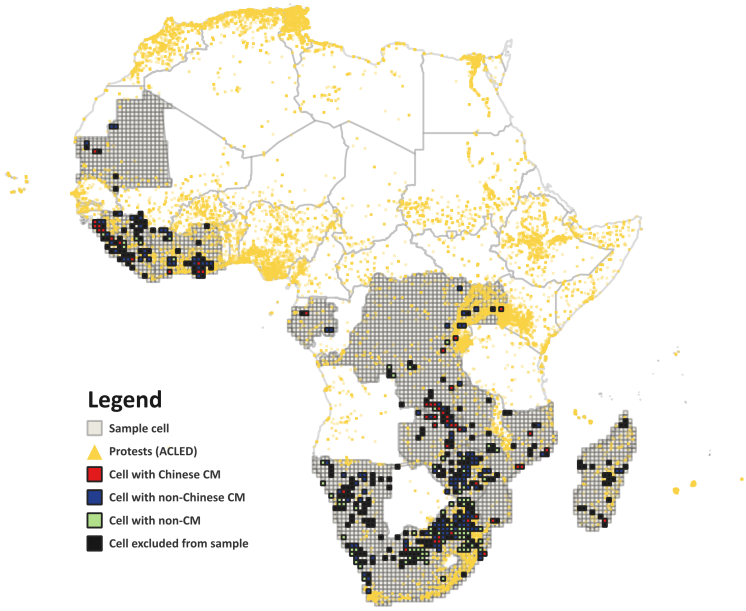
4.3.2 Chinese Outward Investment Data

The treatment is the presence of critical mineral mines owned by Chinese company. To construct this variable, I assemble a novel dataset of Chinese outward investment projects in the global mining sector. This dataset is built in three steps.[11] First, I compile a comprehensive list of 695 China-based, resources-related companies. These companies include firms listed on Chinese and Hong Kong stock exchanges and large-scale private firms identified through government and industry reports. A notable limitation of this approach is that it likely underrepresents small-scale or artisanal mining ventures involving Chinese firms, for which reliable public records are scarce.

Second, for each firm on the list, investment projects are identified by reviewing company's press releases, periodic reports, industry publications, and reports issued by governments and NGOs. Only 131 out of 695 Chinese companies own at least one critical mineral mine worldwide. Lastly, after identifying critical mineral extraction projects owned by Chinese companies, I collect supplement information, including geographic coordinates, development status, the year of first Chinese ownership, the

[11] Appendix 4.A provides details on the data construction process.

Figure 4.4: Protest Events and Critical Mineral Mines



Note: This map illustrates the sample grid cells, instances of social conflict, and the grid cells containing critical mineral mines. The analysis focuses on fifteen sub-Saharan African countries that host at least one Chinese mine. Protest events are calculated based on data from ACLED. Information on Chinese outward investment projects in the critical minerals sector is derived from the author's self-collected data, while data on other critical mineral projects and non-critical mineral mining projects come from S&P SNL Metals and Mining database. Cells not included in the sample include cells with non critical mineral mines and cells with mining activities mentioned in Maus et al. (2022) but not in my dataset and S&P SNL Metals and Mining database.

year operations commenced, and ownership structure, manually. To ensure accurate information, each project entry is confirmed by two external sources.

The dataset used in this study contains 74 operating Chinese-owned mines located in sub-Saharan Africa as of 2023. Chinese company owns at least 10% shares in the ownership of these mines. Average resource size is about 2.5 million tons.[12] This involves producing 18 types of critical minerals.[13] Figure 4.5 shows the spatial and temporal evolution of Chinese investments from 2004 to 2023. Most projects

[12]Based on the information from 50 mines which have reported the total resource size.
[13]Table 4.B.2 shows all types of critical minerals considered in this analysis.

are launched after 2010, with a marked acceleration after 2015. The DRC, Guinea, South Africa, and Zambia hosted the largest shares of these projects.

Each Chinese project is mapped to the sample grid cells. In total, 48 cells (1.6% of total) ever host at least one active Chinese-owned critical mineral mine. In cases where a cell contains multiple Chinese projects, the year of Chinese ownership and the start of operation are recorded as the earliest year among all projects within that cell. Table 4.B.2 lists the number of cells with Chinese projects by mineral type. Nearly half of these cells involve copper and cobalt, reflecting China's growing demand for battery and electric vehicle metals.

4.3.3 Other Mining Operation Data

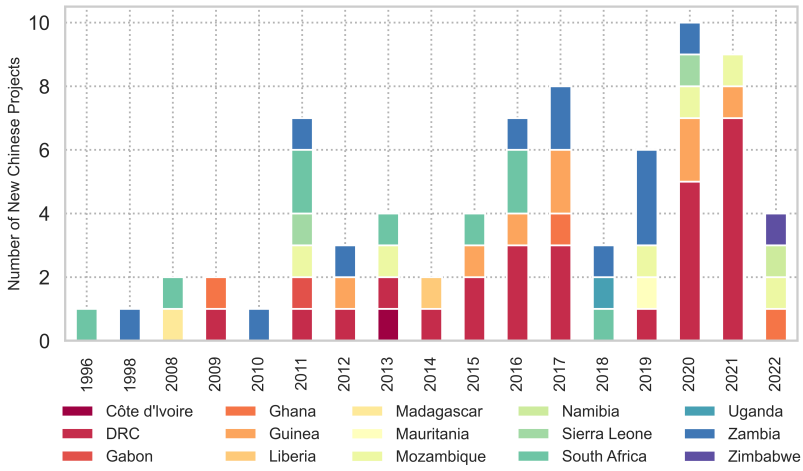
To compare the effects of Chinese-owned mines on local unrest with those of non-Chinese-owned mines, I measure the presence of non-Chinese mines using the S&P SNL Metals and Mining database. This dataset has been widely used in the literature to study the local effects of industrial-scale mining operations (e.g., Berman et al., 2017; Knutsen et al., 2017). A potential concern is that the Chinese mining investment dataset may differ from the SNL database in reporting standards and project coverage. As a result, cells hosting Chinese mines and cells hosting only non-Chinese mines may differ non-randomly because of differences in data construction rather than differences in mine ownership. To mitigate this concern, I harmonize the two datasets using a common set of inclusion rules. Specifically, I restrict the sample to: (1) mines located within the same fifteen countries; (2) mining projects involving critical minerals; (3) projects that are either under construction or in operation; (4) projects with precise coordinates; and (5) projects for which the year of operation can be manually verified using a consistent procedure. This yields 382 operating critical-mineral mines without Chinese ownership. In the final sample, 132 cells, or 4.3% of all cells, host at least one active non-Chinese critical-mineral mine during the sample period.[14]

25 cells overlap across the two datasets. These cells are classified as cells with Chinese investment.[15] As a robustness check, I restrict the analysis to projects

[14]To reduce confounding from other mining activities, I exclude 79 grid cells hosting mines that produce minerals not classified as green-transition minerals, such as coal and diamonds. I further exclude 224 grid cells identified as mining areas by Maus et al. (2022) but not recorded in either of the two project-level datasets.

[15]Consistent with the definition used in the Chinese investment dataset, mining properties recorded in the SNL database are classified as Chinese-owned projects if Chinese firms hold more than 10% of their shares. An additional 13 cells are recorded in the SNL database as hosting Chinese-owned mines but are not recorded in the Chinese mining investment dataset. After trac-

Figure 4.5: Number of Chinese Operating Critical Mineral Mines, by sub-Saharan African Country



*Note:*Calculations based on the author's dataset. Each year on the x-axis corresponds to the onset of ownership and operation of critical mineral extraction projects by Chinese firms.

recorded in the SNL database only to examine whether the main results are sensitive to differences in project coverage between the two datasets.

4.3.4 Other Data

Climate controls Given the well-established relationship between local climatic conditions and conflict incidence (Hsiang et al., 2013; Burke et al., 2015b; Burke et al., 2024), I include annual temperature and precipitation anomalies to control for local weather shocks. Temperature data come from the CRU TS 4.08 dataset (Harris et al., 2020) and precipitation data from CHIRPS v3.0 (Funk et al., 2015). Following Bertinelli et al. (2025), cell-year temperature and precipitation anomalies are computed by subtracting long-term means from annual averages and standardizing the deviations by their respective long-term standard deviations.

Socioeconomic controls and mineral prices Robustness checks additionally control for lagged nighttime light density (Li et al., 2020), population density from LandScan (Lebakula et al., 2025), and the count of Chinese foreign aid projects in

ing the ownership information in the SNL databse, I find that these mining properties are owned by private Chinese companies for which firm identities cannot be verified. They are therefore outside the scope of the Chinese mining investment dataset.

each cell from AidData v3.0 (Goodman et al., 2024). Moreover, I investigate the nexus between exogenous price shocks and protest using the real commodity price data from the World Bank Commodities Price database.

Variables in heterogeneity analyses and mechanism sections Several datasets are employed in the heterogeneity and mechanism analyses. Definitions of the variables and their corresponding sources are reported in Table 4.D.1.

4.3.5 Summary Statistics

Table 4.E.1 summarizes the key variables used in the baseline analysis.[16] Protest activity is rare overall. The mean probability of any protest in a cell-year is 0.08, with an average of 0.46 protests and 0.06 fatalities per cell-year. However, protest activity is significantly higher and deadlier in areas with critical mineral extraction projects than in non-mining areas. Within mining regions, cells with Chinese-owned projects exhibit an even greater likelihood and intensity of protest, suggesting a stronger link between Chinese investments and local unrest.

Table 4.E.2 compares selected characteristics of the cells prior to hosting greenfield Chinese critical mineral mines and hosting non-Chinese mines. Panel (a) shows that time-invariant characteristics differ across groups prior to the commencement of mine operations, while panel (b) indicates that time-varying pre-treatment covariates are generally well balanced.[17] I primarily address imbalances in time-invariant characteristics through grid cell fixed effects. Remaining concerns about time-invariant and time-varying cell characteristics that are not fully addressed by fixed effects and climate-anomaly controls are mitigated through additional baseline-characteristic-by-year fixed effects and time-varying cell-level covariates in the sensitivity tests.

Additional summary statistics for the variables used in the baseline and mechanism analyses are reported in Tables 4.E.3 and 4.E.4, respectively.

[16] Variable definitions are provided in Table 4.D.1.

[17] Following the discussions in Imbens and Wooldridge (2009), t-statistics are sensitive to sample size and may therefore overstate imbalance between groups. Instead, the normalized difference between cells with greenfield Chinese mines and those with only non-Chinese-owned mines is calculated to assess covariate balance. It is defined as

$$ND = \frac{\bar{X}_T - \bar{X}_C}{\sqrt{\frac{s_T^2 + s_C^2}{2}}}$$

where, as a general rule of thumb, $|ND| < 0.25$ is regarded as indicating of covariate balance between groups.

4.4 Empirical Strategy

This study examines the impact of Chinese-owned critical mineral mining operations on local protest in sub-Saharan Africa using a triple-differences design with staggered treatment timing. The identification strategy exploits variation across three dimensions: (i) time, before and after mine openings; (ii) space, between grid cells with and without mining activity; and (iii) ownership, comparing Chinese-owned and non-Chinese mining operations. The baseline specification is given by:

$$Protest_{it} = \alpha + \delta_1 CM_{i,t-1} + \delta_2 CM^{\text{China}}_{i,t-1} + \beta X_{it} + \gamma_i + \gamma_{ct} + \mu_{it} \tag{4.1}$$

where i and t denote cell and year correspondingly. Unit of measurement is at the cell-year level. The outcome variable $Protest_{it}$ is either probability of protest or IHS transformed total number of protests. The parameters are estimated with linear probability model for protest probability and OLS for protest intensity. In the baseline, I retain a relatively larger baseline sample to preserve statistical power and improve the stability of heterogeneous-treatment-effect estimators.

Two variables of interest are central to the analysis. The first, $CM_{i,t-1}$, is a binary indicator equal to one if cell i hosts at least one active critical mineral mine, regardless of ownership, in year $t-1$. To allow time for mining activity to affect protest outcomes, particularly for projects that commence operations late in the year, I use one-year lagged treatment indicators.[18] The corresponding coefficient, δ_1, captures the average effect of the onset of non-Chinese critical mineral mining on local protest, relative to grid cells without active mining.

The second variable, $CM^{\text{China}}_{i,t-1}$, is an interaction term equal to one if, in year $t-1$, cell i: (1) hosts at least one active critical mineral mine (i.e. $CM_{i,t-1} = 1$) and (2) at least one of these operations is owned and operated by a Chinese firm.[19] Thus, $CM^{\text{China}}_{i,t-1}$ indicates the presence of an active Chinese-owned critical mineral operation in a cell. Its coefficient, δ_2, captures the additional effect of Chinese ownership beyond the average impact of any critical mineral extraction. The total effect of Chinese-operated mining on local protest, relative to non-mining cells, is

[18] Using contemporaneous value instead of lagged one yields qualitatively similar estimates.

[19] If a Chinese company acquires a mining concession and develops a new project in a cell where other non-Chinese owned critical mineral extraction is already underway, the operational start year of the Chinese-owned project may differ from the initial year of mining activity in the cell. In such case, $CM_{i,t-1}$ reflects the earliest onset of mining, while $CM^{\text{China}}_{i,t-1}$ identifies the year Chinese operations begin.

therefore given by $\delta_1 + \delta_2$.

The operating status of each grid cell is coded from the first year it hosts an operational critical mineral mine and remains fixed thereafter. This approach estimates treatment effects by comparing changes in protest outcomes between cells that ever host an operating mine and those without any mining activity. Only five mines, located in four grid cells, suspended operations for more than one year due to declining profitability, suggesting that assuming constant active status is not overly restrictive.[20]

Time-varying weather anomalies X_{it} are included to control for climate-driven protest triggers (Miguel et al., 2004; Burke et al., 2015a; Bertinelli et al., 2025). Moreover, this specification includes cell fixed effects (γ_i) and country-year fixed effects (γ_{ct}) to address unobserved heterogeneity and potential reporting bias in the ACLED data. Cell fixed effects control for time-invariant characteristics, such as geological suitability for mining, that may jointly influence extraction activity and protest incidence. Country-year fixed effects absorb national-level time-varying shocks, such as political transitions or policy reforms, that could simultaneously affect mining investment and local protest patterns. Identification therefore relies on within-cell temporal variation in exposure to Chinese-owned mining operations. Therefore, the estimated mining effect should be interpreted as a cell-level association with Chinese-owned mining activity, rather than as an effect confined to locations immediately adjacent to mine sites.

To reduce the risk of overfitting, I keep the baseline specification parsimonious and avoid adding further cell-level covariates, richer fixed effects, or time trends at multiple levels. Instead, I examine the sensitivity of the results to these additional controls in robustness checks.

Standard errors are clustered at the grid cell level to allow for heteroskedasticity and serial correlation within cells. For robustness, following Conley (1999), standard errors that correct for spatial correlation are implemented.

4.4.1 Identification assumption

The internal validity of the design relies on the parallel trends assumption. In this setting, the assumption requires that protest outcomes in grid cells that eventually host Chinese-owned and non-Chinese-owned critical mineral operations follow sim-

[20]Excluding these four grid cells yields virtually identical results: the estimated effects remain significant at the 1% level and similar in magnitude (0.181 for protest probability and 0.462 for protest count) to the baseline estimates.

ilar trends prior to the effective commencement of Chinese extraction, conditional on controls and fixed effects (Olden and Møen, 2022).[21] The plausibility of this assumption is assessed using an event-study framework, estimating:

$$\begin{aligned}\text{Protest}_{it} = \alpha + &\sum_{k=-6,\, k\neq -1}^{6} \delta_{1k}\, \text{CM}_i \cdot \mathbb{1}\{t - \tau_i - 1 = k\} \\ &+ \sum_{k=-6,\, k\neq -1}^{6} \delta_{2k}\, \text{CM}_i^{\text{China}} \cdot \mathbb{1}\{t - \tau_i - 1 = k\} \\ &+ \beta X_{it} + \gamma_i + \gamma_{ct} + \mu_{it}. \end{aligned} \tag{4.2}$$

where τ_i denotes the year in which Chinese-owned critical mineral extraction begins in grid cell i. Event time is defined as $k = t - \tau_i - 1$ to match the baseline specification that uses one-year lagged treatment indicators and $k = -1$ is omitted as the reference period. The farthest lead and lag bin all periods $k \leq -6$ and $k \geq 6$, respectively.

Figure 4.6 plots the estimated Chinese ownership event-time coefficients and their corresponding 95% confidence intervals using OLS. In panel (a), the pre-treatment coefficients are statistically indistinguishable from zero and centered around zero.[22] In panel (b), the pre-treatment coefficients display a mild upward pattern, but this is mainly driven by the far-left binned lead. The remaining non-binned pre-treatment coefficients are individually insignificant, and a joint significance test indicates that they are not jointly different from zero (p-value = 0.2811). Moreover, for both dependent variables, the average post-operation estimates are larger than the corresponding pre-operation averages, with the differences statistically significant at the 1% level. Altogether, these results provide support for the parallel trends assumption.

Recent literature has noted potential bias in conventional difference-in-differences estimators under staggered treatment adoption (De Chaisemartin and d'Haultfoeuille (2023)). Such bias arises when early-treated units serve as controls for later-treated ones. Specifically, this bias is exacerbated when some units are treated throughout the sample period. In my setting, only two Chinese-owned critical mineral mines

[21] Results are robust to excluding weather anomaly controls.

[22] The far-left lead bins all periods $k \leq -6$. For panel (a), the joint test including this binned lead is marginal at the 10% level (p-value = 0.0976), but excluding this binned lead, the remaining pre-treatment coefficients are not jointly different from zero (p-value = 0.5398.)

began operating before 2004 and remains always treated, thereby minimizing the potential for such bias. Decomposition following De Chaisemartin and d'Haultfoeuille (2020) shows that negative weights are negligible for the estimated Chinese ownership event-time coefficients (0.07% of total weight).[23] To further address this concern, Equation 4.2 is re-estimated using two alternative approaches designed for heterogeneous treatment effect: the stacked event-study estimator (Cengiz et al., 2019) and the interaction-weighted estimator (Sun and Abraham, 2021).[24] As shown in Figure 4.6, pre-treatment coefficients are statistically indistinguishable from zero, whereas the post-treatment effects are consistently positive and persistent across all estimators. These results reinforce the robustness of the baseline findings to concerns about bias from staggered treatment timing and heterogeneous treatment effects.

Similarly, Figure 4.F.1 provides no evidence of differential pre-trends in either of the underlying difference-in-differences specifications for Chinese and non-Chinese critical mineral mines.

4.4.2 Threats to the identification

Three possible threats merit further consideration. The first concerns the potential endogeneity of project timing. Operations may be suspended or resumed in response to shifts in profitability or local protest activity. To mitigate this issue, a fixed treatment status is assumed and lagged independent variables in the baseline specification are employed. The absence of significant leads in the event study, estimated with both OLS and heterogeneity-robust estimators, suggests that anticipation effects are limited.

The second challenge relates to the non-random spatial distribution of Chinese investment as the allocation of Chinese mines are concentrated in specific countries and sub-national regions (see Figure 4.4). Country-level selection is unlikely to bias the results for two reasons. First, as shown in Figure 4.A.4, Chinese investment in critical mineral extraction showed no systematic association with host-country governance quality.[25] Second, county-year fixed effects help absorbing unobserved

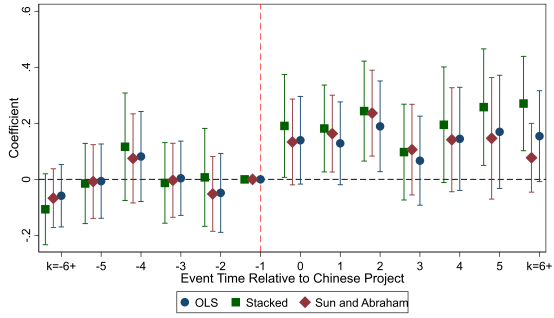
[23] In contrast, 48 grid cells host mines that became operational before 2003; for these, $CM_{i,t-1}$ is always equal to one, and their effects are absorbed by cell fixed effects. Negative weights for $CM_{i,t-1}$ accounts for 18% of total weight.

[24] They are implemented via the Stata routines of *stackedev* (Bleiberg, 2021) and *eventstudyinteract* (Sun, 2021), respectively. Control cohorts are never-treated units.

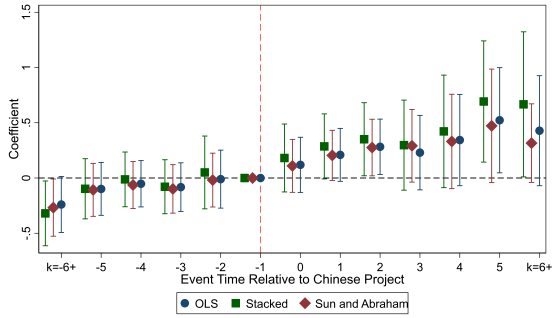
[25] I regress probability of hosting a Chinese investment and number of Chinese projects hosted for each sub-Saharan African country on its pre-determined governance quality and Figure 4.A.4 illustrates the estimated coefficients.

Figure 4.6: No Differential Pre-trend: Event Study

(a) Probability of protest



(b) Number of protest



Note: This figure plots the estimated Chinese ownership event-time coefficients and their corresponding 95% confidence intervals. Event time is defined as $k = t - \tau_i - 1$ to match the baseline specification that uses one-year lagged treatment indicators and $k = -1$ is omitted as the reference period. I bin all years before $k = -6$ and after $k = 6$ at $k = -6$ and $k = 6$ respectively. Point estimates are estimated using (1) OLS; (2) stacked regression approach proposed by Cengiz et al. (2019); and (3) the estimator proposed by Sun and Abraham (2021). I use Stata routines *"stackdid"* created by Bleiberg (2021) and *"eventstudyinteract"* created by Sun (2021) to implement the last two estimators with never-treated group is considered as control group for comparison. Never-treated cells serve as the control group, and treatment cohorts are defined according to the first year in which $CM_{i,t-1}^{\text{China}} = 1$All specifications include the full set of leads and lags for non-Chinese mining, weather anomalies, cell fixed effects, and country-by-year fixed effects. Standard errors are clustered at the cell level.

county-wide common shocks. For robustness, I exclude each host country in turn to assess whether the baseline point estimates are driven by any single country.

At the subnational level, concern about selection issue also appears limited. The particular threat is the potential reverse causality: Chinese projects might, in principle, be located in areas that are systematically more or less protest-prone (Berman et al., 2017; Iacoella et al., 2021). Yet regressions of Chinese ownership on lagged protest incidence and intensity at the focal cell and the corresponding ADM2 region (one- to three-year lags; see Figure 4.F.2) yield insignificant coefficients, suggesting

that investment location is not systematically correlated with prior protest activity. [26] Furthermore, including annual weather anomalies and cell fixed effects in the baseline specification as well as additional covariates in the robustness tests reduce concerns about omitted variables that may be correlated with both investment and protest outcomes.

The third challenge involves non-random selection of Chinese investment with respect to commodity type. Specifically, copper and cobalt as well as gold and silver accounts for half of all Chinese investment. To assess whether the estimated effects are driven by any particular commodity, I re-estimate the model repeatedly, excluding one commodity at a time while retaining all others.

4.5 Main Result

4.5.1 Do Chinese critical mineral mines cause more protest?

I begin with baseline estimates assessing the additional effect of Chinese critical mineral investment on protest activity. Table 4.1 reports the results. Columns (1)–(2) present estimates from specifications including only $CM_{i,t-1}$, weather-shock controls, and a full set of fixed effects. The coefficients on $CM_{i,t-1}$ are positive and statistically significant at the 1% level. The effective commencement of any critical mineral mine is associated with a 12 percentage point increase in protest probability (143% of the sample mean) and a 22% increase in the number of protests.[27]

Columns (3)–(4) add $CM^{\text{China}}_{i,t-1}$ and represent the baseline results. The estimated effects of non-Chinese projects remain positive for both protest probability and intensity, though only the former is significant at conventional levels. In contrast,the coefficients on $CM^{\text{China}}_{i,t-1}$ are positive and significant at the 1% level for both outcomes, indicating that Chinese-owned operations exerted substantially stronger effects on local protest incidence and intensity. Relative to areas with only non-Chinese mines, Chinese-operated projects increased protest probability by 17 percentage points (216% of the mean) and protest intensity by 57%. The combined effect of Chinese-owned extraction ($\gamma_1 + \gamma_2$) corresponds to a 25 percentage point

[26] These regressions include $CM_{i,t-1}$, weather anomalies, and cell and country-year fixed effects.

[27] Following Bellemare and Wichman (2020), the coefficient is interpreted as the approximate percentage change in protest counts, $\frac{\%\ \Delta\ Y}{100} = \exp(\gamma_1) - 1$. This interpretation is approximate given the small average number of protest events, which can amplify scale-dependence in the IHS transformation (Aihounton and Henningsen, 2021; Chen and Roth, 2024). Sensitivity analyses using alternative transformations confirm the robustness of results.

increase in protest probability (303% of the mean) and a 70% rise in protest intensity compared with areas without any critical mineral mining. Such estimated effects of Chinese mining investment are large, but they remain broadly consistent with Christensen (2019), who shows that mine operations can more than double the probability of protests or riots

Columns (5)–(6) restrict the sample to grid cells that either host an operating mine or host a mine under construction that had not begun production by the end of the sample period. This restriction improves comparability between treated and control cells by mitigating selection bias related to geological conditions and other unobserved factors influencing whether a cell hosts mining activity (Mamo et al., 2019). The sample is reduced to 205 grid cells. The estimated effects of Chinese-owned mining remain positive and statistically significant at the 10% level, though attenuated in magnitude, suggesting that the baseline results are unlikely to be driven by selection.

Overall, Table 4.1 confirms that Chinese critical mineral investment increases local protests in host communities.

Table 4.1: Main result: Comparison to Other Mines without Chinese Ownership

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline sample				Restricted sample	
	Probability	IHS(#Protests)	Probability	IHS(#Protests)	Probability	IHS(#Protests)
$CM_{i,t-1}$	0.117***	0.200***	0.071**	0.083	-0.030	-0.081
	(0.026)	(0.060)	(0.029)	(0.067)	(0.034)	(0.076)
$CM^{\text{China}}_{i,t-1}$			0.177***	0.448***	0.093*	0.234*
			(0.050)	(0.137)	(0.049)	(0.128)
Observations	61,140	61,140	61,140	61,140	4,100	4,100
Cell FE	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓
Weather Controls	✓	✓	✓	✓	✓	✓
Adjusted R-squared	0.409	0.594	0.410	0.596	0.459	0.663
Mean of dependent variable	0.0818	0.132	0.0818	0.132	0.260	0.467
Number of cells	3057	3057	3057	3057	205	205
Cells with Chinese Mine	48	48	48	48	48	48

Note: This table displays the baseline result. Estimates are based on equation 4.1. Dependent variables are protest incidence and protest intensity. From Columns (1) to (4), I include all critical mineral mines in the sample. In Columns (5) and (6), the control grid-cells are restricted to the cells with critical mineral mining activities but not start producing until the end of the sample period. All regressions include weather anomalies, cell fixed effects and country-year fixed effects. Standard errors (in parentheses) are clustered at the cell levels; Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

4.5.2 Robustness tests

I assess the robustness of the baseline results along several dimensions: (1) alternative estimators, (2) alternative specifications, (3) sample restrictions, (4) matched samples, (5) alternative definitions of Chinese investment, (6) alternative Chinese

mining investment definition, and (7) alterative protest outcomes. Tables 4.2 and 4.3 summarize the results.[28]

Heterogeneity-robust estimators Rows 1 and 2 of Table 4.2 address concerns about potentially "bad" comparisons arising in staggered treatment designs by applying the estimators proposed by Cengiz et al. (2019) and Sun and Abraham (2021), respectively. The static triple-differences estimates obtained from these estimators are slightly smaller in magnitude than the baseline estimates but remain positive and statistically significant at the 1% level. These findings suggest that the baseline results are not driven by inappropriate comparisons and are robust to treatment effect heterogeneity.

Alternative specifications I further examine the sensitivity of the estimates to alternative model specifications. I begin with two parsimonious specifications: model with no weather controls (Row 3) and replace country-year fixed effects with year fixed effects (Row 4). In Row 5, I replace the lagged treatment variable with its contemporaneous counterpart, $CM_{i,t}^{\text{China}}$. Rows 6–9 sequentially add additional controls, including lagged protest outcomes (Row 6), lagged mine age (Row 7), lagged nighttime luminosity and population (Row 8), interaction terms between cell's pre-determined characteristics and year fixed effects (Row 9), all cell-level covariates included in Rows 8 and 9 (Row 10), and lagged counts of Chinese foreign aid projects (Row 11).[29] Specifically, including Chinese aid projects helps account for broader Chinese economic engagement in sub-Saharan Africa beyond the mining sector. Rows 12–14 modify the fixed-effects structure by replacing county-year fixed effects with ADM2 region-year fixed effects, and by adding commodity-specific trends and cell-specific linear trends. Across all alternative specifications, the point estimates retain the same sign and remain statistically significant, closely aligning with the baseline results.

In Row 15, I replace binary treatment variables with continuous measures of exposure of critical mineral mines to account for multiple presence of mining activities within a cell.[30] The coefficients on the transformed number of Chinese critical

[28] Full results are available upon request.

[29] Cell's pre-determined characteristics include elevation, ruggedness, distance to coast, average malaria infection (2000-2001), total railway length (2000), and total road length (2000).

[30] I use the IHS transformed total number of non-Chinese-owned critical mineral mines ($\#CM_{i,t-1}^{Other}$) and the number of Chinese-owned mines ($\#CM_{i,t-1}^{\text{China}}$) that began operations in year $t-1$. Estimated coefficients on $\#CM_{i,t-1}^{Other}$ are 0.087*** (p-value = 0.000) and 0.134** (p-value = 0.012) for protest probability and intensity, respectively.

Table 4.2: Sensitivity Check I: Alternative Estimator and Specification

	Probability (1)	IHS(#Protests) (2)
Baseline	**0.177*** **	**0.448*** **
	(0.050)	**(0.137)**
(a) Alternative estimators		
(1) Stacked DiD (Cengiz et al., 2019)	0.163***	0.433***
	(0.048)	(0.130)
(2) Sun and Abraham (2021)	0.137***	0.380***
	(0.042)	(0.120)
(b) Alternative specification		
(3) No Controls	0.178***	0.449***
	(0.050)	(0.137)
(4) Year Fixed Effects	0.194***	0.489***
	(0.050)	(0.140)
(5) $ChineseOwn_{i,t} \times CM_{i,t}$	0.158***	0.420***
	(0.045)	(0.122)
(6) Adding $Y_{i,t-1}$	0.156***	0.283***
	(0.043)	(0.086)
(7) Adding Mine's Age(t-1)	0.156***	0.409***
	(0.049)	(0.138)
(8) Adding Nightlights(t-1) and population(t-1)	0.174***	0.443***
	(0.049)	(0.137)
(9) Adding pre-determined characteristics	0.154***	0.375***
	(0.051)	(0.135)
(10) Adding Nightlights(t-1), population(t-1), and pre-determined characteristics	0.153***	0.371***
	(0.051)	(0.134)
(11) Adding Chinese Aids(t-1)	0.177***	0.447***
	(0.050)	(0.137)
(12) Region × year FE	0.256***	0.505***
	(0.080)	(0.170)
(13) With commodity-specific trends	0.123**	0.351***
	(0.051)	(0.134)
(14) With cell-specific trends	0.119**	0.176*
	(0.057)	(0.092)
(15) Number of mines (IHS transformed)	0.187***	0.469***
	(0.038)	(0.122)
(16) Small-scale operation × year FE	0.128***	0.323***
	(0.048)	(0.116)

Note: This table summarizes the results of the sensitivity checks. All estimates are based on equation 4.1. All regressions include $CM_{i,t-1}$, weather anomalies, cell fixed effects and country-year fixed effects. Only the estimated coefficients on $CM^{\text{China}}_{i,t-1}$ are displayed. Standard errors (in parentheses) are clustered at the cell levels; Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

mineral mines remain positive and statistically significant at the 1% level.

Lastly, in Row 16, I control for the potential confounding effect of small-scale mining operations. Such operations can negatively affect local communities and trigger social unrest (Girard et al., 2025; Stoop et al., 2019). Using the data compiled by Tang and Werner (2023), I classify 2,516 mines with an area below 0.062 square kilometers as small-scale operations. I then augment the baseline specification by controlling for the presence of small-scale mining operations interacted with country-year fixed effects. The resulting estimates remain positive and statistically significant at the 1% level.

Sample restrictions Table 4.3 reports another set of robustness tests using alternative sample definitions. Row 1 includes cells in all sub-Saharan African countries. In Rows 2 - 4, I exclude grid cells located in national capitals, first-level administrative seats, and along international borders. I then assess sensitivity to specific host countries by excluding grid cells located in the DRC (Row 5), Guinea (Row 6), South Africa (Row 7), and Zambia (Row 8). Rows 9-11 exclude cells hosting copper and cobalt mines, gold and silver mines, and iron ore mines, respectively. The estimated effects remain robust across these alternative samples and are not driven by particular countries or mineral types. In the appendix, Figure 4.F.3 presents a full set of leave-one-out sensitivity checks by country, commodity, and year. Across all regressions, the estimated coefficients remain statistically significant at least at the 5% level.

To avoid contamination of mining activities to the nearby cells which are part of control group, I exclude the first-order non-mining neighbors of the mining cells. Row 12 shows that the resulting estimates are robust. In Row 13, I exclude cells in the top 1% of the protest counts distribution to account for the influence of outliers and the estimates remain similar.

Matched samples I construct two matched samples. First, in Row 14 of Table 4.3, following Acemoglu et al. (2012) and Sonno (2025), I estimate a neighborhood fixed-effects specification using only grid cells hosting operating critical mineral projects and their first-order non-mining neighbors, thereby exploiting within-neighborhood variation.[31] Second, in Row 15, I implement one-to-one propensity score matching to pair cells hosting Chinese-owned mines with comparable cells hosting non-Chinese mines.[32] In both cases, the estimated effects remain positive and statistically significant.

Definition of Chinese investment In the baseline specification, Chinese critical mineral investment is defined as a mining project in which Chinese firms hold at least 10% of equity shares. If higher ownership shares are associated with greater decision-making power over day-to-day operations, projects with larger Chinese equity stakes may more directly reflect Chinese firms' management practices. There-

[31] I replace the cell fixed effects with neighbor fixed effects in this specification. The resulting estimated coefficients are similar.

[32] Matched samples are constructed using the Stata routine *psmatch2* (Leuven and Sianesi, 2003). Propensity scores are estimated using a logit model. Matching covariates include elevation, terrain ruggedness, ethnic fractionalization, ethnic polarization, total road and rail length, distance to the coast, population, nightlight density, malaria incidence, and country fixed effects.

fore, if differences in management practices contribute to local grievances, mines with higher Chinese ownership shares may be more likely to generate protest responses. To examine this possibility, I vary the ownership threshold to 50% and 67%. Rows 16 and 17 of Table 4.3 show that the estimates become larger and remain positive and statistically significant, suggesting that Chinese management or operational influence may play an important role.

Alternative Chinese investment data Rows 18 and 19 of Table 4.3 assess the robustness of the results to differences in coverage between the Chinese investment dataset and the S&P SNL Metals and Mining database. First, I retain only cells that overlap across the two datasets and classify them as treated. Second, I restrict the treatment group to 38 cells with Chinese projects recorded exclusively in the SNL database. In both exercises, all non-Chinese-owned mines are retained as in the baseline specification. The resulting estimates remain positive and statistically significant at the 1% level.

Mining-related protests Row 20 reports estimates using a restricted outcome that includes only protests explicitly related to mining activities. Using the event descriptions in the ACLED dataset, I identify 489 mining-related protests in the sample cells over the sample period.[33] Although the magnitudes decline slightly, the estimates remain positive and statistically significant at the 1% level, suggesting that the baseline findings are not driven solely by unrelated protest activity.

Permutation tests I conduct two permutation tests to assess whether the estimated effects could arise by chance. In panel (a) of Figure 4.F.4, operation years are randomly reassigned across all critical mineral mining cells. In panel (b), both the locations and operation years of all critical mineral mines are jointly randomized across all sample grid cells. Each procedure is repeated 1,000 times. In both cases, the distribution of placebo coefficients is centered around zero, and the baseline point estimates lie in the far-right tail of the placebo distribution. The resulting p-values are 0.001 and 0.003 for panel (a), and 0.008 and 0.013 for panel (b).[34]

Other robustness checks In Table 4.G.1, I examine alternative clustering levels for standard errors and implement spatially correlated standard errors following

[33] The selected keywords/phases are " mine ", "mining", "miner", "miners", "mineral", "minerals", "quarry", and "mine site".

[34] p-values are computed as the share of placebo t-statistics exceeding the baseline t-statistic.

Table 4.3: Sensitivity Check II: Alternative Sample

	Probability	IHS(#Protests)
	(1)	(2)
Baseline	**0.177*** **	**0.448*** **
	(0.050)	**(0.137)**
(c) Alternative sample		
(1) All SSA countries	0.161***	0.406***
	(0.048)	(0.131)
(2) No national capital	0.177***	0.450***
	(0.050)	(0.137)
(3) No first-level administrative seats	0.173***	0.357***
	(0.052)	(0.118)
(4) No border cells	0.182***	0.378***
	(0.055)	(0.124)
(5) Excluding DRC	0.105**	0.292**
	(0.052)	(0.130)
(6) Excluding Guinea	0.164***	0.426***
	(0.055)	(0.147)
(7) Excluding South Africa	0.211***	0.466***
	(0.056)	(0.150)
(8) Excluding Zambia	0.170***	0.459***
	(0.052)	(0.150)
(9) No copper and cobalt	0.110**	0.360**
	(0.053)	(0.141)
(10) No gold and silver	0.188***	0.386**
	(0.056)	(0.154)
(11) No iron ore	0.198***	0.513***
	(0.052)	(0.143)
(12) Excluding Neighboring cells	0.184***	0.455***
	(0.050)	(0.138)
(13) Excluding outliers	0.189***	0.311***
	(0.051)	(0.092)
(14) Neighborhood FE	0.145**	0.344**
	(0.064)	(0.140)
(15) Matched: PSM	0.144***	0.300**
	(0.053)	(0.143)
(16) Shares Threshold:50%	0.196***	0.493***
	(0.057)	(0.159)
(17) Shares Threshold:67%	0.192***	0.540***
	(0.061)	(0.175)
(18) Only Chinese projects in both dataset	0.223***	0.485***
	(0.052)	(0.144)
(19) All Chinese projects in SNL	0.165***	0.383***
	(0.039)	(0.105)
(20) Mining-related protests	0.095***	0.111***
	(0.030)	(0.039)

Note: This table summarizes the results of the sensitivity checks. All estimates are based on equation 4.1. All regressions include $CM_{i,t-1}$, weather anomalies, cell fixed effects and country-year fixed effects. Only the estimated coefficients on $CM^{\text{China}}_{i,t-1}$ are displayed. Standard errors (in parentheses) are clustered at the cell levels; Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

Conley (1999).[35] The statistical significance of the baseline estimates is largely preserved.

Finally, because the interpretation of average treatment effects with an IHS transformed dependent variable may depend on the measurement scale (Aihounton and Henningsen, 2021; Chen and Roth, 2024), I consider alternative transformations

[35] I use the Stata routine *acreg* developed by Colella et al. (2019) to compute Conley standard errors.

of protest counts. Table 4.G.2 shows that the estimated effects remain positive and statistically significant across alternative transformation approaches.

4.5.3 Additional analyses

This section extends the analysis with additional exercises to investigate several dimensions of the protest-intensifying effects associated with Chinese critical mineral extraction.

Types of conflict The effects of Chinese critical mineral mining are examined across different types of conflict events. Panel (a) of Figure 4.F.5 presents the estimated coefficients and 90% confidence intervals for each event type.[36] The estimated effects on protests and riots are substantially larger than those on battles and violence against civilians.[37] Similarly, using UCDP Georeferenced Event Dataset data (Sundberg and Melander, 2013), which record all instances of organized violence, yields insignificant results.[38] This suggests that Chinese critical mineral investment is associated with social unrest rather than organized or lethal conflict, consistent with prior evidence (Christensen, 2019; Stoop et al., 2019).

Protest related deaths Although Chinese investment is not significantly associated with more lethal forms of conflict, I examine its link to the protest related fatalities derived from the ACLED data. The results, reported in Table 4.G.3, show that the estimated coefficients on CM_{t-1}^{China} are positive and statistically significant at the 1% or 5% levels across all transformation approaches. The magnitudes are economically significant: Column (2) indicates that cells hosting active Chinese critical mineral operations experience, on average, 0.2 additional protest-related deaths (326% of the sample mean), relative to the cells with non-Chinese owned mines. Overall, these findings suggest that while Chinese mining does not raise the overall incidence of lethal conflict, protests near these operations tend to be substantially more violent.

Value of mining operation Previous studies show that fluctuations in global commodity prices influence local conflict dynamics (Dube and Vargas, 2013; Bazzi

[36] Estimated coefficients and corresponding 90% confidence intervals on $CM_{i,t-1}$ for all conflict types are shown in panel (b) of Figure 4.F.5.

[37] Violence against civilians also includes incidents of remote violence.

[38] I use the UCDP GED Global version 25.1 and include only events with the highest level of location precision.

and Blattman, 2014; McGuirk and Burke, 2020). In particular, Berman et al. (2017) find that rising world mineral prices increased conflict likelihood in mining areas. To account for mineral price shock, an interaction term $ln(World\ Price_{i,t-1}) \times CM_i^{ever}$ is included, defined as the natural logarithm of one-year lagged world mineral prices interacted with an indicator for cells that have ever hosted operating critical mineral mines. Price data for nine minerals are drawn from the World Bank. Cells without mining activity are assigned a value of zero, while mines lacking global price data are excluded, leaving 142 mining cells, including 34 with Chinese ownership.

Columns (1)–(2) of Table 4.G.4 show that coefficients on $ln(World\ Price_{i,t-1}) \times CM_i^{ever}$ are positive and significant at the 1% level for both protest probability and intensity, consistent with prior evidence that global positive mineral price shocks heighten local conflict. Importantly, controlling for price fluctuations does not affect the sign or significance of coefficient on $CM_{i,t-1}^{\text{China}}$. Columns (3)–(4) replace $CM_{i,t-1}^{\text{China}}$ with an interaction term between Chinese ownership and mineral prices. Both interaction terms, regardless of nationality, are positive and significant at the 1% level, indicating that Chinese ownership amplifies the effect of rising mineral prices on local protest activity.

Foreign ownership One focal point of the analysis is whether Chinese firms trigger more social unrest than other foreign owners. Table 4.G.5 directly compares Chinese-owned mines with other foreign-owned mines by excluding cells that ever host domestically owned mines. Using the same specification as in equation 4.1, Columns (1) and (2) show that the estimated coefficient on Chinese ownership remains positive and statistically significant at the 1%-5% levels. In Columns (3) and (4), I redefine $CM_{i,t-1}^{China}$ to equal one if a cell hosts an operating Chinese mine and the share of Chinese ownership within the cell exceeds 0.5. Although the resulting point estimates are smaller, they remain positive and statistically significant. Overall, these results indicate that Chinese ownership additionally increases local protest relative to other similar foreign-owned mines.

Non-critical mineral, spillover effects, and greenfield investment Table 4.G.6 displays the results of three additional analyses. Firstly, to assess whether the baseline results are specific to critical minerals, I extend the sample to include all mineral extraction projects, including non-critical minerals such as coal and di-

amonds.[39] Columns (1) and (2) show that the presence of any mining activity significantly increases the probability and intensity of local protest. When restricting attention to critical minerals, only Chinese-owned operations generate positive and statistically significant effects on both protest incidence and intensity.

I further examine potential spillovers by allowing protest outcomes to depend on mining activity in first-degree neighboring cells. Consistent with Berman et al. (2017), Columns (3) and (4) show that there is a positive spillover effect of mining activity, regardless of ownership, on protest in the adjunct area. However, while Chinese-owned critical mineral mines significantly increase protest activity within host cells, there is no evidence of additional spillover effects into neighboring cells. Together, these results indicate that the protest-amplifying effects of Chinese critical mineral mining are localized.

Finally, I examine whether the effects differ between greenfield and brownfield Chinese investments. Although greenfield projects typically involve greater early-stage local disruption, interacting $CM_{i,t-1}^{\text{China}}$ with a greenfield indicator yields statistically insignificant coefficients (Columns (5) and (6)). This suggests that the protest-amplifying effect of Chinese critical mineral mining does not systematically vary by investment type.

4.5.4 Heterogeneous Effects

Quality of governance in host country Domestic institutions can reduce social unrest generated by mining activities (Berman et al., 2017; Christensen, 2019). In the context of Chinese investment which places substantial weight on the host country's regulatory quality, stronger governance and greater transparency can mitigate the outbreak of protest.

To proxy institutional quality, I consider three indicators: (1) the index of democracy from the Polity V project; (2) the estimate of control of corruption; and (3) the estimate of regulatory quality from the World Governance Indicators. For each indicator, I construct a binary variable equal to one if the host country's pre-sample (1996–2003) governance score exceeds the sample median. Then each binary variable is interacted with $CM_{i,t-1}^{\text{China}}$ to assess how the effects of Chinese critical mineral mining vary across institutional settings.

Table 4.4 reports the results. Columns (1)–(2) present estimates with interaction terms for Polity V index. While the point estimates on Chinese investment

[39] Of the 254 cells with operating mines, 132 contain critical mines without Chinese ownership, while 48 contain critical mines with Chinese ownership.

remain positive and statistically significant, protest probability and intensity are significantly attenuated in countries with above-median levels of democracy. Similarly, stronger control of corruption (Columns (3)–(4)) and higher regulatory quality (Columns (5)–(6)) significantly reduce protest incidence, though not intensity. This finding is consistent with the evidence in the previous studies that improved oversight and reduced rent-seeking in better-governed settings can reduce local resistance.

Columns (1)–(8) of Table 4.G.7 report results using alternative governance quality measures from the World Governance Indicators. The interaction coefficients remain consistently negative, although not all indicators are statistically significant at conventional levels. In contrast, the host country's compliant-member status in the Extractive Industries Transparency Initiative does not significantly mitigate local protest.

Table 4.4: Heterogeneous Effects: Institutional Quality

	(1)	(2)	(3)	(4)	(5)	(6)
	Polity V		Control of Corruption		Regulatory Quality	
	Probability	IHS(#Protests)	Probability	IHS(#Protests)	Probability	IHS(#Protests)
$CM^{\text{China}}_{i,t-1}$	0.294***	0.669***	0.264***	0.581***	0.279***	0.591***
	(0.070)	(0.217)	(0.068)	(0.196)	(0.067)	(0.200)
$CM^{\text{China}}_{i,t-1}$	-0.227**	-0.442*	-0.193**	-0.320	-0.227**	-0.326
$\times$ *High Quality*	(0.093)	(0.267)	(0.092)	(0.264)	(0.091)	(0.264)
Observations	61,140	61,140	61,140	61,140	61,140	61,140
Cell FE	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓
Weather Controls	✓	✓	✓	✓	✓	✓
Adjusted R-squared	0.411	0.596	0.410	0.596	0.411	0.596
Mean of dependent variable	0.0806	0.130	0.0806	0.130	0.0818	0.132
Number of cells	3057	3057	3057	3057	3057	3057
Cells with Chinese mines	48	48	48	48	48	48

Note: This table displays the estimates of heterogeneous effects based on institutional quality. All estimates are based on equation 4.1. All regressions include $CM_{i,t-1}$, $CM_{i,t-1} \times High\ Quality$, weather anomalies, cell fixed effects and country-year fixed effects. *High Quality* is a binary variable equal to one if a country's average institutional-quality score between 1996 and 2003 exceeds the sample median. The institutional-quality measures are the democracy index from the Polity V Project, estimates of control of corruption, and regulatory quality from the World Governance Indicator. Standard errors (in parentheses) are clustered at the cell levels; Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

Additional heterogeneous effects Furthermore, Table 4.G.8 reports the heterogeneous effects of Chinese-owned critical mineral mining operations with respect to proximity with China, ethnic diversity, and Chinese mining project's characteristics.

Firstly, I examine the heterogeneous effects with respect to two forms of alignment. In Columns (1) and (2), I measure political proximity to China using the host country's ideal-point distance from China in UN General Assembly voting (Voeten,

2009), averaged over 1996–2003. Countries with below-median ideal-point distance are classified as having low voting disagreement with China. The estimated interaction terms between Chinese investment and the low-disagreement indicator are negative, but statistically insignificant. In Columns (3) and (4), I proxy for economic proximity to China using the share of the host country's total trade accounted for by bilateral trade with China using the BACI data (Gaulier and Zignago, 2010). The estimated interaction terms are again negative but statistically insignificant. Overall, the results provide limited evidence that political or economic alignment with China systematically moderates the local protest effects of Chinese-owned mining investment.

Secondly, I examine heterogeneity along ethnic diversity, given its documented association with political instability and conflict (Esteban and Ray, 2008). Two measures were considered: (1) the ethnic fractionalization index (Alesina et al., 2003) and (2) the ethnic polarization (RQ) index (Montalvo and Reynal-Querol, 2005).[40] As reported in Columns (5) - (8), coefficients on both indices are statistically insignificant, indicating that ethnic diversity does not moderate the relationship between Chinese mining and local protest.

Lastly, I investigate heterogeneity with respect to characteristics of Chinese mining projects. In Columns (9) and (10), I distinguish Chinese projects by ownership type, separating projects owned by Chinese private firms, which serve as the reference group, from those owned by Chinese state-owned enterprises (SOEs). I construct a binary indicator equal to one if SOE-owned projects account for more than half of Chinese mining activity within a cell. The estimated coefficients on the SOE interaction terms are statistically insignificant. In Columns (11) and (12), I examine whether the effect varies with the size of Chinese mining projects. Project size is measured by the total mining resources associated with Chinese-owned projects within a cell. The estimated coefficients on the project-size interaction terms are also statistically insignificant. Overall, these results provide little evidence that the heterogeneous effects are systematically explained by Chinese SOE ownership or by the resource size of Chinese-owned mining projects.

[40] They are derived from the Geo-referencing Ethnic Groups (GREG) dataset (Weidmann et al., 2010) and 2001 LandScan population data. $EF_i = 1 - \sum_{n=1}^{N} Ps_n^2$ and $RQ_i = 1 - \sum_{n=1}^{N} [\frac{0.5 - Ps_n}{0.5}]^2 Ps_n$ where Ps_n denotes 2001 population of ethnic group n.

4.6 Mechanisms

In the absence of effective regulation, the adverse consequences of critical mineral mining may be amplified under Chinese ownership. Anecdotal evidence points to environmental degradation, heightened perceptions of corruption, and deteriorating local economic conditions around Chinese-operated mines, factors that are closely associated with elevated local unrest. This section examines these mechanisms using satellite imagery and individual-level survey data from the Afrobarometer and the DHS.[41]

4.6.1 Environmental Degradation and Local Grievances

Recent evidence identifies environmental pollution as a major concern associated with Chinese-operated mining in sub-Saharan Africa. Numerous reports document severe environmental degradation near Chinese mining sites (see, e.g., Inamuco, 2022). Because pollution from mineral extraction adversely affects health and economic outcomes (Aragón and Rud, 2016; Heft-Neal et al., 2018; Von der Goltz and Barnwal, 2019), such externalities are likely to intensify local resistance.

I first provide empirical evidence on the environmental impact of Chinese critical mineral operations. I examine two dimensions of environmental degradation: air pollution and vegetation loss. Column (1) of Table 4.5 reports estimates using satellite-derived $PM_{2.5}$ concentrations.[42] The results show that Chinese-operated mines significantly increase local air pollution, whereas non-Chinese mines have no detectable effect. Relative to cells without mining, Chinese operations raise $PM_{2.5}$ concentrations by 0.6 $\mu g/m^3$. To gauge the potential health relevance of this increase, I conduct a back-of-the-envelope calculation using the estimate from Atkinson et al. (2014), who report a 1.04% increase in all-cause mortality for a 10 $\mu g/m^3$ increase in $PM_{2.5}$. Under this benchmark, a 0.6 $\mu g/m^3$ increase corresponds to a 0.063% increase in all-cause mortality. Applied mechanically to 8.8 million annual deaths in sub-Saharan Africa, this implies approximately 5,500 additional deaths per year.[43]

[41] The Afrobarometer does not provide granular geolocation data for respondents in the more recent survey rounds. As Chinese mining investment expanded substantially since the 2010s, I choose to use less granular data (at the ADM1 level) which can cover the full sample period.

[42] Satellite-based $PM_{2.5}$ concentration data are derived from SEDAC (Van Donkelaar et al., 2024).

[43] This back-of-the-envelope assumes a constant concentration-response relationship and applies the estimated local $PM_{2.5}$ increase mechanically to aggregate mortality in sub-Saharan Africa.

Chinese mining activities also generate substantial vegetation losses. Estimates based on NDVI (Column (2)) and deforestation measures (Column (3)) show that Chinese operations significantly reduce vegetation cover and increase tree-cover loss.[44] In aggregate, Chinese mining reduces NDVI by 1.9 units (7% of the mean) and increases deforestation by 1.4 units (172% of the mean).[45]

These environmental effects remain robust to multiple-hypothesis testing corrections. The Romano-Wolf stepdown adjusted p-values are 0.004 for $PM_{2.5}$, 0.016 for NDVI, and 0.016 for deforestation.[46] Event-study estimates in Figure 4.F.6 further show no evidence of differential pre-trends prior to mine openings, supporting the interpretation that the estimated environmental changes emerge after Chinese critical mineral mining operations begin.

Further robustness tests in Table 4.G.9 show that these environmental effects are generally robust. In panel (a), the estimates remain similar in sign and magnitude after adding controls for nighttime lights and population density. In Panel (b), the effects are attenuated but remain mostly significant and same-signed when the control group is restricted to cells with reported mining activities, except in Column (5).

Moreover, the heterogeneous-effect analysis in Table 4.G.10 shows that stronger institutional quality generally mitigates the environmental degradation associated with Chinese mining investment. Except for the insignificant estimates in Columns (2) and (4), the interaction terms have the expected signs and are statistically significant. This suggests that democratic accountability and regulatory quality can help reduce the adverse environmental effects of Chinese mining.

These environmental changes may impose direct health costs on nearby communities. Using eight waves of Demographic and Health Surveys data covering ten countries, I show in Figure 4.7 that exposure to Chinese mining significantly increases the likelihood of child underweight and short-term illnesses, including cough, diarrhea, and fever.[47] Taken together, these findings indicate that Chinese

[44]The Normalized Difference Vegetation Index (NDVI) is derived from MOD13 C2 (Didan, 2015), and forest cover loss data is derived from Global Forest Change V1.11 (Hansen et al., 2013).

[45]Robustness checks restricting to cells with substantial baseline forest cover yield consistent results. Restricting to cells with $>$ 4,000 tree-covered pixels (0.1% of area) yields coefficients of 0.859 ($p = 0.001$) on $CM_{i,t-1}$ and 0.586 ($p = 0.049$) on $CM^{\text{China}}_{i,t-1}$.

[46]I implement the Romano–Wolf correction using the Stata routine *rwolf2* (Clarke, 2021), with 500 bootstrap replications and random seed 123456.

[47]The combined effects associated with the onset of Chinese critical-mineral operations correspond to increases of 2.8%, 26%, 38%, and 27% in the probabilities of underweight, cough, diarrhea, and fever, respectively, relative to their sample means.

Table 4.5: Mechanism I: Environmental Degradation and Chinese Investment

	(1) $PM_{2.5}$	(2) NDVI	(3) Deforestation
$CM_{i,t-1}$	-0.175	-0.574	0.816***
	(0.163)	(0.461)	(0.239)
$CM^{\text{China}}_{i,t-1}$	0.781***	-1.298**	0.579**
	(0.248)	(0.638)	(0.276)
Romano–Wolf p-value	[0.004]	[0.016]	[0.016]
Observations	61,100	57,260	61,140
Cell FE	✓	✓	✓
Country-Year FE	✓	✓	✓
Weather Controls	✓	✓	✓
Adjusted R-squared	0.984	0.990	0.696
Mean of dependent variable	23.72	26.45	0.809
Number of cells	3,055	2,863	3,057
Cells with Chinese mines	48	47	48

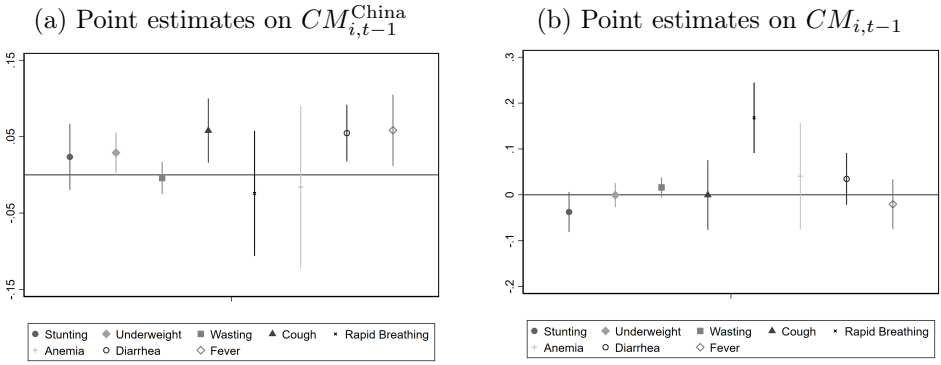
Note: This table reports the estimated effects of mining operations on environmental outcomes. All estimates are based on equation 4.1. $PM_{2.5}$ is measured as the cell-level average concentration of fine particulate matter ($\mu g/m^3$). NDVI refers to the cell-level Normalized Difference Vegetation Index. Deforestation is measured using forest-cover loss. Romano–Wolf stepdown adjusted p-values are reported in brackets for the coefficient on $CM^{\text{China}}_{i,t-1}$ across the three environmental outcomes. All regressions include $CM_{i,t-1}$, weather anomalies, cell fixed effects, and country-year fixed effects. Standard errors, reported in parentheses, are clustered at the cell level. Significance levels are *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

critical-mineral mining generates localized environmental and health damages that may heighten community grievances and increase the risk of protest.

To connect these environmental and health impacts to local unrest, Table 4.6 reports estimates using protest outcomes related to environmental degradation. In Columns (1) and (2), the outcomes are constructed from protest events whose ACLED descriptions directly refer to environmental degradation. The estimated coefficients on Chinese mining operations are positive and statistically significant at the 10% level. The magnitudes are also economically significant: Chinese operations increase the probability of environment-related protest by 5 (6) percentage points. For protest intensity, the estimates imply an increase of approximately 6.3% (7.9%), relative to cells with non-Chinese mining operations (cells without critical mineral mining activity).

I next return to the full set of protest events and examine whether the broader protest response is stronger in areas where environmental damage is more likely to affect local livelihoods. I first examine heterogeneity with respect to local water stress using the World Resources Institute's Aqueduct 4.0 dataset (World Resources Institute, 2023), which measures water stress as the ratio of total water withdrawals to available renewable surface and groundwater supply. Grid cells with above-median water stress are classified as high-risk areas. The interaction coefficients in Columns (3) and (4) are positive and statistically significant, suggesting that

Figure 4.7: Child Health Outcomes, DHS Data



Note: Estimated coefficients and the corresponding 90% confidence intervals on $CM^{\text{China}}_{i,t-1}$ and $CM_{i,t-1}$ for a set of child-level health outcomes. Each dependent variable is binary and equals one if the child is reported to experience one of the following conditions: stunting (n=77,608), underweight (n=77,286), wasting (n=77,143), cough (n=141,558), rapid breathing (n=90,747), anemia (n=62,006), diarrhea (n=141,505), or fever (n=148,336). A child is classified as stunted, underweight, and wasted if their height-for-age Z-score, weight-for-age Z-score , and weight-for-height Z-score below -2. All specifications include the same sets of $CM^{\text{China}}_{i,t-1}$, $CM_{i,t-1}$, weather anomalies, control variables for child (child's gender; mother's age, age squared, education level, household's wealth index, and number of children), as well country-by-birth year fixed effects, birth year-by-birth month fixed effects, country-by-interview year-month fixed effects, cell-specific trends (birth cohort for stunting, underweight, and wasting or interview cohort for other short-term illnesses), and cell fixed effects. Standard errors are clustered at the cell levels.

Chinese mining operations have stronger effects on both the incidence and intensity of protest in areas with higher water stress.

Then, I test whether the effects of Chinese mining operations are more pronounced in cells with greater agricultural exposure. Using the MODIS MCD12Q1 global land-cover product (Friedl and Sulla-Menashe, 2022), I calculate the cropland share of each grid cell in 2003. I classify grid cells with above-median cropland share, conditional on having positive cropland area, as areas where local livelihoods may be more dependent on crop production and therefore more vulnerable to soil contamination and land degradation. The estimated coefficient for high cropland share in Column (5) is positive but statistically insignificant, while the estimate in Column (6) is positive and statistically significant at the 10% level. Overall, these results suggest that Chinese mining operations are associated with more environment-related protests, while their broader protest effects are amplified in water-stressed or agriculturally exposed areas.

Lastly, to further assess whether the evidence is consistent with an environmental grievance mechanism, I conduct a mediation analysis following Imai et al. (2010). The results, reported in Appendix Table 4.G.11, are qualitatively consistent

Table 4.6: Mechanism I: Environment-related Protest and Chinese Investment

	(1)	(2)	(3)	(4)	(5)	(6)
	Environment-related Protest		Water Stress		Cropland Share	
	Probability	IHS(#Protests)	Probability	IHS(#Protests)	Probability	IHS(#Protests)
$CM^{\text{China}}_{i,t-1}$	0.051*	0.061*	0.093	0.170	0.140**	0.214*
	(0.027)	(0.035)	(0.066)	(0.124)	(0.066)	(0.127)
$CM^{\text{China}}_{i,t-1}$			0.172*	0.563**	0.076	0.467*
$\times$ *High Risk*			(0.099)	(0.263)	(0.101)	(0.266)
Observations	61,140	61,140	61,140	61,140	61,140	61,14
Cell FE	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓
Weather Controls	✓	✓	✓	✓	✓	✓
Adjusted R-squared	0.254	0.369	0.410	0.597	0.410	0.596
Mean of dependent variable	0.0117	0.0151	0.0806	0.130	0.0818	0.132
Number of cells	3057	3057	3057	3057	3057	3057
Cells with Chinese mines	48	48	48	48	48	48

Note: This table reports the estimated effects of Chinese mining investment on environment-related protest and heterogeneity by environmental exposure. All estimates are based on equation 4.1. Columns (1) and (2) use protest events whose ACLED descriptions contain keywords related to environmental degradation. Columns (3)-(6) use all protest events and include the interaction $CM^{\text{China}}_{i,t-1} \times High\ Risk$. In Columns (3) and (4), *High Risk* equals one if a cell's water-stress measure exceeds the sample median. In Columns (5) and (6), *High Risk* equals one if a cell's cropland share in 2003 exceeds the sample median, conditional on having positive cropland area. All regressions include $CM_{i,t-1}$, weather anomalies, cell fixed effects, and country-year fixed effects. Standard errors, reported in parentheses, are clustered at the cell level. Significance levels are *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

with this channel. The indirect effects through $PM_{2.5}$ and deforestation are positive and significant, while the NDVI estimates are small and statistically insignificant.[48] The mediated shares are modest, ranging from less than 1% for $PM_{2.5}$ and NDVI to around 1-2% for deforestation. Because mediation analysis relies on strong assumptions, I interpret these results as supplementary evidence that environmental degradation contributes to local grievances, rather than as a definitive decomposition of the treatment effect.

4.6.2 Other channels

Deterioration of Economic Well-being Chinese critical mineral mining may adversely affect the economic well-being of the local community. Although such activities generate substantial revenues, these rents are often unequally distributed, potentially fueling local grievances and protest risk in resource-rich areas (Arellano-Yanguas, 2011; Caselli and Michaels, 2013; Bornschier and Vogt, 2024). Moreover, the Dutch disease literature suggests that resource booms can crowd out tradable sectors such as manufacturing (see Mien and Goujon (2021)). Critics further argue that Chinese firms often prioritize Chinese over local labor (Wegenast et al.,

[48] For all regressions, standard errors are estimated using 500 bootstrap replications with random seed 123456.

Table 4.7: Mechanism II: Economic Well-being

	(1)	(2)	(3)	(4)
	Satellite Imagery		Individual-level (Afrobarometer)	
	Nighttime Light	Gini Coefficient	Unemployed	Cash Poverty
$CM_{i,t-1}$	0.178***	-0.012	0.062**	0.023
	(0.073)	(0.062)	(0.030)	(0.030)
$CM^{\text{China}}_{i,t-1}$	0.058	-0.061	-0.025	0.025
	(0.054)	(0.069)	(0.026)	(0.035)
Observations	61,140	1,940	48,785	53,935
Cell FE	✓			
Country-Year FE	✓	✓	✓	✓
Weather Controls	✓	✓	✓	✓
ADM1 FE		✓	✓	✓
Individual Controls			✓	✓
Adjusted R-squared	0.830	0.682	0.178	0.150
Mean of dependent variable	0.606	0.734	0.690	0.815
Number of cell / ADM1 regions	3057	97	57	57

Note: This table reports estimates of the relationship between Chinese mining operations and various measures of economic grievance. Column (1) presents results based on equation 4.1, where the unit of observation is the cell-year. The dependent variable is the inverse hyperbolic sine-transformed average nighttime light intensity at the cell level. The regression include $CM_{i,t-1}$, weather anomalies, cell fixed effects and country-year fixed effects. In Column (2), the outcome variable is within region Gini coefficient at ADM1 level, with $CM_{i,t-1}$, weather anomalies, ADM1 fixed effects and country-year fixed effects as controls. Columns (3) to (4) report estimates from regressions using individual-year data, also based on equation 4.1. The dependent variable in Column (3) is a binary indicator equal to one if a respondent reported being unemployed. In Columns (4), the dependent variable is a binary indicator for whether the respondent reported having gone without cash income, respectively. These regressions include $CM_{i,t-1}$, weather anomalies, individual level controls, ADM1 fixed effects and country-year fixed effects. Standard errors are clustered at the ADM1 level for Columns (2) to (4), and at the cell level for Column (1); Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

2019; Yang, 2022), thereby limiting employment opportunities and lowering the opportunity cost of protest.

Table 4.7 reports the results. Using nighttime light intensity as a proxy for economic activity (Gibson et al., 2020), Columns (1) and (2) show that Chinese ownership has no additional effect on average and dispersion of luminosity.[49] Then, Columns (3) and (4) use Afrobarometer data to examine individual economic outcomes. Again, the point estimates suggest that there is no significant additional effect of Chinese investment on individual employment and poverty.[50]

Perceived Corruption and Trust Another mechanism considers whether increased corruption links Chinese mining to social unrest. The resource curse liter-

[49] Calculated as $Gini = \frac{\sum_{i=1}^{n}\sum_{j=1}^{m}|x_i - x_j|}{2N^2\bar{x}}$, where x_i denotes average nighttime light density for cell i, $\bar{x}$ the ADM1 mean, and N the number of cells.

[50] In Column (3), the outcome variable is constructed based on responses to "Do you have a job that pays a cash income?" Respondents answering "No, not looking" or "No, looking" were coded as unemployed. In Columns (4), respondents were coded as cash income poor if they report going without enough cash income at least once in the past year.

ature suggests that natural resource rents can weaken institutions and foster rent-seeking, often through corruption (e.g., Karl, 1997; Ploeg, 2011; Savoia and Sen, 2021). Empirical evidence similarly indicates that mining can heighten corruption in settings with weak governance (Knutsen et al., 2017; Mavisakalyan and Minasyan, 2025). Given repeated allegations that Chinese firms engage in bribery to secure mining contracts (Sun et al., 2017; Bath and Gu, 2024), Chinese investment in critical minerals may exacerbate local corruption and erode trust in local political actors, thereby increasing the risk of protest.

To test this mechanism, I utilize Afrobarometer data to measure perceived corruption across government actors.[51] Panel (a) of Table 4.8 shows that Chinese ownership is not positively associated with higher perceived levels of corruption. Similarly, as shown in panel (b) of Table 4.8, Chinese investment does not statistically influence local trust in various government actors.[52] However, as the Afrobarometer only provides less granular geolocation for respondents, the null or negative effects of Chinese investment on perceived corruption about and local trust in political actors may be subjected to aggregation bias.

Alternative mechanisms Existing studies and policy reports suggest several alternative mechanisms. First, Chinese mining firms have been accused of poor labour practices, including unsafe working conditions, excessive working hours, anti-union practices, wage disputes, and abusive treatment of local workers, which may provoke labour mobilization and strikes (e.g., Carciotto and Chikohomero, 2022). Second, concerns that Chinese investment gives foreign actors control over strategic resources and weakens national sovereignty may fuel neo-colonial narratives and anti-Chinese sentiment (e.g., Langan, 2017). Third, mining operations may generate social unrest through land appropriation, displacement, and disputes over compensation and resettlement (e.g., Amnesty International, 2018). However, data limitations prevent me from examining these mechanisms directly. I therefore acknowledge that local responses to Chinese critical-mineral mining investment may operate through alternative channels beyond environmental degradation and health effects.

[51] These measures are based on responses to the question: "How many of the following people do you think are involved in corruption?", covering the police, local councilors, government officials, judges, elected leaders, and the president. Responses were coded on a four-point scale ranging from 0 to 3, corresponding to "none", "some of them", "most of them", and "all of them", respectively.

[52] Based on Afrobarometer responses to "How much do you trust each of the following?" Including the police, local government, ruling party, president, and parliament, coded from "not at all", "a little bit", "a lot", and "a very good deal", respectively.

Table 4.8: Mechanism III: Corruption and Trust, Afrobarometer

	(1)	(2)	(3)	(4)	(5)	(6)
a: Perceived corruption about	Police	Local	Official	Judge	Parliament	President
$CM_{i,t-1}$	-0.106*	0.053	0.079**	-0.018	0.003	-0.075
	(0.056)	(0.037)	(0.035)	(0.070)	(0.037)	(0.049)
$CM^{\text{China}}_{i,t-1}$	0.052	-0.060**	-0.120***	-0.008	0.057	0.018
	(0.057)	(0.029)	(0.029)	(0.067)	(0.035)	(0.041)
Observations	51,158	49,538	49,855	48,720	47,719	48,102
ADM1 FE	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓
Weather Controls	✓	✓	✓	✓	✓	✓
Individual Controls	✓	✓	✓	✓	✓	✓
Adjusted R-squared	0.065	0.076	0.070	0.069	0.114	0.129
Mean of dependent variable	1.770	1.392	1.497	1.455	1.421	1.383
Number of ADM1 regions	57	57	57	57	57	57

	(7)	(8)	(9)	(10)	(11)
b: Trust in	Police	Local Officials	Ruling Party	Parliament	President
$CM_{i,t-1}$	0.089	-0.053	0.018	0.011	0.042
	(0.082)	(0.072)	(0.056)	(0.062)	(0.073)
$CM^{\text{China}}_{i,t-1}$	-0.121	-0.050	-0.039	-0.064	-0.062
	(0.074)	(0.078)	(0.047)	(0.058)	(0.064)
Observations	53,185	51,724	52,552	50,978	52,806
Cell FE	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓
Weather Controls	✓	✓	✓	✓	✓
Adjusted R-squared	0.119	0.147	0.199	0.171	0.195
Mean of dependent variable	1.393	1.443	1.527	1.521	1.764
Number of AMD1 region	57	57	57	57	57

Note: This table displays the estimates of the relationship between Chinese mining operations and various indicators of perceived corruption and trust in different types of institutions. All estimates are based on equation 4.1 with the unit of measurement is at individual-year levels. All dependent variables are constructed from the Afrobarometer survey data. The outcome variables in Columns (1) to (6) are a categorical variables constructed based on the response to the question of "How many of the following people do you think are involved in corruption, or haven't you heard enough about them to say?", referring to the police, local councilors, government officials, judge, elected leaders, and the president. These variables with values ranging from 0 to 3: 0 for "none", 1 for "some of them", 2 for "most of them", and 3 for "all of them". The outcome variables in Columns (7) to (11) are a categorical variables constructed based on the response to the question of "How much do you trust each of the following?", referring to police, local government body, ruling party, president, and parliament/national assembly. These variables with values ranging from 0 to 3: 0 for "not at all", 1 for "a little bit", 2 for "a lot", and 3 for "a very good deal". All regressions include $CM_{i,t-1}$, weather anomalies, ADM1 fixed effects, country-year fixed effects and individual controls. Standard errors (in parentheses) are clustered at the ADM1 levels; Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

4.7 Conclusion

China holds a dominant position in the energy transition supply chains and is rapidly expanding its investment in upstream extraction in sub-Saharan Africa. This paper provides the first piece of evidence on the effect of Chinese investment on local protest in the host community.

The findings are threefold. Firstly, using a triple-differences research design, this study demonstrates that the commencement of operations at Chinese-owned critical mineral mines leads to a significant increase in both the incidence and intensity of local protests the host area, beyond the effects observed in regions hosting

non-Chinese critical mineral mining projects. These results suggest that Chinese ownership exerted a distinctive effect on local protest dynamics. The estimated effects remain robust across extensive sensitivity analyses and randomization inference tests.

Secondly, this paper examines heterogeneity across institutional contexts. Host country with higher levels of democracy, stronger control of corruption, and better regulatory quality significantly mitigates the impact of Chinese critical mineral extraction on protest outcomes, underscoring the importance of local regulatory capacity in shaping Chinese firm's behavior abroad.

Third, this paper investigates the underlying mechanisms. The evidence points to environmental degradation as the primary channel: Chinese critical mineral extraction is associated with higher $PM_{2.5}$ concentrations, reduced vegetation cover, and increased deforestation in host regions. Additional results suggest that these environmental impacts translate into adverse child health outcomes. Moreover, Chinese mining operations are positively associated with environment-related protests, and protest responses are stronger in areas with higher environmental risks. Taken together, these findings suggest that environmental degradation is a key mechanism linking Chinese mining investment to local protest. By contrast, we find no significant evidence that Chinese mining increases perceived corruption or reduces local economic well-being.

Overall, this paper provides new evidence on the relationship between Chinese investment in critical minerals and local protest in host communities. The results underscore the importance of host-country governance in mitigating the adverse effects generated by Chinese mining activities. Particularly, stronger environmental safeguards can mitigate the negative environmental impacts of Chinese mining and thereby reduce social tensions between local communities and Chinese operators. Several limitations remain. First, the analysis focuses on conflict-related outcomes and does not examine broader socio-economic dimensions, such as education or labor market effects. The recent surge in Chinese investment in critical minerals, together with data limitations, constrains the exploration of these outcomes. Second, opacity in the sector hampers the systematic identification of artisanal and small-scale mining operations under Chinese control, which are likely underrepresented in the dataset. Third, due to limited individual-level survey coverage, the analysis is restricted to sub-Saharan Africa, even though Chinese mining activities are expanding in other regions, including Latin America. Future research should extend the empirical scope to address these limitations.

References

Acemoglu, D., García-Jimeno, C., & Robinson, J. A. (2012). Finding Eldorado: Slavery and long-run development in Colombia. *Journal of Comparative Economics*, *40*(4), 534–564.

Agüero, J. M., Balcázar, C. F., Maldonado, S., & Ñopo, H. (2021). The value of redistribution: Natural resources and the formation of human capital under weak institutions. *Journal of Development Economics*, *148*, 102581.

Ahn, D. P., & Ludema, R. D. (2020). The sword and the shield: The economics of targeted sanctions. *European Economic Review*, *130*, 103587.

Aihounton, G. B., & Henningsen, A. (2021). Units of measurement and the inverse hyperbolic sine transformation. *Econometrics Journal*, *24*(2), 334–351.

Alesina, A., Devleeschauwer, A., Easterly, W., Kurlat, S., & Wacziarg, R. (2003). Fractionalization. *Journal of Economic Growth*, *8*, 155–194.

Amnesty International. (2018). *"our lives mean nothing": The human cost of Chinese mining in Nagonha, Mozambique* (Report No. AFR 41/7851/2018). Amnesty International. https://www.amnesty.org/en/documents/afr41/7851/2018/en/

Aragón, F. M., & Rud, J. P. (2013). Natural resources and local communities: Evidence from a Peruvian gold mine. *American Economic Journal: Economic Policy*, *5*(2), 1–25.

Aragón, F. M., & Rud, J. P. (2016). Polluting industries and agricultural productivity: Evidence from mining in Ghana. *Economic Journal*, *126*(597), 1980–2011.

Arellano-Yanguas, J. (2011). Aggravating the resource curse: Decentralisation, mining and conflict in Peru. *Journal of Development Studies*, *47*(4), 617–638.

Arezki, R., & van der Ploeg, F. (2025). Escaping the critical minerals curse. *CEPR Policy Insight*, (147).

Associated Press. (2025). *A river 'died' overnight in Zambia after an acidic waste spill at a Chinese-owned mine*. Retrieved May 28, 2025, from https://www.politico.com/news/2025/03/16/a-river-died-overnight-in-zambia-after-an-acidic-waste-spill-at-a-chinese-owned-mine-00232234

Atkinson, R., Kang, S., Anderson, H., Mills, I., & Walton, H. (2014). Epidemiological time series studies of pm2. 5 and daily mortality and hospital admissions: A systematic review and meta-analysis. *Thorax*, *69*(7), 660–665.

Autor, D., Dorn, D., & Hanson, G. H. (2013). The China syndrome: Local labor market effects of import competition in the United States. *American Economic Review*, *103*(6), 2121–2168.

Babatunde, A. O., Asante, R., Asunka, J., Eisenman, J., Egu Lansana, F., Naidu, S., Ogbonnaya, I., Ouoba, Y., & Sangare, B. (2025). *Chinese mining in west africa: Responding to the environmental and social impacts* (Issue Brief). Atlantic Council. Retrieved February 15, 2026, from https://www.atlanticcouncil.org/in-depth-research-reports/issue-brief/chinese-mining-in-west-africa/

Bath, V., & Gu, T. (2024). Foreign investment, investment treaties and corruption in China and Hong Kong. In N. Teramura, L. Nottage, & B. Jetin (Eds.), *Corruption and illegality in Asian investment arbitration* (pp. 209–234). Springer Nature Singapore.

Bazillier, R., & Girard, V. (2020). The gold digger and the machine. evidence on the distributive effect of the artisanal and industrial gold rushes in Burkina Faso. *Journal of Development Economics*, *143*, 102411.

Bazzi, S., & Blattman, C. (2014). Economic shocks and conflict: Evidence from commodity prices. *American Economic Journal: Macroeconomics*, *6*(4), 1–38.

Bellemare, M. F., & Wichman, C. J. (2020). Elasticities and the inverse hyperbolic sine transformation. *Oxford Bulletin of Economics and Statistics*, *82*(1), 50–61.

Bennon, M., & Fukuyama, F. (2022). The obsolescing bargain crosses the belt and road initiative: Renegotiations on bri projects. *Oxford Review of Economic Policy*, *38*(2), 278–301.

Benshaul-Tolonen, A. (2019). Local industrial shocks and infant mortality. *Economic Journal*, *129*(620), 1561–1592.

Berman, N., Couttenier, M., Mathieu, Rohner, D., & Thoenig, M. (2017). This mine is mine! How minerals fuel conflicts in Africa. *American Economic Review*, *107*(6), 1564–1610.

Bertinelli, L., Cömertpay, R., & Maystadt, J.-F. (2025). Ethnic diversity and conflict in sub-Saharan Africa: Evidence from refugee-hosting areas. *Journal of Development Economics*, *172*, 103393.

Bezzola, S., Günther, I., Brugger, F., & Lefoll, E. (2022). CSR and local conflicts in African mining communities. *World Development*, *158*, 105968.

Bleiberg, J. (2021). *STACKEDEV: Stata moduler* [Statistical Software Components S459027]. Boston College Department of Economics.

Bornschier, S., & Vogt, M. (2024). The politics of extractivism: Mining, institutional responsiveness, and social resistance. *World Development*, *176*, 106493.

Brautigam, D., Diao, X., McMillan, M. S., & Silver, J. (2019). *Chinese investment in Africa: How much do we know?* (Synthesis Paper No. 2). Centre for Economic Policy Research and Private Enterprise Development in Low-Income Countries. London. Retrieved April 28, 2025, from https://pedl.cepr.org/publications/chinese-investment-africa-how-much-do-we-know

Bräutigam, D., & Gallagher, K. P. (2014). Bartering globalization: China's commodity-backed finance in Africa and Latin America. *Global Policy*, *5*(3), 346–352.

Burke, M., Hsiang, S. M., & Miguel, E. (2015a). Climate and conflict. *Annual Review of Economics*, *7*(1), 577–617.

Burke, M., Hsiang, S. M., & Miguel, E. (2015b). Global non-linear effect of temperature on economic production. *Nature*, *527*(7577), 235–239.

Burke, M., Ferguson, J., Hsiang, S., & Miguel, E. (2024). New evidence on the economics of climate and conflict. *Handbook of the Economics of Conflict*, *1*, 249–305.

Carciotto, S., & Chikohomero, R. (2022). *Chinese labour practices in six southern african countries* (Monograph No. 207). Institute for Security Studies. Retrieved June 4, 2026, from https://issafrica.s3.amazonaws.com/site/uploads/Mono-207.pdf

Caselli, F., & Michaels, G. (2013). Do oil windfalls improve living standards? Evidence from Brazil. *American Economic Journal: Applied Economics*, *5*(1), 208–238.

Cengiz, D., Dube, A., Lindner, A., & Zipperer, B. (2019). The effect of minimum wages on low-wage jobs. *Quarterly Journal of Economics*, *134*(3), 1405–1454.

Chen, J., & Roth, J. (2024). Logs with zeros? some problems and solutions. *Quarterly Journal of Economics*, *139*(2), 891–936.

Chen, W., Dollar, D., & Tang, H. (2018). Why is China investing in Africa? Evidence from the firm level. *World Bank Economic Review*, *32*(3), 610–632.

Chen, Y. (2025). *Can China fix the problems with transition mineral mining abroad?* (Accessed 2025-08-12). Global Witness. https://globalwitness.org/en/campaigns/transition-minerals/can-china-fix-the-problems-with-transition-mineral-mining-abroad/

Christensen, D. (2019). Concession stands: How mining investments incite protest in Africa. *International Organization*, *73*(1), 65–101.

Clarke, D. (2021). *RWOLF2: Stata module* [Statistical Software Components S458970]. Boston College Department of Economics.

Colella, F., Lalive, R., Sakalli, S. O., & Thoenig, M. (2019). Inference with arbitrary clustering. *IZA Discussion Paper n. 12584*.

Conley, T. G. (1999). GMM estimation with cross sectional dependence. *Journal of Econometrics*, *92*(1), 1–45.

Coulomb, R., Dietz, S., Godunova, M., & Nielsen, T. B. (2015). *Critical minerals today and in 2030: An analysis for OECD countries*. Organisation for Economic Co-operation and Development. Retrieved March 27, 2025, from https://www.oecd.org/content/dam/oecd/en/publications/reports/2015/09/critical-minerals-today-and-in-2030_g17a26fa/5jrtknwm5hr5-en.pdf

Couttenier, M., & Rohner, D. (2025). Critical minerals and conflict: A policy roadmap. *CEPR Discussion Paper*, (20881).

de Freitas, M. V. (2023). *The impact of Chinese investments in Africa: Neocolonialism or cooperation?* Policy Center for the New South. Retrieved March 7, 2025, from https://www.policycenter.ma/publications/impact-chinese-investments-africa-neocolonialism-or-cooperation

De Chaisemartin, C., & d'Haultfoeuille, X. (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review*, *110*(9), 2964–2996.

De Chaisemartin, C., & d'Haultfoeuille, X. (2023). Two-way fixed effects and differences-in-differences with heterogeneous treatment effects: A survey. *Econometrics Journal*, *26*(3), C1–C30.

Didan, K. (2015). *MOD13A3 vegetation indices monthly, v006* (Data set). Data set. NASA LP DAAC. https://doi.org/10.5067/MODIS/MOD13A3.006

Dreher, A., Fuchs, A., Parks, B., Strange, A., & Tierney, M. J. (2021). Aid, China, and growth: Evidence from a new global development finance dataset. *American Economic Journal: Economic Policy*, *13*(2), 135–174.

Dreher, A., Fuchs, A., Parks, B., Strange, A., & Tierney, M. J. (2022). *Banking on beijing: The aims and impacts of china's overseas development program.* Cambridge University Press.

Dube, O., & Vargas, J. F. (2013). Commodity price shocks and civil conflict: Evidence from Colombia. *Review of Economic Studies*, *80*(4), 1384–1421.

Egbetokun, A. (2024). *Despite the drawbacks, Chinese investments in Africa are worth it. just.* Retrieved March 28, 2025, from https://blogs.lse.ac.uk/africaatlse/2024/07/31/despite-the-drawbacks-chinese-investments-in-africa-are-worth-it-just/

Eichenauer, V. Z., & Wang, F. (2024). Mild deglobalization: Foreign investment screening and cross-border investment. *CESifo Working Paper Series*, (2265).

Escobar, B., Malik, A. A., Zhang, S., et al. (2025). *Power playbook: Beijing's bid to secure overseas transition minerals.* AidData at William & Mary. Williamsburg, VA. Retrieved March 28, 2025, from https://aiddata.org/publications/power-playbook-beijings-bid-to-secure-overseas-transition-minerals

Esteban, J., & Ray, D. (2008). Polarization, fractionalization and conflict. *Journal of Peace Research*, *45*(2), 163–182.

Ferguson, G. (2017). China's deliberate non-enforcement of foreign corruption: A practice that needs to end. *The International Lawyer*, *50*(3), 503–528.

Frattaroli, M. (2020). Does protectionist anti-takeover legislation lead to managerial entrenchment? *Journal of Financial Economics*, *136*(1), 106–136.

Friedl, M., & Sulla-Menashe, D. (2022). *MCD12Q1 land cover type, v061* (Data set). Data set. NASA LP DAAC. https://doi.org/10.5067/MODIS/MCD12Q1.061

Funk, C., Peterson, P., Landsfeld, M., Pedreros, D., Verdin, J., Shukla, S., Husak, G., Rowland, J., Harrison, L., Hoell, A., et al. (2015). The climate hazards infrared precipitation with stations—a new environmental record for monitoring extremes. *Scientific data*, *2*(1), 1–21.

Gallagher, K. S., & Qi, Q. (2018). *Policies governing China's overseas development finance: Implications for climate change.* Center for International Environment and Resource Policy, The Fletcher School, Tufts University. Retrieved April 2, 2025, from https://bpb-us-e1.wpmucdn.com/sites.tufts.edu/dist/3/7323/files/2018/03/CPL_ChinaOverseasDev.pdf

Gaulier, G., & Zignago, S. (2010). Baci: International trade database at the product-level. the 1994-2007 version. *CEPII Working Papers*, (2010–23). https://www.cepii.fr/CEPII/fr/publications/wp/abstract.asp?NoDoc=2726

Gibson, J., Olivia, S., & Boe-Gibson, G. (2020). Night lights in economics: Sources and uses. *Journal of Economic Surveys*, *34*(5), 955–980.

Girard, V., Molina-Millán, T., & Vic, G. (2025). Artisanal mining in Africa. Green for Gold? *Economic Journal*, *135*(672), 2578–2597.

Goodman, S., Zhang, S., Malik, A. A., Parks, B. C., & Hall, J. (2024). Aiddata's geospatial global Chinese development finance dataset. *Scientific Data*, *11*(1), 529.

Hansen, M. C., Potapov, P. V., Moore, R., Hancher, M., Turubanova, S. A., Tyukavina, A., Thau, D., Stehman, S. V., Goetz, S. J., Loveland, T. R., et al. (2013). High-resolution global maps of 21st-century forest cover change. *Science*, *342*(6160), 850–853.

Harris, I., Osborn, T. J., Jones, P., & Lister, D. (2020). Version 4 of the CRU TS monthly high-resolution gridded multivariate climate dataset. *Scientific Data*, *7*(1), 109.

Heft-Neal, S., Burney, J., Bendavid, E., & Burke, M. (2018). Robust relationship between air quality and infant mortality in Africa. *Nature*, *559*(7713), 254–258.

Hodler, R., Lechner, M., & Raschky, P. A. (2023a). Institutions and the resource curse: New insights from causal machine learning. *PLOS ONE*, *18*(6), e0284968.

Hodler, R., Schaudt, P., & Vesperoni, A. (2023b). Mining for peace. *CESifo Working Paper Series No. 10207*.

Högbladh, S. (2025). *UCDP GED codebook, version 25.1*. Data set codebook. Uppsala University.

Hsiang, S. M., Burke, M., & Miguel, E. (2013). Quantifying the influence of climate on human conflict. *Science*, *341*(6151), 1235367.

Iacoella, F., Martorano, B., Metzger, L., & Sanfilippo, M. (2021). Chinese official finance and political participation in Africa. *European Economic Review*, *136*, 103741.

Imai, K., Keele, L., & Tingley, D. (2010). A general approach to causal mediation analysis. *Psychological methods*, *15*(4), 309.

Imbens, G. W., & Wooldridge, J. M. (2009). Recent developments in the econometrics of program evaluation. *Journal of Economic Literature*, *47*(1), 5–86.

Inamuco, I. (2022). *Suspension of Chinese miner for pollution in DRC points to wider problem*. Mongabay. Retrieved May 28, 2025, from https://news.mongabay.com/2022/06/suspension-of-chinese-miner-for-pollution-in-drc-points-to-wider-problem/

Inclusive Development International. (2022). *Regulating Chinese overseas investment and finance* [Retrieved March 12, 2025, from https://www.inclusivedevelopment.net/china-global-program/china-global-newsletter-edition-6/].

International Energy Agency. (2025). Critical minerals policy tracker: Explore key policies related to critical minerals needed for the energy transition [Accessed 2025-02-28]. https://www.iea.org/data-and-statistics/data-tools/critical-minerals-policy-tracker

Karl, T. L. (1997). *The paradox of plenty: Oil booms and petro-states* (Vol. 26). Univ of California Press.

Kim, T.-Y., Gould, T., Bennett, S., et al. (2021). *The role of critical minerals in clean energy transitions*. International Energy Agency. Retrieved March 2, 2025, from https://iea.blob.core.windows.net/assets/ffd2a83b-8c30-4e9d-980a-52b6d9a86fdc/TheRoleofCriticalMineralsinCleanEnergyTransitions.pdf

Kim-Leffingwell, S., Shen, S., Yang, Y., et al. (2024). Money backfires: How Chinese investment fuels anti-China protests abroad. *World Development*, *178*, 106566.

Knutsen, C. H., Kotsadam, A., Olsen, E. H., & Wig, T. (2017). Mining and local corruption in Africa. *American Journal of Political Science*, *61*(2), 320–334.

Langan, M. (2017). Emerging powers and neo-colonialism in Africa. In *Neo-colonialism and the poverty of 'development' in Africa* (pp. 89–117). Springer.

Lebakula, V., Sims, K., Reith, A., et al. (2025). Landscan global annual gridded population, 2000–2022. *Scientific Data*, *12*(1), 495.

Lee, C. K. (2018). *The specter of global China: Politics, labor, and foreign investment in Africa*. University of Chicago Press.

Leuven, E., & Sianesi, B. (2003). *PSMATCH2: Stata module* [Statistical Software Components S432001, revised 01 Feb 2018]. Boston College Department of Economics. https://EconPapers.repec.org/RePEc:boc:bocode:s432001

Li, X., Zhou, Y., Zhao, M., & Zhao, X. (2020). A harmonized global nighttime light dataset 1992–2018. *Scientific data*, *7*(1), 168.

Links, S., de Feijter, T., & Lammertink, J. (2021). *Chinese approaches to overseas responsible business: Insights from the DRC cobalt industry*. LeidenAsiaCentre. Retrieved April 12, 2025, from https://www.leidenasiacentre.nl/wp-content/uploads/2021/10/Chinese-Approaches-to-Overseas-Responsible-Business.pdf

Mamo, N., Bhattacharyya, S., & Moradi, A. (2019). Intensive and extensive margins of mining and development: Evidence from Sub-Saharan Africa. *Journal of Development Economics*, *139*, 28–49.

Marysse, S., & Geenen, S. (2009). Win-win or unequal exchange? the case of the sino-congolese cooperation agreements. *The Journal of Modern African Studies*, *47*(3), 371–396.

Maus, V., Giljum, S., da Silva, D. M., Gutschlhofer, J., da Rosa, R. P., Luckeneder, S., Gass, S. L., Lieber, M., & McCallum, I. (2022). An update on global mining land use. *Scientific Data*, *9*(1), 433.

Mavisakalyan, A., & Minasyan, A. (2025). Mining and mistrust in government. *European Economic Review*, *175*, 105002.

Maystadt, J.-F., De Luca, G., Sekeris, P. G., & Ulimwengu, J. (2014). Mineral resources and conflicts in DRC: A case of ecological fallacy? *Oxford Economic Papers*, *66*(3), 721–749.

Maystadt, J.-F., Tsang, K., Van Biesebroeck, J., & Warrinnier, N. (2024). Targeting the corps: The incidence of humanitarian sanctions on firm performance in China. *CEPR Discussion Paper*, (18790).

McCauley, J. F., Pearson, M. M., & Wang, X. (2022). Does Chinese FDI in Africa inspire support for a china model of development? *World Development*, *150*, 105738.

McGuirk, E., & Burke, M. (2020). The economic origins of conflict in Africa. *Journal of Political Economy*, *128*(10), 3940–3997.

Mejía, L. B. (2020). Mining and human capital accumulation: Evidence from the Colombian gold rush. *Journal of Development Economics*, *145*, 102471.

Mien, E., & Goujon, M. (2021). 40 years of dutch disease literature: Lessons for developing countries. *Comparative Economic Studies*, *64*(3), 351.

Miguel, E., Satyanath, S., & Sergenti, E. (2004). Economic shocks and civil conflict: An instrumental variables approach. *Journal of Political Economy*, *112*(4), 725–753.

Mohr, C., & Trebesch, C. (2025). Geoeconomics. *Annual Review of Economics*, *17*, 563–587.

Mol, A. P. (2011). China's ascent and Africa's environment. *Global Environmental Change*, *21*(3), 785–794.

Montalvo, J. G., & Reynal-Querol, M. (2005). Ethnic polarization, potential conflict, and civil wars. *American Economic Review*, *95*(3), 796–816.

National Bureau of Statistics of China. (2025). *Net overseas direct investment stock at the end of this year*. Retrieved June 4, 2025, from https://data.stats.gov.cn/english/easyquery.htm?cn=C01

Nunn, N., & Qian, N. (2014). Us food aid and civil conflict. *American Economic Review*, *104*(6), 1630–1666.

Olden, A., & Møen, J. (2022). The triple difference estimator. *Econometrics Journal*, *25*(3), 531–553.

Ploeg, F. v. d. (2011). Natural resources: Curse or blessing? *Journal of Economic Literature*, *49*(2), 366–420.

RAID. (2009). *Chinese mining operations in Katanga: Mining workers and community rights in the Democratic Republic of the Congo*. Rights and Accountability in Development. Retrieved March 12, 2025, from https://raid-uk.org/wp-content/uploads/2023/03/drc-china-report.pdf

RAID. (2021). *The road to ruin? electric vehicles and workers' rights abuses at DRC's industrial cobalt mines*. RAID and Centre d'Aide Juridico-Judiciaire. Retrieved March 2, 2025, from https://www.raid-uk.org/wp-content/uploads/2023/03/report_road_to_ruin_evs_cobalt_workers_nov_2021.pdf

Ramdoo, I., et al. (2024). *What makes minerals and metals "critical"?: A practical guide for governments on building resilient supply chains*. IISD/IGF. Retrieved March 5, 2025, from https://www.iisd.org/system/files/2024-05/igf-what-makes-minerals-metals-critical.pdf

Ross, M. L. (2015). What have we learned about the resource curse? *Annual Review of Political Science*, *18*(1), 239–259.

Runfola, D., Anderson, A., Baier, H., Crittenden, M., Dowker, E., Fuhrig, S., Goodman, S., Grimsley, G., Layko, R., Melville, G., Mulder, M., Oberman, R., Panganiban, J., Peck, A., Seitz, L., Shea, S., Slevin, H., Youngerman, R.,

& Hobbs, L. (2020). Geoboundaries: A global database of political administrative boundaries. *PLOS ONE*, *15*(4).

Sabbe, B. (2022). *Grievances, governance and gold in the Eastern DRC* (IPIS Briefing). International Peace Information Service. Retrieved February 5, 2025, from https://ipisresearch.be/weekly-briefing/ipis-briefing-december-2021-january-2022-grievances-governance-and-gold-in-the-eastern-drc/

Sändig, J., Hönke, J., & Kabemba, C. (2024). Contention and concessions: Exploring the responsiveness of Chinese mining companies in Africa. *Extractive Industries and Society*, *20*, 101535.

Savoia, A., & Sen, K. (2021). The political economy of the resource curse: A development perspective. *Annual Review of Resource Economics*, *13*(1), 203–223.

Shinn, D. H. (2016). The environmental imact of China's investment in Africa. *Cornell Int'l LJ*, *49*, 25.

Sonno, T. (2025). Globalization and conflicts: The good, the bad, and the ugly of corporations in Africa. *Economic Journal*, ueae103.

Stoop, N., Verpoorten, M., & Van Der Windt, P. (2019). Artisanal or industrial conflict minerals? Evidence from Eastern Congo. *World Development*, *122*, 660–674.

Sun, I. Y., & Tang, X. (2016). Social responsibility or development responsibility-what is the environmental impact of chinese investments in Africa: What are its drivers, and what are the possibilities for action. *Cornell Int'l LJ*, *49*, 69.

Sun, I. Y., Jayaram, K., & Kassiri, O. (2017). *Dance of the lions and dragons: How Africa and China are engaging and how will the partnership evolve?* McKinsey & Company. Retrieved March 1, 2025, from https://www.mckinsey.com/featured-insights/middle-east-and-africa/the-closest-look-yet-at-chinese-economic-engagement-in-africa

Sun, L. (2021). *EVENTSTUDYINTERACT: Stata module* [Statistical Software Components S458978, revised 11 Sep 2022]. Boston College Department of Economics. https://ideas.repec.org/c/boc/bocode/s458978.html

Sun, L., & Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, *225*(2), 175–199.

Sundberg, R., & Melander, E. (2013). Introducing the UCDP georeferenced event dataset. *Journal of Peace Research*, *50*(4), 523–532.

Tang, L., & Werner, T. T. (2023). Global mining footprint mapped from high-resolution satellite imagery. *Communications Earth & Environment*, *4*(1), 134.

Van Donkelaar, A., Hammer, M. S., Bindle, L., et al. (2024). *Global annual PM2.5 grids, v5.gl.04* (Data set). Data set. Palisades, NY, NASA SEDAC. https://doi.org/10.7927/AS2R-9P42

Vanden Eynde, O., & Vargas, J. (2025). Climate change, natural resources, and conflict. *Economic Policy*, *40*(123), 651–677.

Voeten, E. (2009). *United nations general assembly ideal points* (Data set; Version V37). Data set. Harvard Dataverse. https://doi.org/10.7910/DVN/LEJUQZ

Von der Goltz, J., & Barnwal, P. (2019). Mines: The local wealth and health effects of mineral mining in developing countries. *Journal of Development Economics*, *139*, 1–16.

Wang, X., Pearson, M. M., & McCauley, J. F. (2022). Foreign direct investment, unmet expectations, and the prospects of political leaders: Evidence from Chinese investment in Africa. *Journal of Politics*, *84*(3), 1403–1419.

Wegenast, T., Krauser, M., Strüver, G., & Giesen, J. (2019). At Africa's expense? disaggregating the employment effects of chinese mining operations in sub-Saharan Africa. *World Development*, *118*, 39–51.

Weidmann, N. B., Rød, J. K., & Cederman, L.-E. (2010). Representing ethnic groups in space: A new dataset. *Journal of Peace Research*, *47*(4), 491–499.

World Resources Institute. (2023). *Aqueduct global maps 4.0* (Data set). Data set. Retrieved June 22, 2025, from https://datasets.wri.org/dataset/aqueduct-global-maps-4

Yang, Y. (2022). Bring your own workers: Chinese OFDI, Chinese overseas workers, and collective labor rights in Africa. *World Development*, *152*, 105808.

Yuan, P., Li, D., Feng, K., Wang, H., Wang, P., & Li, J. (2024). Assessing the supply risks of critical metals in china's low-carbon energy transition. *Global Environmental Change*, *86*, 102825.

Zhou, W., Crochet, V., & Wang, H. (2025). Demystifying China's critical minerals strategies: Rethinking 'de-risking'supply chains. *World Trade Review*, 1–25.

Appendix 4.A Dataset of Chinese critical mineral investment

The dataset is compiled involving three steps. I begin with identify 695 Chinese companies engaged in mining activities both domestically and abroad. First, I use firm-level data from the China Stock Market & Accounting Research (CSMAR) database to develop an automated keyword-based algorithm, which identifies 519 A-share listed firms involved in the extraction, processing, or trading of critical minerals. I then supplement this list with 63 additional firms listed on the Hong Kong Stock Exchange. For example, Jinchuan Group International Resources, which is only listed in Hong Kong, operates several critical mineral extraction projects in Africa. Finally, I add 22 privately held firms identified through government documents and industry reports, such as the China-Africa Development Fund. The resulting dataset focuses on large-scale, industrial mining operations and does not capture artisanal or small-scale activities involving Chinese companies.

Second, for each identified company, I track the specific critical mineral mining projects overseas in which they are involved. This is based on information disclosed in firm's press releases and periodic reports as well as the reports from government, industry, and NGO. For example, Zijin Mining Group lists its overseas investment projects on its corporate website (see Figure 4.A.1), while MMG Limited provides details of its international operations in its annual report (see Figure 4.A.2). A total of 131 Chinese companies own at least one critical mineral mine worldwide.

After identifying a mining project operated or financed by a Chinese firm, I manually collect data on its geographical coordinates, ownership structure, development status, ownership structure, and type of extracted mineral. To enhance data reliability, each project entry is cross-validated using at least two independent sources. In case, the information from two sources are not aligned and I seek the third information sources to confirm. Moreover, geographical coordinates are validated using satellite imagery and mapping tools such as Google Maps and Bing Maps.

In total, I identify 206 projects under Chinese ownership, with Chinese firms holding at least a 50% ownership stake in 145 of them.[53] These mining operations are large scale with the average estimated resources amounts to 332 million tons (based on 160 projects with data available). Figure 4.A.3 presents a map illustrating the locations of all Chinese critical mineral mining operations. The top three countries receiving the largest share of Chinese investments are Australia, the Democratic Republic of the Congo, and South Africa. Notably, 102 operations are situated in sub-Saharan Africa.

Table 4.A.1 presents the distribution of Chinese investment projects by mineral type. In response to rising global demand for green technologies, key materials essential to these technologies—such as cobalt, copper, graphite, lithium, manganese, rare earth elements, and nickel—together account for approximately half of all Chinese investments. In addition, Chinese firms also invest substantially in gold and

[53] I include only mining operations that are either under construction or currently in production.

silver, iron ore, and bauxite.[54]

Using this dataset, three patterns of Chinese investment in global critical mineral extraction sector can be observed. Firstly, the expansion of Chinese firms have only been accelerated since the mid-2000s. Figure 4.3 depicts the evolution of Chinese ownership in overseas critical mineral mining projects. Although Chinese companies began investing in overseas mining projects in the 1990s, their presence remained limited for several years. A notable increase in activity began in the mid-2000s, initially concentrated in sub-Saharan African countries. Since 2010, two key trends have emerged. First, Chinese involvement in critical global mineral mining projects has accelerated, leading to a substantial increase in the cumulative number of projects with Chinese ownership. By 2023, the total number of projects reached 206, more than double since 2010. Second, Chinese investment has expanded beyond sub-Saharan Africa to include other regions, notably developed countries such as Australia and Canada. Nonetheless, sub-Saharan Africa still plays a central role.

Secondly, Chinese state-owned enterprises (SOEs) are major players in the critical mineral extraction sector, owning 138 projects globally. Australia and sub-Saharan African countries remain the primary recipients of these investments. Moreover, on average, projects owned by Chinese SOEs are significantly larger. The mean estimated resource volume is 3.96 million tons for SOE-owned mines, compared to just 1.77 million tons for those owned by non-SOE counterparts.

Thirdly, I find no significant evidence that Chinese investment in the critical mineral extraction sector is influenced by a country's governance quality. This conclusion is based on a country-level analysis using two outcome variables: (1) a binary indicator equal to one if a country hosted at least one Chinese investment project as of 2023, and (2) the total number of projects hosted by a country as of 2023. I regress each of these outcomes on individual governance indicators from the World Governance Indicators, one at a time, while controlling for a set of macroeconomic variables.[55] Figure 4.A.4 presents the estimated coefficients for each governance indicator at both the extensive and intensive margins, none of which are statistically significant. This finding is consistent with the results reported in Chen et al. (2018) that Chinese investments tend to be indifferent to governance quality of recipient country. To examine the protest impacts of Chinese investments in critical mineral mining, I compile a project-level dataset on Chinese owned critical mineral mining operations using publicly available sources.

[54]Since there is no standardized definition of critical minerals and various countries have created their own lists based on strategic and economic priorities (Ramdoo et al., 2024), I adopt a broader definition in this dataset. Accordingly, I include also mines producing gold, silver, and iron ore, consistent with broader classifications in the literature (for example, Coulomb et al. (2015)).

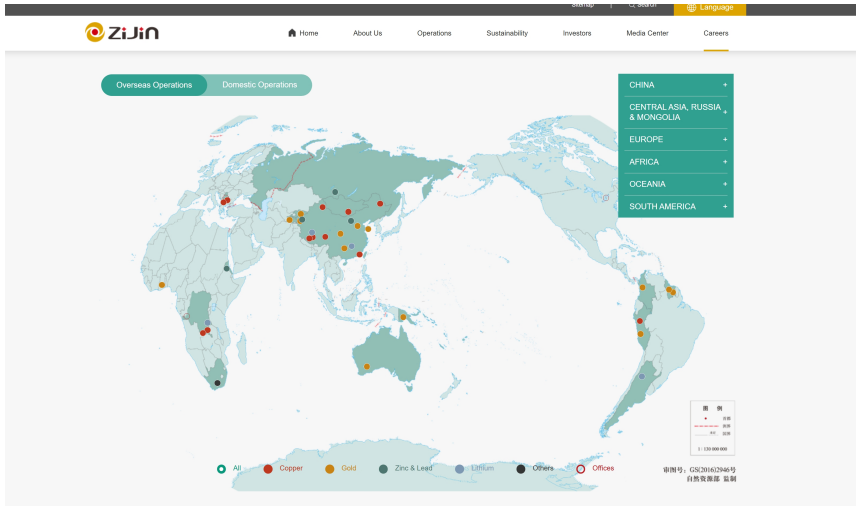
[55]There are six governance indicators, and I use their average values from 1996 to 2003 in the analysis. The macroeconomic controls include the IHS-transformed real GDP per capita, real GDP growth, the FDI-to-GDP ratio, and the IHS-transformed population, all measured as of 2002.

Table 4.A.1: List of Critical Minerals Included

Critical Mineral	Number of Mines with Chinese ownership
Bauxite	13
Chromite	1
Cobalt & Copper	70
Gold & Silver	38
Graphite	2
Heavy Mineral Sands	3
Iron ore	19
Lithium	15
Manganese	8
Nickel	7
Niobium	2
Phosphate	1
Platinum Group Metals (PGMs)	2
Tin	2
Titanium	9
Tungsten	1
Zinc	12
Zircon	1

Note: Information in this table is compiled by the author.

Figure 4.A.1: Example I: Operations of Zijin Mining Group



Note: This is the screenshot of the homepage of Zijin Mining Group.
Access: https://www.zijinmining.com/global/Zijin-Overseas.htm

Figure 4.A.2: Example II: Operations of MMG Limited

Mineral Resources and Ore Reserves Continued

Mineral Resources[1]

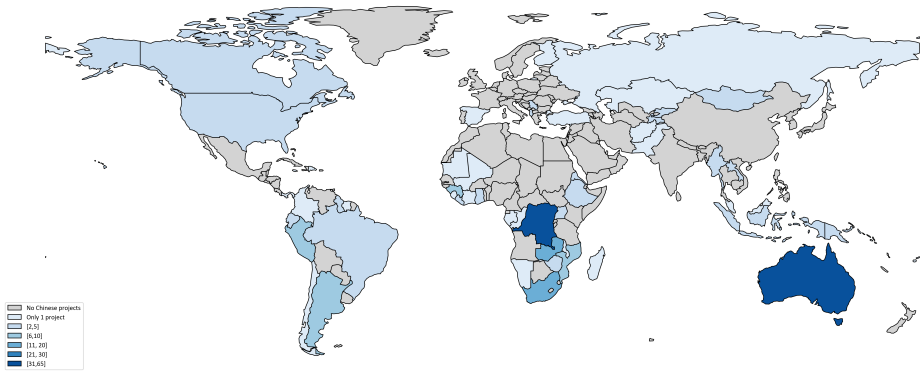
All data reported here is on a 100% asset basis, with MMG's attributable interest shown against each asset within brackets.

	2023								2022							
Deposit	Tonnes (Mt)	Cu (%)	Zn (%)	Pb (%)	Ag (g/t)	Au (g/t)	Mo (ppm)	Co (%)	Tonnes (Mt)	Cu (%)	Zn (%)	Pb (%)	Ag (g/t)	Au (g/t)	Mo (ppm)	Co (%)
Las Bambas (62.5%)																
Ferrobamba Oxide Copper																
Indicated	0.02	1.3							0.03	1.7						
Inferred																
Total	**0.02**	**1.3**							**0.03**	**1.7**						
Ferrobamba Primary Copper																
Measured	380	0.59			2.6	0.05	220		470	0.56			2.3	0.04	210	
Indicated	220	0.66			3.2	0.06	180		270	0.70			3.3	0.06	180	
Inferred	39	0.80			2.8	0.07	190		110	0.84			4.2	0.08	170	
Total	**640**	**0.63**			**2.8**	**0.05**	**200**		**850**	**0.64**			**2.9**	**0.05**	**190**	
Ferrobamba Total	**640**								**850**							
Chalcobamba Oxide Copper																
Indicated	6.2	1.4							6.8	1.4						
Inferred	0.53	1.2							0.1	1.5						
Total	**6.7**	**1.4**							**6.9**	**1.4**						
Chalcobamba Primary Copper																
Measured	150	0.51			1.5	0.02	120		140	0.54			1.7	0.02	140	
Indicated	190	0.60			2.2	0.03	120		180	0.64			2.5	0.03	110	
Inferred	43	0.47			1.9	0.02	100		29	0.56			2.4	0.03	130	
Total	**380**	**0.55**			**1.9**	**0.02**	**120**		**340**	**0.60**			**2.1**	**0.03**	**120**	
Chalcobamba Total	**387**								**347**							
Sulfobamba Primary Copper																
Indicated	93	0.62			4.4	0.02	140		84	0.67			4.7	0.02	170	
Inferred	110	0.54			6.0	0.02	64		98	0.58			6.5	0.02	120	
Total	**210**	**0.58**			**5.2**	**0.02**	**98**		**180**	**0.62**			**5.7**	**0.02**	**140**	
Sulfobamba Total	**210**	**0.58**			**5.2**	**0.02**	**98**		**180**	**0.62**			**5.7**	**0.02**	**140**	
Oxide Copper Stockpile																
Indicated	14	1.1							14	1.1						
Total	**14**	**1.1**							**14**	**1.1**						
Sulphide Stockpile																
Measured	25	0.36			2.2		110		30	0.38			2.2		130	
Total	**25**	**0.36**			**2.2**		**110**		**30**	**0.38**			**2.2**		**130**	
Las Bambas Total	**1,300**								**1,400**							

1 S.I. units used for metals of value; Cu=copper, Zn=zinc, Pb=lead, Ag=silver, Au=gold, Mo=molybdenum, Co=cobalt.

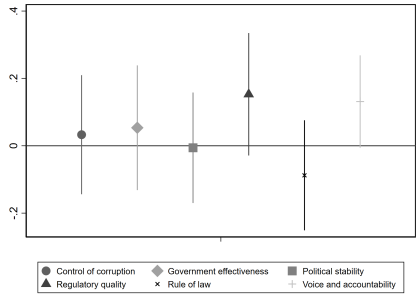
Note: This is the screenshot of the the annual report 2023 of MMG Limited.
Access: https://www.mmg.com/wp-content/uploads/2024/04/MMG-Annual-Report-2023-Full.pdf

Figure 4.A.3: Map: Chinese-owned Critical Mineral Mining Operations, by 2023.

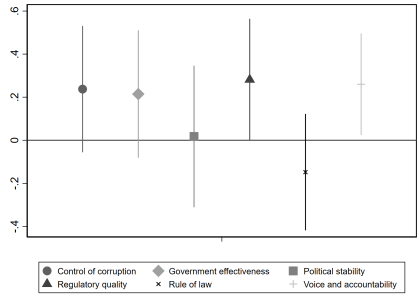


Note: This figure is compiled by the author.

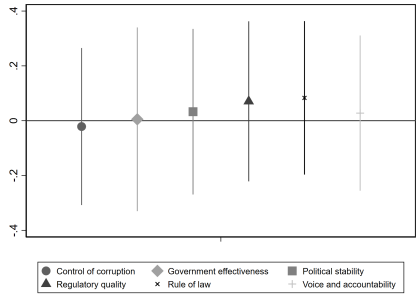
Figure 4.A.4: Governance of Country as Determinants



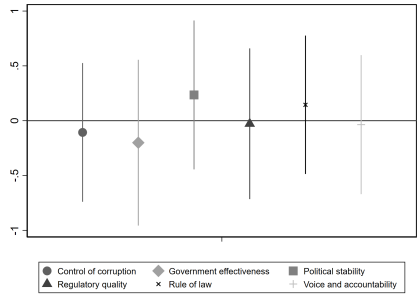
(a1) All countries (n=152): Probability of hosting a project



(a2) All countries (n=152): Number of projects



(b1) SSA countries (n=41): Probability of hosting a project



(b2) SSA countries (n=41): Number of projects

*Note:*Estimated coefficients and corresponding 90% confidence intervals on six different public governance indicators are plotted. Dependent variables are (a) a binary variable taking value of one if a country hosting at least one critical mineral extraction project owned by Chinese companies and (b) total number of projects (IHS transformed) for extensive and intensive margins respectively. All regressions include macroeconomic control variables of IHS-transformed real GDP per capita, real GDP growth, the FDI-to-GDP ratio, and the IHS-transformed population.

Appendix 4.B Mining Operations in analyses

Table 4.B.1: Countries Hosting Chinese Critical Mineral Mines

Country	Cells	With Chinese mines	With other mines
Congo, DRC	747	11	6
Cote d'Ivoire	102	1	7
Gabon	92	1	2
Ghana	75	3	8
Guinea	64	6	8
Liberia	35	1	4
Madagascar	230	1	2
Mauritania	362	1	3
Mozambique	275	5	3
Namibia	259	1	12
Sierra Leone	17	1	2
South Africa	389	8	39
Uganda	74	1	3
Zambia	229	6	7
Zimbabwe	110	1	26

*Note:*This table displays the spatial distribution of the sample grid cells. Column (1) presents the number of grid cells included in the baseline sample, by country. Columns (2) and (3) show the number of cells with Chinese critical mineral mines and non-Chinese critical mineral mines, respectively.

Table 4.B.2: Cells with Mining Activities, Type of Critical Minerals

Mineral	Cell with active Chinese mining operation	Cell with active non-Chinese mining operation	Usage in Energy Transition Technology
Bauxite	6	4	Solar, Wind, and EV
Chromite	1	2	Wind
Copper & Cobalt	15	15	Solar, Wind, Battery, and EV
Fluorspar	0	1	Solar
Gold & Silver	8	56	Solar
Graphite	1	3	Battery
Heavy Sand Metals	0	3	Wind
Iron Ore	3	11	Wind
Lithium	2	3	Battery
Manganese	4	6	Battery
Nickel	1	3	Battery
Niobium	0	1	Wind
PGMs	0	13	Battery
Phosphorus	1	2	Solar and Battery
Tin	0	2	Solar
Titanium	6	0	Solar and Wind
Tungsten	0	2	Battery
Zinc	0	4	Solar, Wind, and Battery
Zircon	0	1	Solar

Note: This table displays the distribution of the sample cells by types of commodity. Columns (1) and (2) display number of cells with operating mining activities with Chinese ownership and without Chinese ownership by types of critical mineral included in the analysis.

Appendix 4.C Afrobarometer Data and DHS Data

In Section 4.6, I utilize data from the second (2004) to the eighth wave (2022) of the Afrobarometer. This involves 10 sub-Saharan African countries spanning from 2003 to 2022. Total observations are 63,547. Table 4.C.1 presents the information regarding the observation number and the country involving in each wave of the Afrobarometer. Namibia and Zimbabwe are not included as there are no operating Chinese critical mineral mines before 2022.

Table 4.C.1: Waves of Afrobarometer

Country	Year(s)	Observations
Cote d'Ivoire	2013, 2014, 2016, 2017, 2019, 2021	3,112
Gabon	2015, 2017, 2020, 2021	2,336
Ghana	2005, 2008, 2012, 2014, 2017, 2019, 2022	10,947
Guinea	2013, 2015, 2017, 2019, 2022	4,604
Liberia	2008, 2012, 2015, 2018, 2020, 2022	2,022
Mozambique	2005, 2008, 2012, 2015, 2018, 2021, 2022	8,989
Sierra Leone	2012, 2015, 2018, 2020, 2022	1,998
South Africa	2006, 2008, 2011, 2015, 2018, 2021, 2022	5,465
Uganda	2005, 2008, 2011, 2012, 2015, 2016, 2017, 2019, 2022	10.839
Zambia	2005, 2009, 2012, 2013, 2014, 2017, 2020, 2022	4,120

Note: This table displays the spatial distribution of individual respondents for various waves of the Afrobarometer.

Moreover, in Section 4.6, I utilize data from the fourth (2003) to the nine wave (2023) of the DHS. This involves 10 sub-Saharan African countries spanning from 2003 to 2023. Total observations are 178,200. Table 4.C.2 presents the information regarding the observation number and the country involving in each wave of the DHS.

Table 4.C.2: Waves of DHS

Country	Year(s)	Observations
Cote d'Ivoire	2012, 2021	15,362
Congo, DRC	2007, 2013, 2023	34,185
Gabon	2012, 2019, 2020, 2021, 2022	11,956
Ghana	2008, 2014, 2019, 2022, 2023	16,898
Guinea	2005,2012,2018,2021	18,282
Liberia	2007,2013,2019 ,2022	13,347
Madagascar	2008,2021	12,631
Mozambique	2011, 2015, 2022, 2023	22,953
Sierra Leone	2008, 2013,2019	13,121
Zambia	2007, 2013, 2018	19,465

Note: This table displays the spatial distribution of individual respondents for various waves of the DHS.

Appendix 4.D Definition of Variables

Table 4.D.1: List of Variables

Variable	Description	Source
$Incidence_{it}$	Dummy variable equals to 1 if at least one social conflicts occurred in a cell in specific year.	ACLED
$Intensity_{it}$	Number of protest occurred in a cell in specific year. IHS transformed	ACLED
$Death_{it}$	Number of fatalities due to protest occurred in a cell in specific year. IHS transformed	ACLED
$CM_{i,t-1}$	Dummy variable equals to 1 if at least one mine operating in a cell in year $t-1$	S&P SNL Metals and Mining database & Hand collected by author
$CM^{\text{China}}_{i,t-1}$	Dummy variable equals to 1 if at least one mine owned by a Chinese company in a cell in year $t-1$.	Hand collected by author
$\#\ CM_{i,t-1}$	Total number of critical mineral mines operating in a cell in year $t-1$	S&P SNL Metals and Mining database & Hand collected by author
$\#\ CM^{\text{China}}_{i,t-1}$	Total number of critical mineral mines operating by a Chinese company in a cell in year $t-1$.	Hand collected by author
$Temperature_{it}$	Temperature anomalies in a cell in specific year. It is calculated as $\frac{Average\ temperature_{it} - Long\ term\ mean\ temperature_i}{Standard\ Deviation\ Long\ term\ mean\ temperature_i}$. $Long\ term\ mean\ temperature_i$ is the average temperature in cell i over the sample period.	CRU TS 4.08
$Rainfall_{it}$	Rainfall anomalies in a cell in specific year. It is calculated as $\frac{Average\ rainfall_{it} - Long\ term\ mean\ rainfall_i}{Standard\ Deviation\ Long\ term\ mean\ rainfall_i}$. $Long\ term\ mean\ rainfall_i$ is the average rainfall in cell i over the sample period.	CHIRPS v3.0
$Price_{it}$	Global mineral price, in constant 2010 USD.	World Bank Commodities Prices
$Nighttime\ Light_{i,t-1}$	Average harmonized night time light density in cell i in year $t-1$. IHS transformed.	Li et al. (2020)
$Population_{i,t-1}$	Population density in cell i in year $t-1$. IHS transformed.	LandScan
$Aid_{i,t-1}$	Number of Chinese aid projects in cell i in year $t-1$. IHS transformed.	Goodman et al. (2024)
$Polity\ V_c$	Dummy variable equals to 1 if PolityV index (1996−2001) in a country exceeds the sample median..	PolityV Project
$Corruption_c$	Dummy variable equals to 1 if control of corruption (1996−2001) in a country exceeds the sample median.	WGI
$Effectiveness_c$	Dummy variable equals to 1 if government effectiveness (1996−2001) in a country exceeds the sample median.	WGI
$Stability_c$	Dummy variable equals to 1 if political stability (1996−2001) in a country exceeds the sample median.	WGI
$Regulation_c$	Dummy variable equals to 1 if regulatory quality (1996−2001) in a country exceeds the sample median.	WGI
$Rule\ of\ law_c$	Dummy variable equals to 1 if rule of law (1996−2001) in a country exceeds the sample median.	WGI
$Voice_c$	Dummy variable equals to 1 if voice and accountability (1996 −2001) in a country exceeds the sample median.	WGI
$Water\ Stress_i$	Dummy variable equals to 1 if base estimate of water stress in a cell exceeds the sample median.	World Resources Institute
$Cropland\ Share_i$	Dummy variable equals to 1 if cropland share in a cell exceeds the sample median in 2003.	MCD12Q1.061
$UN\ Voting_i$	Dummy variable equals to 1 if a country's ideal-point distance estimates from China in UN General Assembly voting (all votes) is below the sample median.	Voeten (2009)
$Trade_i$	Dummy variable equals to 1 if a country's trade share with China is above the sample median.	BACI
$Ethnic\ fractionalization_i$	Dummy variable equals to 1 if a cell's fractionalization index is above the sample median. Grid level ethnic fractionalization index: $EF_i = 1 - \sum_{n=1}^{N} Ps_n^2$. Ps_n is the population share of an ethnic group n in cell i.	GREG & Landscan
$Ethnic\ polarization_i$	Dummy variable equals to 1 if a cell's polarization index is above the sample median. Grid level ethnic polarization index: $RQ_i = 1 - \sum_n [\frac{0.5 - Ps_n}{0.5}]^2 Ps_n$. Ps_n is the population share of an ethnic group n in cell i.	GREG & Landscan
SOE_i	Dummy variable equals to 1 if more than half of Chinese mining projects in a cell are owned by Chinese state-owned enterprises.	Hand collected by author
$Size_i$	HS-transformed total mineral resources of Chinese projects in a cell.	Hand collected by author

List of Variables, Continued

Variable	Description	Source
$PM2.5_{it}$	Average PM2.5 particles density in cell i in specific year.	SEDAC's global PM2.5 data (V5.GL.04)
$NDVI_{it}$	Average normalized difference vegetation index over first three months of the wet season in cell i in specific year. Rescaled to range from -100 to 100	MOD13 C2 & RADS
$Deforestation$	Number of newly deforested pixel within a cell i in specific year. Scaled by 10,000.	GFC V1.11
$Corruption\ police_{ht}$	Categorical variable for question of whether "Polices" are corrupted.	Afrobarometer
$Corruption\ local_{ht}$	Categorical variable for question of whether "Local councilors" are corrupted.	Afrobarometer
$Corruption\ official_{ht}$	Categorical variable for question of whether "Officials" are corrupted.	Afrobarometer
$Corruption\ judge_{ht}$	Categorical variable for question of whether "Judges" are corrupted.	Afrobarometer
$Corruption\ elected_{ht}$	Categorical variable for question of whether "Elected leaders" are corrupted.	Afrobarometer
$Corruption\ president_{ht}$	Categorical variable for question of whether "President" is corrupted.	Afrobarometer
$Trust\ police_{ht}$	Categorical variable for question of whether "Polices" are trusted.	Afrobarometer
$Trust\ local_{ht}$	Categorical variable for question of whether "Local officials" are trusted.	Afrobarometer
$Trust\ ruling_{ht}$	Categorical variable for question of whether "Ruling Party" is trusted.	Afrobarometer
$Trust\ president_{ht}$	Categorical variable for question of whether "President" is trusted.	Afrobarometer
$Trust\ parliament_{ht}$	Categorical variable for question of whether "Parliament" is corrupted.	Afrobarometer
$Night\ Time\ Light_{it}$	Average harmonized night time light density in cell i in specific year. IHS transformed.	Li et al. (2020)
$Gini\ Coefficient$	Within ADM1 region Gini coefficient of average night time light density in ADMI region in specific year.	Li et al. (2020)
$Unemployed_{ht}$	Dummy variable equals to 1 if respondent h was unemployed in year t. Only individuals with age lower than or equal to 65.	Afrobarometer
$Cash\ Poverty_{ht}$	Dummy variable equals to 1 if respondent h answered that he / she had gone without cash "just once or twice", "several times", "many times" or "always" in year t	Afrobarometer
$Stunting_{ct}$	Dummy variable equals to 1 if height-for-age Z-score below -2.	DHS
$Underweight_{ct}$	Dummy variable equals to 1 if weight-for-age Z-score below -2.	DHS
$Wastingct$	Dummy variable equals to 1 if weight-for-height Z-score below -2.	DHS
$Cough_{ct}$	Dummy variable equals to 1 if had cough in last two weeks.	DHS
$Rapid\ Breathing_{ct}$	Dummy variable equals to 1 if had short, rapid breaths in last two weeks.	DHS
$Anemia_{ct}$	Dummy variable equals to 1 if had any levels of anemia	DHS
$Diarrhea_{ct}$	Dummy variable equals to 1 if had diarrhea recently	DHS
$Fever_{ct}$	Dummy variable equals to 1 if had fever in last two weeks	DHS
$Mother\ age_{ct}$	Age of child's mother	DHS
$Mother\ age\ squared_{ct}$	Age-squared of child's mother	DHS
$Mother\ education_{ct}$	Educational level of child's mother	DHS
$Mother\ urban_{ct}$	Dummy variable equals to 1 if child's mother lived in urban area	DHS
$Wealth_{ct}$	Household's wealth index, 5 categories	DHS
$Number\ of\ children_{ct}$	Household's number of living children	DHS
$Elevation$	Mean of elevation for cell i	Harmonized world soil database v1.2
$Ruggedness$	Standard deviation of elevation for cell i	Harmonized world soil database v1.2
$Average\ Malaria\ Infection$	Average number of newly diagnosed Plasmodium falciparum cases, within cell i between 2000 - 2001.	Malaria Atlas Project
$Total\ railway$	Total length (km) of railway within cell i as of 2000. IHS transformed	DIVA-GIS
$Total\ road$	Total length (km) of road within cell i as of 2000. IHS transformed	DIVA-GIS
$Distance\ to\ coast$	Total distance (km) to the coast. IHS transformed	Natural Earth

Appendix 4.E Summary Statistics

Table 4.E.1: Distribution of Protest Outcomes: Types of Cells

	Obs.	mean	sd	median	P75	P90	P95	Max
Panel A: With Chinese mines								
Prob(Protest > 0)	363	0.49	0.50	0.00	1.00	1.00	1.00	1.00
# of Protest	363	3.85	10.42	0.00	3.00	9.00	19.00	88
ihs(# of Protest)	363	0.98	1.27	0.00	1.82	2.89	3.64	5.17
# of Death	315	0.65	2.65	0.00	0.00	1.00	4.00	27.00
ihs(# of Death)	315	0.27	0.70	0.00	0.00	0.88	2.09	3.99
Panel B: With other mines								
Prob(Protest > 0)	1,955	0.32	0.47	0.00	1.00	1.00	1.00	1.00
# of Protest	1,955	2.74	14.01	0.00	1.00	4.00	11.00	235
ihs(# of Protest)	1,955	0.58	1.08	0.00	0.88	2.09	3.09	6.15
# of Death	1,830	0.29	1.87	0.00	0.00	0.00	1.00	50.00
ihs(# of Death)	1,830	0.13	0.48	0.00	0.00	0.00	0.88	4.61
Panel C: Controls								
Prob(Protest > 0)	58,822	0.07	0.26	0.00	0.00	0.00	1.00	1.00
# of Protest	58,822	0.37	4.04	0.00	0.00	0.00	1.00	194
ihs(# of Protest)	58,822	0.11	0.48	0.00	0.00	0.00	0.88	5.96
# of Death	55,938	0.05	0.61	0.00	0.00	0.00	0.00	48.00
ihs(# of Death)	55,938	0.02	0.20	0.00	0.00	0.00	0.00	4.56

Note: This table reports the distribution of protest outcomes by types of cells. IHS denotes the inverse hyperbolic sine transformation. Protest probability is an indicator equal to one if at least one protest occurs in a cell-year.

Table 4.E.2: Summary Statistics: Balanced Table, Pre-treatment Characteristics

	Pre-treatment mean of Greenfield Chinese mines (n = 39)	**Normalized difference** (n = 91)	**p-value on t-stat**
Panel A: Time Invariant Variables			
Elevation (meters)	555.82	-0.21	0.00
Ruggedness (meters)	65.75	-0.25	0.00
Ethnic Polarization (RQ), 2000	0.66	-0.02	0.80
Ethnic Fractionalization, 2000	0.17	0.00	0.80
Average Malaria Infection, 2000-2001	0.41	0.32	0.00
Total Railway Length (ihs(km)), 2000	2.46	0.25	0.00
Total Road Length (ihs(km)), 2000	5.70	0.22	0.00
Distance to Coast (ihs(km))	7.88	0.28	0.00
Panel B: Time Variant Variables			
ihs($nighttime\ light_{t-1}$)	0.67	0.00	0.99
ihs($population_{t-1}$)	11.88	0.26	0.00
Prob($Protest_{t-1} > 0$)	0.08	0.00	0.95
ihs(# of $Protest_{t-1}$)	0.11	0.00	0.90
ihs(# of $Death_{t-1}$)	0.02	0.00	0.72
$Temperature\ Anomaly_{t-1}$	-0.15	-0.10	0.19
$Precipitation\ Anomaly_{t-1}$	0.02	0.02	0.85

Note: This table reports selected characteristics of the cells prior to hosting greenfield Chinese critical mineral mining operations (n=39) and hosting non-Chinese mines (n=52). Columns (2) presents the mean values for cells with greenfield Chinese mines. Columns (3) presents the normalized differences relative to cells with non-Chinese critical mineral mining operations, calculated following the method proposed by Imbens and Wooldridge (2009). It is defined as $ND = \frac{\bar{X}_T - \bar{X}_C}{\sqrt{\frac{s_T^2 + s_C^2}{2}}}$ where, as a general rule of thumb, $|ND| < 0.25$ is regarded as indicating of covariate balance between groups. Columns (4) shows the scale-dependent p-values based on t-statistics from mean comparisons between two groups.

Table 4.E.3: Summary Statistics for Variables in Section 4.5

	With Chinese mines			With other mines			Control		
	Obs.	Mean	sd	Obs.	Mean	sd	Obs.	Mean	sd
Panel A: Control Variables									
Precipitation Anomaly	363	0.03	0.95	1955	0.05	0.94	58822	0.01	0.98
Temperature anomaly	363	0.26	1.05	1955	0.07	1.03	58822	0.04	1.00
ihs($nighttime\ light_{t-1}$)	363	2.01	1.08	1955	1.61	1.17	58822	0.55	0.88
ihs($population_{t-1}$)	363	12.70	1.41	1955	11.91	2.06	58822	10.12	2.95
Panel B: Variables for heterogeneity analysis in Section 4.5.4									
Polity V	363	0.57	0.50	1955	0.56	0.50	58822	0.48	0.50
Corruption	363	0.47	0.50	1955	0.56	0.50	58822	0.55	0.50
Regulation	363	0.47	0.50	1955	0.60	0.49	58822	0.53	0.50
Effective	363	0.47	0.50	1955	0.54	0.50	58822	0.55	0.50
Stability	363	0.34	0.47	1955	0.27	0.45	58822	0.51	0.50
Rule of law	363	0.52	0.50	1955	0.60	0.49	58822	0.56	0.50
Voice	363	0.58	0.49	1955	0.59	0.49	58822	0.63	0.48
UN Voting	363	0.32	0.47	1955	0.54	0.50	58822	0.34	0.47
Trade	363	0.59	0.49	1955	0.72	0.45	58822	0.38	0.48
high EF	363	0.45	0.50	1955	0.43	0.49	58822	0.39	0.49
high RQ	363	0.45	0.50	1955	0.43	0.49	58822	0.39	0.49
SOE	363	0.65	0.48	1955	0.01	0.09	58822	0.01	0.08
IHS(Size)	245	18.36	3.21	1955	0.21	1.83	58580	0.21	1.97

*Note:*This table displays the summary statistics of variables in section 4.5. Columns (1) to (3) are the summary statistics for cells with hosting at least one Chinese operated critical mineral mines. Columns (4) to (6) represent the summary statistics for cells with hosting non-Chinese mines. Lastly, Columns (7) to (9) show the summary statistics for all control cells.

Table 4.E.4: Summary Statistics for Variables in Section 4.6

	With Chinese mines			With other mines			Control		
	Obs.	mean	sd	Obs.	mean	sd	Obs.	mean	sd
Panel A: Cell-level outcome variables									
PM2.5($\mu g/m^3$)	363	21.95	7.43	1955	21.09	9.17	58782	23.82	11.93
Deforestation	363	3.11	3.90	1955	1.30	2.92	58822	0.78	1.75
NDVI	351	43.05	30.85	1876	22.74	35.28	55033	26.47	46.91
asinh(Nighttime light)	363	2.00	1.09	1955	1.64	1.17	58822	0.56	0.88
Within ADMI Gini Coefficient	195	0.67	0.22	135	0.80	0.19	1630	0.74	0.24
Panel B: Cell-level variables for heterogeneity analysis in Table 4.6									
Water Stress	363	0.52	0.50	1955	0.76	0.43	58822	0.49	0.50
Cropland	363	0.35	0.48	1955	0.34	0.47	58822	0.12	0.33
Panel C: Individual-level variables from the Afrobarometer									
Corruption: Police	12644	1.77	0.88	17987	1.75	0.89	33349	1.80	0.90
Corruption: Local councilors	12293	1.50	0.85	17540	1.47	0.87	32165	1.35	0.83
Corruption: Officials	12362	1.53	0.82	17616	1.52	0.83	32413	1.49	0.83
Corruption: Judge	12105	1.45	0.86	17204	1.44	0.88	31680	1.45	0.87
Corruption: Parliament	11795	1.52	0.84	16697	1.48	0.86	31192	1.39	0.85
Corruption: President	11998	1.46	0.90	17026	1.40	0.92	31250	1.37	0.91
Trust: Police	13046	1.31	1.12	18544	1.34	1.12	34840	1.43	1.12
Trust: Local Officials	12657	1.32	1.08	18064	1.34	1.08	33838	1.52	1.07
Trust: Ruling Party	12877	1.44	1.16	18321	1.48	1.16	34419	1.60	1.16
Trust: President	12894	1.64	1.17	18276	1.70	1.17	34727	1.84	1.15
Trust: Parliament	12340	1.40	1.10	17491	1.45	1.10	33670	1.59	1.09
Unemployed	12299	0.69	0.46	17090	0.67	0.47	31869	0.66	0.48
Cash Poverty	13196	0.79	0.40	18775	0.77	0.42	35380	0.81	0.39
Urban	13250	1.56	0.50	18846	1.53	0.50	35586	1.57	0.50
Education	13188	1.51	0.83	18768	1.51	0.83	35432	1.40	0.82
Gender	13250	1.50	0.50	18846	1.50	0.50	35586	1.50	0.50
Age-level	13250	1.74	1.10	18846	1.77	1.12	35586	1.72	1.09
Panel C: Individual-level variables from the DHS									
Stunting	2042	0.34	0.48	3926	0.29	0.45	72167	0.35	0.48
Underweight	2053	0.14	0.35	3940	0.13	0.33	71813	0.17	0.38
Wasting	2041	0.04	0.21	3933	0.06	0.24	71691	0.07	0.25
Cough	4424	0.21	0.41	8359	0.19	0.39	129772	0.22	0.42
Rapid Breathing	3500	0.09	0.29	5259	0.12	0.33	82542	0.13	0.34
Anemia	1950	0.62	0.49	4098	0.66	0.47	56342	0.66	0.47
Diarrhea	4419	0.14	0.34	8362	0.13	0.34	129720	0.15	0.36
Fever	4888	0.19	0.39	9205	0.21	0.40	135240	0.22	0.42
Mother: Age	5295	29.22	7.07	10057	28.86	7.20	148281	29.03	7.26
Mother: Age-squared	5295	903.98	434.95	10057	884.72	440.50	148281	895.56	447.37
Mother: Education	5295	1.04	0.93	10057	0.95	0.90	148274	0.92	0.85
Mother: Urban	5295	0.36	0.48	10057	0.36	0.48	148281	0.29	0.45
Household: Wealth index	5295	3.16	1.40	10057	2.74	1.25	148281	2.43	1.28
Household: Number of living children	5295	3.57	2.15	10057	3.30	1.98	148281	3.49	2.10

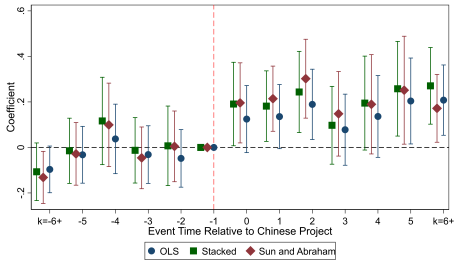
Note: This table displays the summary statistics of variables used in section4.6. Columns (1) to (3) are the summary statistics for cells with ever hosting at least one Chinese operated critical mineral mines. Columns (4) to (6) represent the summary statistics for cells with ever hosting non-Chinese mines. Lastly, Columns (7) to (9) show the summary statistics for all control cells. In panel B, mining areas are aggregated to the ADM1 region level.

Appendix 4.F Additional Figures

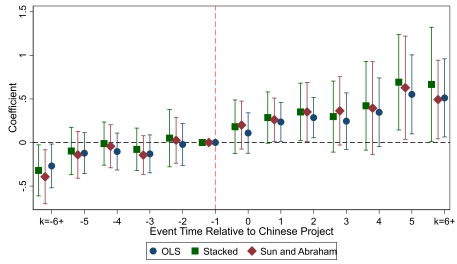
Figure 4.F.1: Event Study: Two double-differences, by Ownership

(a) Chinese Critical Mineral Mines vs No Mining Area

(i) Probability of Protest

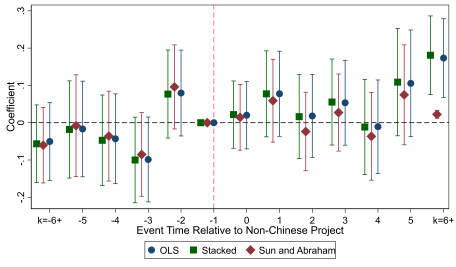


(ii) Number of Protests, IHS transformed

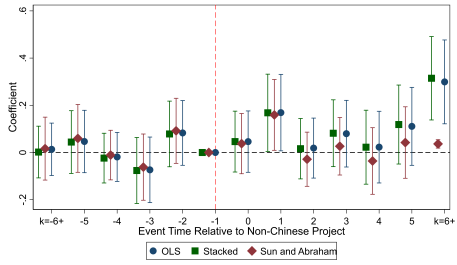


(b) Non-Chinese Critical Mineral Mines vs No Mining Area

(i) Probability of Protest



(ii) Number of Protests, IHS transformed

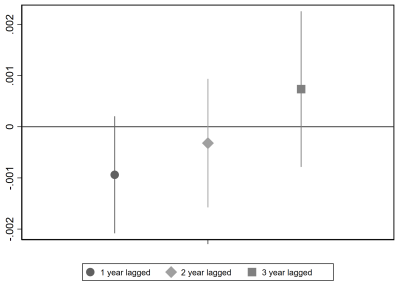


*Note:*This figure presents event-study estimates from two separate difference-in-differences specifications. It plots the individual event-time coefficients associated with CM_i^{China} in panel (a) and CM_i in panel (b), together with their corresponding 95% confidence intervals. Event time is defined as $k = t - \tau_i - 1$ to match the baseline specification that uses one-year lagged treatment indicators and $k = -1$ is omitted as the reference period. I bin all years before $k = -6$ and after $k = 6$ at $k = -6$ and $k = 6$ respectively. Panel A compares cells that eventually host Chinese-owned critical mineral mines to never-mining cells, using the year Chinese operations began as the treatment onset. Panel B compares cells with eventually non-Chinese mines to never-mining cells, with treatment defined by the initial year of non-Chinese operations. All models control for weather anomalies and include cell and country-year fixed effects. Standard errors are clustered at the cell level.

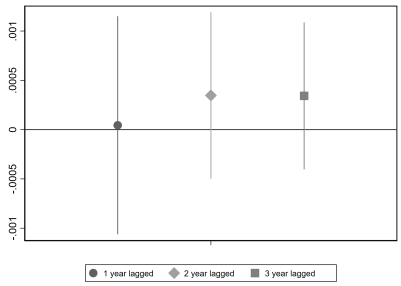
Figure 4.F.2: No Reverse Causality: Chinese Ownership as Dependent Variable

(a) Lagged Protest at the cell

(i) Lagged Probability of Protest

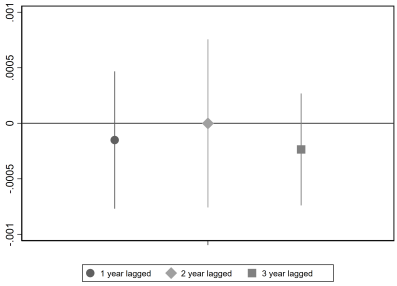


(ii) Lagged Number of Protests, IHS transformed

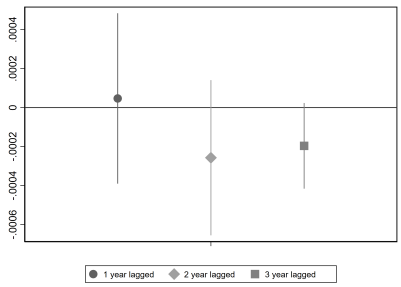


(b) Lagged Protest at the ADM2 region (excluding the focal cell)

(iii) Lagged Probability of Protest

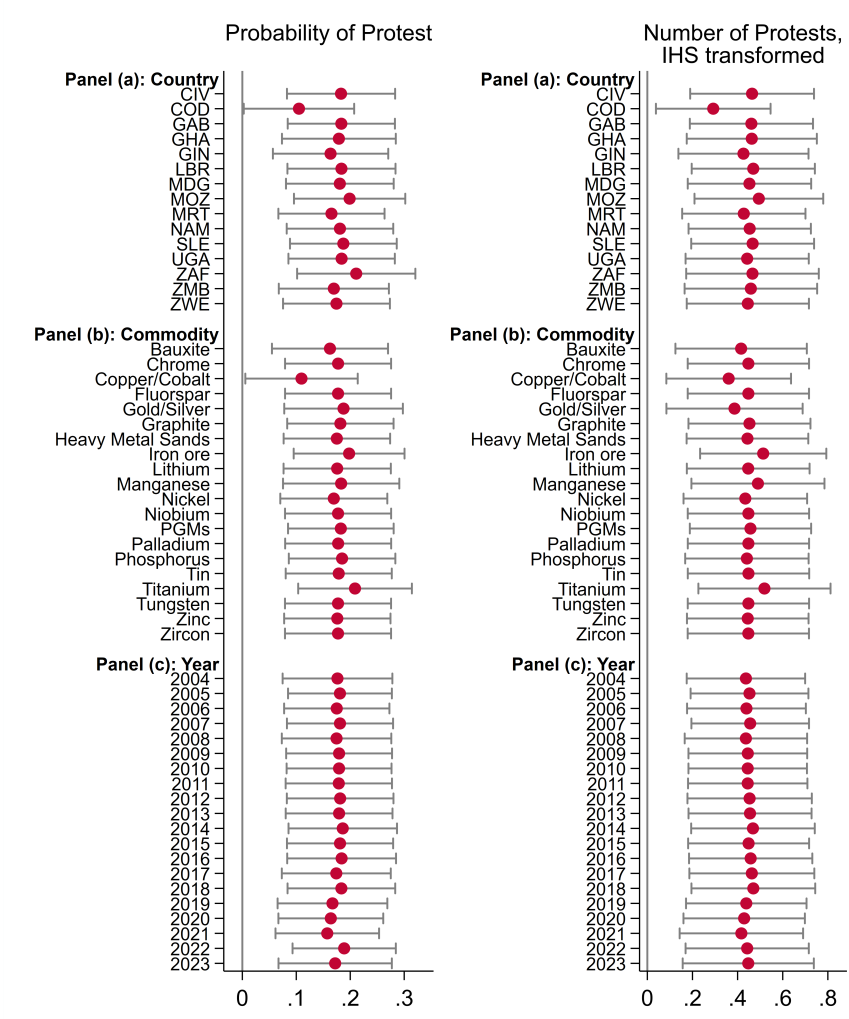


(iv) Lagged Number of Protests, IHS transformed



*Note:*This figure presents the estimated coefficients with the corresponding 90% confidence interval of lagged probability of protest and lagged number of protest at the focal cell in Panel (a) and at the ADM2 region level (excluding the focal cell) in Panel (b) on a binary variable of Chinese ownership. Chinese ownership equals to one if a Chinese company started to own the first project in a cell i. All years after the ownership year in that specific cell i are dropped. All regressions include $CM_{i,t-1}$, weather anomalies, and a full set of cell fixed effects and country-year fixed effects. Standard errors are clustered at the cell levels.

Figure 4.F.3: Sensitivity Check: Leave-One-Out Checks by Country, Commodity, and Year

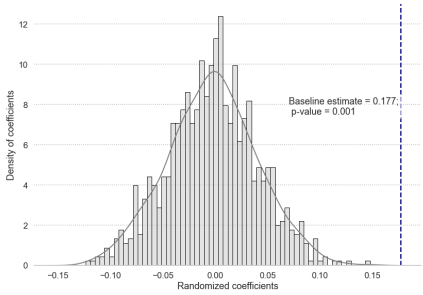


*Note:*This figure reports leave-one-out checks based on the baseline specification. Dots represent point estimates on $CM_{i,t-1}^{\text{China}}$ and horizontal bars report 95% confidence intervals. Panel (a) sequentially excludes one country, Panel (b) excludes one commodity, and Panel (c) excludes one year group. In eacn panel, the left column reports estimates for probability of protest, and the right column reports estimates for the IHS-transformed number of protests. All regressions include $CM_{i,t-1}$, weather anomalies, and a full set of cell fixed effects and country-year fixed effects. Standard errors are clustered at the cell levels.

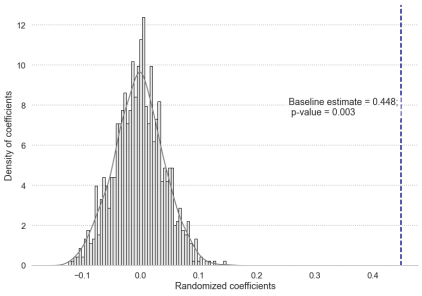
Figure 4.F.4: Permutation Tests

(a) Randomized Operating Years for Mining Cells

(i) Probability of Protest

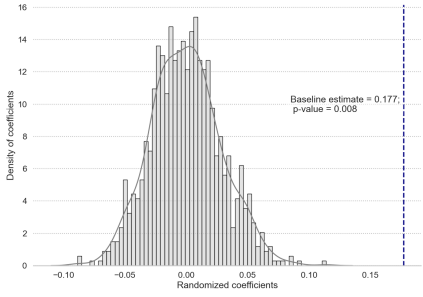


(ii) Number of Protests, IHS transformed

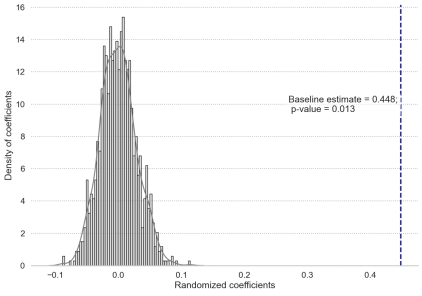


(b) Randomized Location and Operation Years for All Observations

(iii) Probability of Protest

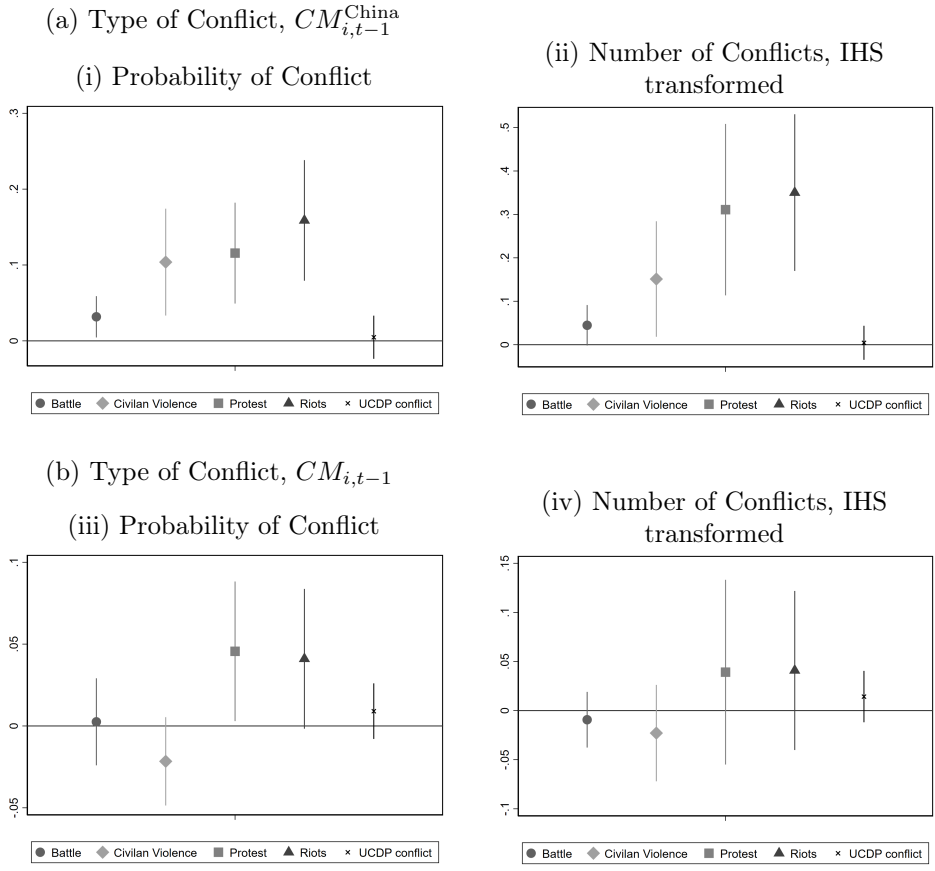


(iv) Number of Protests, IHS transformed



Note: This figure presents the distribution of 1,000 estimated coefficients on the interaction term $CM^{\text{China}}_{i,t-1}$, derived from Equation (4.1). In Panel (a), the randomization process permutes the operation year of critical mineral mining projects within all mining cells. In Panel (b), the randomization process jointly permutes the location and operation years of critical mineral mining projects for all observations. Columns (i) and (iii) display the resulting distributions for protest incidence and Columns (ii) and (iv) display protest intensity, respectively. The vertical dashed lines indicate the baseline estimates reported in Columns (3) and (4) of Table 4.1. The associated p-values are calculated as the share of randomized t-statistics that exceed the observed t-statistic in absolute magnitude. Standard errors are clustered at the cell levels.

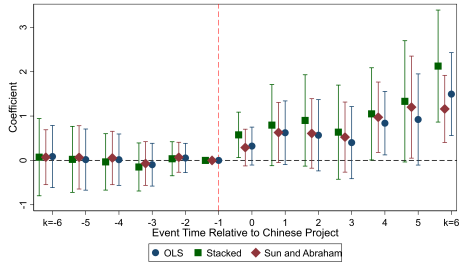
Figure 4.F.5: Type of Conflict



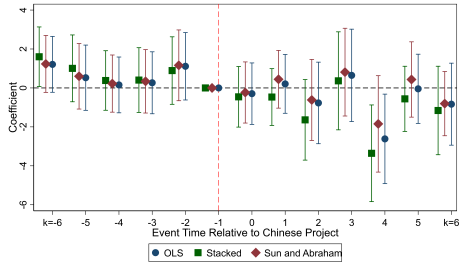
Note: Estimated coefficients and the corresponding 90% confidence intervals on $CM^{\text{China}}_{i,t-1}$ and $CM_{i,t-1}$ with respect to five types of conflicts are plotted. Dependent variables in (i) and (iii) are conflict incidence and in (ii) and (iv) are intensity respectively. Civilian violence includes remote violence. Except for the "UCDP conflict" of which the conflict events data are derived from the UCDP Georeferenced Event Dataset (GED) Global version 25.1 (Sundberg and Melander, 2013; Högbladh, 2025), all conflict events data are derived from the ACLED. All regressions include $CM^{\text{China}}_{i,t-1}$, weather anomalies and a full set of cell fixed effects and country-year fixed effects. Standard errors are clustered at the cell levels.

Figure 4.F.6: Mechanism I: Environmental Degradation, Event Study

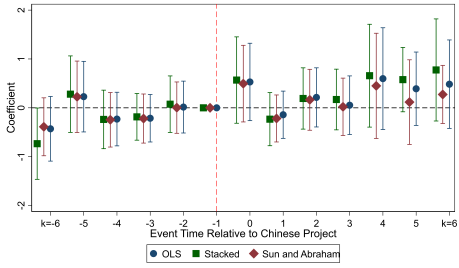
(a) $PM_{2.5}$



(b) NDVI



(c) Deforestation Index



Notes: The figure plots the event-time coefficient estimates and their corresponding 95% confidence intervals for Chinese-owned mining. Event time is defined as $k = t - \tau_i - 1$, consistent with the one-year-lagged treatment indicators used in the baseline specification, and $k = -1$ is omitted as the reference period. Event times earlier than $k = -6$ and later than $k = 6$ are binned at $k = -6$ and $k = 6$, respectively. The outcome variables are PM2.5 in panel (a), NDVI in panel (b), and the deforestation index in panel (c). Estimates are obtained using: (1) OLS; (2) the stacked difference-in-differences approach of Cengiz et al. (2019); and (3) the interaction-weighted estimator of Sun and Abraham (2021). The latter two estimators are implemented using the Stata commands `stackdid`, developed by Bleiberg (2021), and `eventstudyinteract`, developed by Sun (2021). Never-treated cells serve as the control group, and treatment cohorts are defined according to the first year in which $CM^{\text{China}}_{i,t-1} = 1$. All specifications include the full set of leads and lags for non-Chinese mining, weather anomalies, cell fixed effects, and country-by-year fixed effects. Standard errors are clustered at the cell level.

Appendix 4.G Additional Tables

Table 4.G.1: Sensitivity Test: Clustered SE and Spatial Correction SE

	(1) Probability	(2) IHS(#Protests)
$CM_{i,t-1}^{\text{China}}$	**0.177**	**0.448**
	(0.050)* **	**(0.137)*** **
Alternative clustered SE		
Clustered at ADM2 levels	(0.054)***	(0.137)***
Clustered at Country levels	(0.070)**	(0.137)***
Conley (1999) SE		
Distance Cutoff: 100km	[0.050]***	[0.137]***
Cutoff: 250km	[0.050]***	[0.137]***
Cutoff: 500km	[0.050]***	[0.136]***
Cutoff: 1000km	[0.051]***	[0.136]***

Note: This table displays the estimates of alternative specifications of standard errors. All estimates are based on equation 4.1. All regressions include $CM_{i,t-1}$, weather anomalies, cell fixed effects and country-year fixed effects. The statistics in bold are estimated coefficients and clustered standard errors at the cell levels shown in Columns (2) and (3) in Table 4.1. Clustered Standard errors at the ADM2 and country levels are displayed in parentheses. Standard errors accounting for spatial correlation with varying distance cutoffs and infinite serial correlation are estimated using the Stata command *"acreg"* created by Colella et al. (2019) and displayed in square parentheses. Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

Table 4.G.2: Sensitivity Test: Alternative Functional Form

	(1) ln(protest+1)	(2) # protests	(3) Winsorized # protests	(4) PPML (winsorized)	(5) Square root	(6) Cube root
$CM_{i,t-1}$	0.063	0.122	0.120	0.444**	0.085	0.080
	(0.055)	(0.599)	(0.556)	(0.196)	(0.087)	(0.052)
$CM^{\text{China}}_{i,t-1}$	0.365***	2.500**	2.528**	0.304*	0.551***	0.358***
	(0.114)	(1.232)	(1.192)	(0.164)	(0.181)	(0.102)
Observations	61,140	61,140	61,140	22,860	61,140	61,140
Cell FE	✓	✓	✓	✓	✓	✓
Year FE				✓		
Country-Year FE	✓	✓	✓		✓	✓
Weather Controls	✓	✓	✓	✓	✓	✓
Adjusted R-squared	0.607	0.595	0.628	.	0.638	0.579
Mean of dependent variable	0.105	0.464	0.415	1.110	0.148	0.116
Number of cells	3057	3057	3057	1143	3057	3057
Cells with Chinese mines	48	48	48	40	48	48

Note: This table presents estimates for protest intensity using alternative functional forms. The transformation method is indicated in the column headings. In Columns (3) and (4), the number of protests is winsorized at the 99.9th percentile to mitigate the influence of outliers. Except for Column (4), where year fixed effects are included instead, all regressions include $CM_{i,t-1}$, weather anomalies, a full set of cell fixed effects and country-year fixed effects. Standard errors (in parentheses) are clustered at the cell levels; Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

Table 4.G.3: Chinese Investment and Protest: Number of Fatalities

	(1) IHS(death)	(2) # of death	(3) Winsorized # death	(4) log(death+1)	(5) Square root	(6) Cubic root
$CM_{i,t-1}$	0.016	0.117	0.015	0.014	0.025	0.018
	(0.040)	(0.182)	(0.085)	(0.032)	(0.046)	(0.033)
$CM_{i,t-1}^{\text{China}}$	0.198***	0.525**	0.411**	0.157**	0.211***	0.166***
	(0.077)	(0.265)	(0.193)	(0.061)	(0.081)	(0.058)
Observations	61,140	61,140	61,140	61,140	61,140	61,140
Cell FE	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓
Weather Controls	✓	✓	✓	✓	✓	✓
Adjusted R-squared	0.278	0.221	0.268	0.279	0.278	0.270
Mean of dependent variable	0.0305	0.0607	0.0530	0.0237	0.0318	0.0271
Number of firms	3057	3057	3057	3057	3057	3057
Cells with Chinese mines	48	48	48	48	48	48

Note: This table displays the estimates for fatalities in protest. Estimates are based on equation 4.1. All regressions include $CM_{i,t-1}$, weather anomalies, cell fixed effects and country-year fixed effects. The dependent variable are transformed number of fatalities in protests. Transformation methods applied to the dependent variable are in the column heads. In Column (3), the number of death is winsorized at the 99.9th percentile to mitigate the influence of outliers. Standard errors (in parentheses) are clustered at the cell levels; Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

Table 4.G.4: Chinese Investment and Protest: World Mineral Price

	(1) Probability	(2) IHS(#Protests)	(3) Probability	(4) IHS(#Protests)
$CM_{i,t-1}$	0.074**	0.089		
	(0.034)	(0.080)		
$CM^{\text{China}}_{i,t-1}$	0.218***	0.573***		
	(0.060)	(0.166)		
$ln(World\ \ Price_{i,t-1})$	0.077***	0.173***	0.089***	0.186***
$\times CM^{ever}_{i}$	(0.022)	(0.049)	(0.022)	(0.047)
$ln(World\ \ Price_{i,t-1})$			0.033***	0.079***
$\times CM^{ever}_{i} \times CM^{\text{China}}_{i,t-1}$			(0.007)	(0.021)
Observations	60,100	60,100	60,100	60,100
Cell FE	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓
Weather Controls	✓	✓	✓	✓
Adjusted R-squared	0.410	0.601	0.410	0.601
Mean of dependent variable	0.0805	0.130	0.0805	0.130
Number of cells	3005	3005	3005	3005
Cells with Chinese mines	34	34	34	34

Note: This table displays the estimates that control for fluctuations in mineral prices. Additional interaction term $ln(World\ \ Price_{i,t-1}) \times CM^{ever}_{i}$ and $ln(World\ \ Price_{i,t-1}) \times CM^{China\ ever}_{i}$ are added to 4.1. All regressions include $CM_{i,t-1}$, weather anomalies, cell fixed effects and country-year fixed effects. Standard errors (in parentheses) are clustered at the cell levels; Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

Table 4.G.5: Chinese Investment and Protest: Chinese-owned and Foreigner-owned Mines

	(1)	(2)	(3)	(4)
	All		Share of Chinese mine > 0.5	
	Probability	IHS(#Protests)	Probability	IHS(#Protests)
$CM_{i,t-1}$	0.068**	0.077	0.080**	0.120*
	(0.035)	(0.080)	(0.035)	(0.072)
$CM_{i,t-1}^{\text{China}}$	0.150***	0.392**	0.116*	0.265**
	(0.058)	(0.157)	(0.061)	(0.126)
Observations	59,340	59,340	59,340	59,340
Cell FE	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓
Weather Controls	✓	✓	✓	✓
Adjusted R-squared	0.390	0.575	0.389	0.574
Mean of dependent variable	0.0739	0.117	0.0739	0.117
Number of firms	2967	2967	2967	2967
Cells with Chinese CM mines	39	39	34	34

*Note:*This table reports the estimation results based on a sample comprising cells that contain foreign-owned critical mineral mines and cells that have never hosted mining activities. $CM_{i,t-1}$ equals one if a cell contains an operating critical mineral mine owned by any foreign company (including Chinese firms) at time $t-1$. $CM_{i,t-1}^{\text{China}}$ equals one if a cell hosts an operating critical mineral mine owned by Chinese firms at time $t-1$. In Columns (3) and (4), $CM_{i,t-1}^{\text{China}}$ equals one if a cell hosts an operating critical mineral mine owned by Chinese firms at time $t-1$ and the share of Chinese ownership in that cell is greater than 0.5. All regressions control for weather anomalies, cell fixed effects, and country-year fixed effects. Standard errors (in parentheses) are clustered at the cell levels; Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

Table 4.G.6: Chinese Investment and Protest: Other Additional Analyses

	(1)	(2)	(3)	(4)	(5)	(6)
	Non-critical mineral		Spillover Effect		Greenfield	
	Probability	IHS(#Protests)	Probability	IHS(#Protests)	Probability	IHS(#Protests)
$CM_{i,t-1}$	-0.051	-0.145	0.085***	0.120*	0.068**	0.101
	(0.070)	(0.145)	(0.030)	(0.068)	(0.030)	(0.066)
$CM^{\text{China}}_{i,t-1}$	0.174***	0.437***	0.178***	0.449***	0.158**	0.560**
	(0.050)	(0.137)	(0.050)	(0.138)	(0.077)	(0.250)
$All\ \ Mine_{i,t-1}$	0.124*	0.228*				
	(0.067)	(0.136)				
$CM\ Neighbour\ Cell_{i,t-1}$			0.031**	0.080***		
			(0.015)	(0.030)		
$CM^{\text{China}}\ Neighbour\ Cell_{i,t-1}$			-0.002	0.003		
			(0.028)	(0.054)		
$CM^{\text{China}}_{i,t-1} \times Greenfield$					0.035	-0.206
					(0.098)	(0.284)
Observations	63,080	63,080	61,140	61,140	61,140	61,140
Cell FE	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓
Weather Controls	✓	✓	✓	✓	✓	✓
Adjusted R-squared	0.414	0.602	0.410	0.596	0.410	0.596
Mean of dependent variable	0.0862	0.141	0.0818	0.132	0.0818	0.132
Number of cells	3154	3154	3057	3057	3057	3057
Cells with Chinese CM mines	48	48	48	48	48	48

Note: This table displays the results of three additional analyses. In Columns (1) and (2), I add $All\ CM_{i,t-1}$ which takes value of one if a cell hosting any mining activities regardless of nationality of ownership and types of mineral. In Columns (3) and (4), I add $CM\ Neighbour\ Cell_{i,t-1}$ and $CM^{\text{China}}\ Neighbour\ Cell_{i,t-1}$ which are terms which taking value of one if a cell's immediate neighbor is hosting any active critical mineral mine and one active Chinese critical mineral mine, respectively. In Columns (5) and (6), $CM^{\text{China}}_{i,t-1} \times Greenfield$ is an interaction term which taking value of one if a cell is hosting an operating Chinese critical mineral mine in year $t-1$ and the first active Chinese critical mineral mine in the cell is a greenfield project.All regressions include $CM_{i,t-1}$, weather anomalies, cell fixed effects and country-year fixed effects. Standard errors (in parentheses) are clustered at the cell levels in Columns (1) and (2); Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

Table 4.G.7: Heterogeneous Effects: Additional Institutional Quality Measures

	Government Effectiveness		Political Stability		Rule of Law	
	(1)	(2)	(3)	(4)	(5)	(6)
	Probability	IHS(#Protests)	Probability	IHS(#Protests)	Probability	IHS(#Protests)
$CM^{\text{China}}_{i,t-1}$	0.262***	0.576***	0.199***	0.551***	0.229***	0.517**
	(0.067)	(0.194)	(0.063)	(0.188)	(0.070)	(0.215)
$CM^{\text{China}}_{i,t-1}$	-0.195**	-0.307	-0.070	-0.330	-0.115	-0.158
$\times$ *High Quality*	(0.092)	(0.264)	(0.103)	(0.226)	(0.099)	(0.274)
Observations	61,140	61,140	61,140	61,140	61,140	61,140
Cell FE	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓
Weather Controls	✓	✓	✓	✓	✓	✓
Adjusted R-squared	0.410	0.596	0.410	0.596	0.410	0.596
Mean of dependent variable	0.0818	0.132	0.0818	0.132	0.0818	0.132
Number of cells	3057	3057	3057	3057	3057	3057
Cells with Chinese mines	48	48	48	48	48	48

	Voice and Accountability		EITI Compliant Country	
	(7)	(8)	(9)	(10)
	Probability	IHS(#Protests)	Probability	IHS(#Protests)
$CM^{\text{China}}_{i,t-1}$	0.264***	0.621***	0.169***	0.504***
	(0.073)	(0.228)	(0.063)	(0.183)
$CM^{\text{China}}_{i,t-1}$	-0.165*	-0.341	-0.003	-0.200
$\times$ *High Quality*	(0.097)	(0.275)	(0.087)	(0.225)
Observations	61,140	61,140	61,140	61,140
Cell FE	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓
Weather Controls	✓	✓	✓	✓
Adjusted R-squared	0.410	0.596	0.411	0.596
Mean of dependent variable	0.0818	0.132	0.0818	0.132
Number of cells	3057	3057	3057	3057
Cells with Chinese mines	51	51	51	51

*Note:*This table reports heterogeneous effects based on alternative measures of institutional quality. All estimates are based on equation 4.1. All regressions include $CM_{i,t-1}$, its interaction with the corresponding institutional-quality measure, weather anomalies, cell fixed effects, and country-year fixed effects. In Columns (1)–(8), *High Quality* is a binary variable equal to one if a country's average institutional-quality score between 1996 and 2003 exceeds the sample median. The institutional-quality measures are government effectiveness, political stability, rule of law, and voice and accountability from the World Governance Indicators. In Columns (9) and (10), *High Quality* equals one if a country is classified as compliant in $t-1$. Standard errors, reported in parentheses, are clustered at the cell level. Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

Table 4.G.8: Heterogeneous Effects: Alignment with China, Ethnic Diversity, and Firm's Characteristics

a: Alignment	(1)	(2)	(3)	(4)
	UN Voting Alignment		Trade Share with China	
	Probability	IHS(#Protests)	Probability	IHS(#Protests)
$CM^{\text{China}}_{i,t-1}$	0.221***	0.549***	0.210***	0.528**
	(0.067)	(0.188)	(0.081)	(0.231)
$CM^{\text{China}}_{i,t-1}$	-0.121	-0.294	-0.060	-0.147
$\times$ *High Alignment*	(0.092)	(0.243)	(0.102)	(0.282)
Observations	61,140	61,140	61,140	61,140
Cell FE	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓
Weather Controls	✓	✓	✓	✓
Adjusted R-squared	0.410	0.596	0.410	0.597
Mean of dependent variable	0.0818	0.132	0.0818	0.132
Number of cells	3057	3057	3057	3057
Cells with Chinese mines	48	48	48	48

b: Ethnic Diversity	(5)	(6)	(7)	(8)
	Fractionalization		Polarization	
$CM^{\text{China}}_{i,t-1}$	0.196***	0.287**	0.196***	0.287**
	(0.070)	(0.115)	(0.070)	(0.115)
$CM^{\text{China}}_{i,t-1}$	0.003	0.451	0.003	0.451
$\times$ *HighDiversity*	(0.107)	(0.300)	(0.107)	(0.300)
Observations	59,720	59,720	59,720	59,720
Cell FE	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓
Weather Controls	✓	✓	✓	✓
Adjusted R-squared	0.407	0.594	0.407	0.594
Mean of dependent variable	0.0830	0.134	0.0830	0.134
Number of cells	2986	2986	2986	2986
Cells with Chinese mines	44	44	44	44

c: Project Characteristics	(9)	(10)	(11)	(12)
	Ownership		Size of Mineral Resources	
$CM^{\text{China}}_{i,t-1}$	0.193**	0.427*	0.313	0.876
	(0.077)	(0.233)	(0.251)	(0.582)
$CM^{\text{China}}_{i,t-1}$	-0.027	0.035		
$\times$ *SOE*	(0.096)	(0.272)		
$CM^{\text{China}}_{i,t-1}$			-0.006	-0.017
$\times$ *Size*			(0.014)	(0.032)
Observations	61,140	61,140	60,780	60,780
Cell FE	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓
Weather Controls	✓	✓	✓	✓
Adjusted R-squared	0.410	0.596	0.409	0.598
Mean of dependent variable	0.0818	0.132	0.0809	0.131
Number of firms	3057	3057	3039	3039
Number of sanctioned	48	48	33	33

This table reports heterogeneous effects by host-country alignment with China, ethnic diversity, and Chinese project characteristics. All estimates are based on equation 4.1. In Columns (1)–(2), *High Alignment* equals one if a host country's UN General Assembly ideal-point distance from China is below the sample median. In Columns (3)–(4), *High Alignment* equals one if the share of bilateral trade with China in total trade is above the sample median. In Columns (5)–(6) and (7)–(8), *HighDiversity* s based on above-median ethnic fractionalization and polarization in 2001, respectively. In Columns (9)–(10), *SOE* equals one if more than half of Chinese mining projects in a cell are owned by Chinese state-owned enterprises. In Columns (11)–(12), *Size* is the IHS-transformed total mineral resources of Chinese projects in a cell. Except for panel (c), all regressions include $CM_{i,t-1}$, its interaction with the corresponding heterogeneity variable, weather anomalies, cell fixed effects, and country-year fixed effects. Standard errors, reported in parentheses, are clustered at the cell level. Significance levels are *** p<0.01, ** p<0.05, and * p<0.1.

Table 4.G.9: Mechanism I: Environmental Degradation, Robustness

a: Adding nightlight and population	(1) $PM_{2.5}$	(2) NDVI	(3) Deforestation
$CM_{i,t-1}$	-0.169	-0.503	0.846***
	(0.162)	(0.455)	(0.237)
$CM_{i,t-1}^{\text{China}}$	0.787***	-1.270**	0.580**
	(0.248)	(0.626)	(0.277)
Observations	61,100	57,260	61,140
Cell FE	✓	✓	✓
Country-Year FE	✓	✓	✓
Weather Controls	✓	✓	✓
Adjusted R-squared	0.984	0.990	0.696
Mean of dependent variable	23.72	26.45	0.809
Number of cells	3,055	2,863	3,057
Cells with Chinese mines	48	47	48
b: Cells with mining activities only	(4)	(5)	(6)
$CM_{i,t-1}$	-0.274	-0.217	0.012
	(0.186)	(0.441)	(0.194)
$CM_{i,t-1}^{\text{China}}$	0.421**	-0.785	0.405*
	(0.201)	(0.548)	(0.223)
Observations	4,100	3,960	4,100
Cell FE	✓	✓	✓
Country-Year FE	✓	✓	✓
Weather Controls	✓	✓	✓
Adjusted R-squared	0.984	0.984	0.793
Mean of dependent variable	21.26	31.29	1.465
Number of cells	205	198	205
Cells with Chinese mines	48	46	48

Note: This table reports robustness checks for the estimated effects of Chinese mining operations on environmental outcomes. Panel A adds lagged nighttime light intensity and lagged population to control for local economic development and urbanization. Panel B restricts the sample to mining zones only. All regressions include $CM_{i,t-1}$, weather anomalies, cell fixed effects, and country-year fixed effects. Standard errors, reported in parentheses, are clustered at the cell level. Significance levels are *** p<0.01, ** p<0.05, and * p<0.1.

Table 4.G.10: Mechanism I: Environmental Degradation, Institutional Quality

	(1)	(2)	(3)	(4)	(5)	(6)
	Polity V			Regulatory Quality		
	$PM_{2.5}$	NDVI	Deforestation	$PM_{2.5}$	NDVI	Deforestation
$CM^{China}_{i,t-1}$	1.051***	-1.623**	0.873***	0.824***	-0.253	0.125
	(0.329)	(0.715)	(0.332)	(0.197)	(0.834)	(0.237)
$CM^{China}_{i,t-1}$	-0.089**	0.131	-0.110**	-0.045	1.542**	-0.600***
$\times$ *Quality*	(0.043)	(0.153)	(0.046)	(0.318)	(0.756)	(0.213)
Observations	61,100	57,260	61,140	61,100	57,260	61,140
Cell FE	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓
Weather Controls	✓	✓	✓	✓	✓	✓
Adjusted R-squared	0.984	0.990	0.696	0.984	0.990	0.696
Mean of dependent variable	23.72	26.45	0.809	23.72	26.45	0.809
Number of firms	3055	2863	3057	3055	2863	3057
Cells with Chinese mines	48	46	48	48	46	48

Note: This table displays the estimates of heterogeneous effects on environmental outcomes based on institutional quality. All estimates are based on equation 4.1. All regressions include $CM_{i,t-1}$, $CM_{i,t-1} \times$ *Quality*, weather anomalies, cell fixed effects and country-year fixed effects. *Quality* are continuous variables of the democracy index from the Polity V Project and regulatory quality from the World Governance Indicator. All indicators are measured as averages between 1996 and 2003. Standard errors (in parentheses) are clustered at the cell levels; Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

Table 4.G.11: Mechanism I: Environmental Degradation, Mediation Analysis

	$PM_{2.5}$		NDVI		Deforestation	
	Protest incidence	Protest intensity	Protest incidence	Protest intensity	Protest incidence	Protest intensity
Indirect effect	0.001	0.003**	0.000	0.001	0.003**	0.006**
through mediator	(0.001)	(0.001)	(0.000)	(0.001)	(0.002)	(0.003)
Direct effect	0.176***	0.445***	0.183***	0.465***	0.174***	0.442***
	(0.051)	(0.138)	(0.051)	(0.142)	(0.049)	(0.134)
Total effect	0.177***	0.448***	0.184***	0.466***	0.177***	0.448***
	(0.051)	(0.138)	(0.051)	(0.142)	(0.049)	(0.134)
Share mediated	0.006	0.006	0.002	0.001	0.018	0.013
Observations	61,100	61,100	57,260	57,260	61,140	61,140
Clusters	3,055	3,055	2,863	2,863	3,057	3,057

Note: This table reports bootstrap mediation estimates following Imai et al. (2010). The indirect effect is calculated as the product of the estimated effect of lagged Chinese mining operation on the environmental mediator and the estimated association between the mediator and the protest outcome, conditional on lagged Chinese mining operation. Protest intensity is measured as the inverse-hyperbolic-sine transformed number of protests. All regressions include $CM_{i,t-1}$, weather anomalies, a full set of cell fixed effects and country-year fixed effects and cell-specific trends. Bootstrap standard errors, reported in parentheses, are clustered at the cell level and estimated using 500 replications with random seed 123456. Significance levels are *** p<0.01, ** p<0.05, and * p<0.1.