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A natural field experiment on the effectiveness of economic sanctions before and after the Russian invasion of Ukraine

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Here, using two rounds of natural field experiments based on an audit-study approach, we investigated whether sanctioned Russian government officials retain access to the international financial system. Researchers sent 7,081 email solicitations to intermediary firms in 202 countries and jurisdictions to establish shell companies and set up corporate bank accounts. Responses to sanctioned names (middle initial modified) are compared with equivalent solicitations from non-sanctioned placebo names. Round 1 results in December 2020 reveal no statistically significant difference in financial access for the sanctioned names compared with placebo. Round 2 findings in May 2022, after the Russian invasion of Ukraine, indicate that sanctioned individuals' access to international corporate services and banking was significantly reduced (drop in non-compliant response rates: 4.0 percentage points, 95% confidence interval 1.2–6.8, $P = 0.006$, Cohen's $d = 0.207$). The results suggest that factors related to the war and not sanctions law proved decisive in constricting sanctioned individuals' access to the global financial system.

Economic sanctions have become the main tool of international coercion short of war. Conventional sanctions block trade or financial relations with targeted states[1–4]. Recently, however, 'smart sanctions' applied via private firms have targeted individuals and companies, rather than whole countries, aiming to constrict their access to the financial system[5–9] (Supplementary Information, section 1.1). The most important current such sanctions target Russian officials, but research has questioned their effectiveness[10–13] and raised concerns about unintended consequences[14–16]. Do these sanctions effectively deny targeted individuals access to the financial system?

Lack of data availability and a focus on country-level analysis has limited prior research. Previous work has failed to credibly identify the causal effects of sanctions owing to the many confounding factors involved in their application. This problem echoes causal inference challenges faced by other related areas of international social science such as civic behaviour[17–19] and environmental stewardship[20,21] that randomized control trials have addressed. Thus, this study tests the effectiveness of financial sanctions targeting individual Russian officials via a two-stage natural field audit experiment before and after the February 2022 invasion of Ukraine.

The results reveal that sanctioned names before the invasion produced no significant difference in access to global-banking and company-incorporation services compared with placebo names. After the invasion, however, the sanctioned names received significantly fewer responses than placebo, cutting the baseline response rate roughly in half. Ominously, sanctioned names still received many positive responses; factors related to the war diminished access to global finance for sanctioned individuals, but far from completely. Throughout the text, we use 'sanctioned names' to refer to names in our experimental treatment condition, which were names drawn from the Magnitsky sanctions list but with middle initials modified.

Prior research generally focuses on the effectiveness or ineffectiveness of sanctions at the country level in causing targeted governments to desist from undesired actions[3,4,22–25]. Effective sanctions are

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considered necessary to induce government behavioural change. We test the proximate effectiveness of one important instance of smart sanctions. Empirical analysis indicates that economic sanctions generally prove ineffective[10–12,26–29] or counterproductive, resulting in broad pessimism about sanctions as a tool of diplomacy, although with dissent[1,4,26].

Yet, there is little evidence concerning the effectiveness of smart sanctions in the post-Cold War era in which governments apply sanctions against individuals via private firms[5–7,10,30–32]. Uncertainties about effectiveness reflect the mixed motives of private firms conscripted by governments to enforce sanctions while they pursue profits[33]. In the twenty-first century, such measures have been increasingly applied through the financial system, in contrast with the earlier focus on trade in physical goods. Some senior policymakers have referred to this strategy as 'financial warfare'[34]. Since the end of the Cold War and even more so after the turn of the century, economic sanctions are thus increasingly mediated, targeted and financial.

Smart sanctions were first introduced to better focus economic pressure against political elites. Proponents argued that directly damaging the economic interests of these elites (the proximate aim of sanctions) would be more likely to induce the policy change desired by the sanctioning country. Furthermore, such narrowly targeted sanctions would cause less collateral damage and suffering among the wide society governed by this elite. Conversely, old-style trade sanctions applied on a state-to-state basis might only harm societies that had no influence on the policy decisions of their rulers[1,2,7–9].

This change in the design and application of a new generation of sanctions parallels and partially overlaps with the trend since the 1990s of governments conscripting banks and other firms in the financial sector to enforce rules against money laundering and the financing of terrorism[34,35]. Despite similarities between these policies, however, this Article deals only with smart sanctions, not parallel efforts to enlist private firms in fighting financial crime.

This study's tests align closely with the targeted (individual), mediated (indirect) and financial character of modern sanctions. It tests the proximate effectiveness of smart Magnitsky sanctions aimed at restricting targeted Russian individuals' access to the financial system. The experimental design provides robust causal inferences compared with observational methods (see Supplementary Information, section 2, for full details on the methodological approach). The subject pool is composed of law firms and incorporation services whose main business is forming companies and establishing corporate bank accounts for customers. We did not approach banks, because to do so in a deceptive manner is a criminal offence in many jurisdictions. According to the international Know Your Customer (KYC) rule mandated in multiple global treaties, these intermediary firms have a legal duty to identify their customers.

Intermediaries in countries with specific sanctions legislation have a further duty to exclude would-be individual customers listed under the sanctions laws. Firms that fail to identify customers by definition cannot screen out sanctioned individuals. The subject pool was compiled using internet searches employing a standard set of terms in conjunction with the name of every country and financial jurisdiction in the world. This produced a set of 5,285 firms in 202 countries and jurisdictions used for the study, many of which were contacted twice in round 1 for 7,081 observations total (see Supplementary Information, section 2.1, for more details).

Industry-insider informants emphasized to us that email is the principal means of business initiation and communication in the sector. For legal protection, researchers adapted the names by changing the middle initial of six Russian officials targeted by Magnitsky-Act sanctions and used the adapted names to serve as the treatment condition. These are compared with placebo approaches from individuals who hailed from innocuous Western countries and were not on sanctions lists.

Magnitsky sanctions legislation was first passed in 2012 in the USA and later adopted by Australia, Canada, the UK and the UK's nine overseas dependencies deploying its sanctions list (for example, British Virgin Islands, Cayman Islands and Guernsey). It is named in memory of the Russian lawyer and anticorruption investigator Sergei Magnitsky, who was killed in 2009 while imprisoned in Moscow by state officials. Governments enacted these sanctions, which have since been expanded to target individuals from many other countries beyond Russia, to prevent named individuals from accessing the international financial system. These laws command private firms not to do business with named individuals on pain of large fines and prison sentences, and they build on prior laws that firms must check the identity of their customers (the KYC rule) (see Supplementary Information, section 2.2, for more background about the Magnitsky sanctions).

Online searches indicated that the six treatment names (with the altered middle initials included) were unambiguously associated with Magnitsky sanctions; for each name, nearly every search result on the first page made the link to the Magnitsky case and/or the resulting sanctions. Thus, even the most perfunctory due diligence by those receiving the treatment solicitation—due diligence that is required by law—should have raised the conspicuous sanctions risk. In combination, this should mean that the risk is easy to detect and involves potentially severe consequences.

The placebo email solicitations were substantively the same as the treatment emails in asking for either a shell company or a bank account. In requesting company formation, the key difference in the placebo was in the deployment of innocuous names (the most common male names in the respective countries not associated with famous people) from Australia, Austria, Denmark, Finland, the Netherlands, New Zealand, Norway and Sweden—jurisdictions that are not on sanctions lists. In asking intermediaries for corporate bank accounts, the study differentiated the Magnitsky treatment names from placebo identities using—to minimize legal jeopardy from banking law—the real names of research assistants who represented legally incorporated companies based in Australia, New Zealand and the UK.

Proof of identity primarily means a copy of the picture page of the individual's passport, supplemented with utility bills and/or bank references. Unless intermediary gatekeeper firms have such proof of identity, the companies and bank accounts they provide are essentially anonymous and untraceable, and they create the principal point of vulnerability to evade sanctions (as well as to commit other crimes such as tax evasion and money laundering). For example, Russian oligarchs' yachts, mansions and private jets are seldom registered in their own names, but rather are owned in the name of shell companies they control. The same is true of their bank accounts. Unless it is possible to 'look through the corporate veil' to find the real person in control of the shell companies, targeted individuals cannot be excluded from the financial system, and sanctions are therefore ineffective.

Roughly four-fifths of the emails received no reply from firms in countries with Magnitsky laws. Such a high non-response rate is standard in audit studies of the industry[35]. To verify this, before the study we sent innocuous emails from placebo identities to all firms asking only if the companies assisted foreign customers and without any request for incorporation or banking. This check provided the baseline level of non-response. The pre-check response rate was roughly equivalent to that reported here, suggesting that the vast majority of firms simply do not respond to email requests from foreigners. This indicates that the responding firms in the study form the principal population of interest given that they are the intermediaries who typically interact with potential international customers through email.

Two independent research assistants read each response and coded each as refusal, compliant or non-compliant. Refusal responses indicated that the subject firm did not wish to do business with the requester for any reason. Compliant responses required photo identification documentation or an in-person meeting with the beneficial

owner of the company. Photo ID from or meetings with a company representative did not qualify as compliant, as this could enable the real owner to remain anonymous. Non-compliant responses failed to specify photo ID or in-person meeting requirements for the real owner. If research assistants disagreed on the coding of responses then a third, senior researcher determined the final outcome value (see Supplementary Information, section 2.3, for further details about the outcome coding).

We guarded against the possibility that firms might fail to demand identification in the initial exchange but then later require identity verification in subsequent stages of the transaction. To check for this, the research team selected a random sample of 76 firms and progressed communication and negotiations through the entire process, stopping short of completion only when final payment was to be sent. In 15 cases, we fully completed the incorporation process. And we also opened multiple bank accounts. In no instance did any intermediary substantively tighten (or loosen) its compliance requirements from the first communication in a way that would have changed the outcome coding, providing reassurance that corporate service providers (CSPs) do not routinely engage in 'bait-and-switch' behaviour in the KYC process.

The experiment measuring the effectiveness of the sanctions first compares the response, refusal, KYC compliance or non-compliance rates from intermediaries in reply to email solicitations from sanctioned names (treatment) versus those from non-sanctioned names (placebo) in countries that have Magnitsky legislation. Relative to the low-risk placebo group, were the high-risk Magnitsky names any less likely to receive a reply to their solicitations, were they more likely to be refused and were they more likely to be asked for proof of identity in compliance with international rules? The larger the difference between the low-risk placebo and high-risk treatment on these measures, the more effective the sanctions. Conversely, if high- and low-risk customers receive the same rate and types of replies on these measures because firms fail to distinguish between treatment and placebo email solicitations, this is strong evidence that sanctions are ineffective.

The before-and-after picture provided by the first round of solicitations conducted from November 2019 through December of 2020 and the second in May of 2022 following the Russian invasion of Ukraine aims to determine whether sanctions compliance shifted in the interim. Though the relevant law did not change, did factors associated with the invasion alter behaviour?

What ethically justifies the use of deception in our study? Simply asking firms whether they follow sanctions law is unlikely to provide a true picture of their behaviour. Indeed, prior surveys using informed consent and then later compared with field experiments testing a similar set of financial intermediary firms show general unreliability of survey measures in the domain[36].

The study's research subjects are firms, and the individuals receiving and responding to the emails are agents of the companies working in their official capacities. Indifferent to which agents responded, the study exclusively probed company procedures as revealed by the agents, and no personal information from individuals was sought or retained. Based on interviews with industry representatives and calculations drawn from the timing of email communications, we estimate that reading and responding to the emails on average took 3–10 min. Individuals' costs were thus relatively low. We received clearance for the study from university institutional review boards (IRBs), and we de-identified all data to ensure no firm or individual agent could come to harm from the research.

Finally, the stakes are high: financial sanctions are a key instrument of foreign policy, not just a diverting academic puzzle. When considering ethics in experiments, the *Belmont Report* principle of beneficence must be balanced along with autonomy and justice[37], and in this case, the scale weighs strongly in favour of providing causally identified evidence on a matter of great policy import (see Supplementary Information, section 1.2, for more details).

Results

Researchers made 7,081 email solicitations, inserting treatment or placebo names into base emails modelled on standard correspondence in global financial services (including emails made public by leaks of the Panama and Paradise Papers). Email is not the only form of solicitation and communication in the incorporation and business-banking industries; they also use phone calls, video meetings and in-person visits. However, both the document leaks and informants indicate that email is the primary method of correspondence. The relatively larger number of placebo subjects in round 1 compared with round 2 reflects the fact that the sanctions interventions form part of a larger study whose details are reported elsewhere[38].

In these email solicitations modelled on standard correspondence in international finance, researchers randomly assigned the emails to originate from either low-risk names or from the names of individual Russian officials targeted on sanctions lists to learn about these two groups' relative access to the international financial system. If the sanctions were effective, solicitations from high-risk sanctioned names—the treatment—would have gained access to the financial system less often and less easily than equivalent solicitations from innocuous low-risk names not on any international sanctions list—the placebo.

The results show the general ineffectiveness of sanctions in the pre-invasion round of the experiment in 2019 and 2020. By contrast, the post-invasion findings indicate that sanctioned names' access to the international financial system dropped significantly, given the notable discrimination between sanctioned and non-sanctioned names. In 2022, sanctioned names in treatment (mean (M) = 0.895, s.d. 0.307, n = 361) were significantly more likely to receive no response than placebo names (M = 0.788, s.d. 0.409, n = 354, difference −0.107, z = −3.907, P = 0.0001, 95% confidence interval (CI) −0.157 to −0.054, Cohen's d = −0.295) (Table 1 and Fig. 1).

Before the Russian invasion, private firms in countries with Magnitsky sanctions legislation were statistically just as likely to accept business, and to flout compliance requirements, when dealing with high-risk sanctioned names as they were when fielding requests from the low-risk placebo solicitations. If anything, sanctioned names in treatment were marginally more likely to receive responses that ignored transparency standards.

Indeed, the first round pre-invasion results in Table 1 and Fig. 1 show some modest reaction to the high-risk treatment in the non-compliant category. Firms not complying with global transparency standards by failing to demand photo ID replied to the high-risk sanctioned names at higher rates that are not significant statistically (M = 0.046, s.d. 0.210, n = 153) compared with low-risk placebo names (M = 0.024, s.d. 0.152, n = 1,350, difference −0.022, z = −1.626, P = 0.104, 95% CI −0.056 to 0.012, Cohen's d = −0.139). The proportion non-compliant was roughly twice the rate in treatment as placebo (4.6% versus 2.4%), although the difference fails to reach conventional significance thresholds. Furthermore, relative to the control group, the solicitations from sanctioned names were not significantly less likely to receive a response at all nor more likely to be refused service or have ID demanded.

In the second post-invasion round in May of 2022, we repeated the same experiment, once again randomly assigning the same treatment and control email solicitations to a random sample of the same pool of intermediary firms from the same jurisdictions. Crucially, the relevant Magnitsky sanctions law remained the same in nearly all of the tested jurisdictions across the pre- and post-invasion rounds. Only Australia passed a Magnitsky law in the interim, which affected less than 1% of the study's observations; omission or inclusion of them has no discernable substantive or statistically significant effects on the reported findings (see Supplementary Information, section 2.2.1, for a timeline of Magnitsky sanctions laws and experiment rounds).

The overall results post-invasion in round 2, however, were strikingly different from pre-invasion. In stark contrast to round 1, in round 2 each outcome save compliant was significantly different for the

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Table 1 | Pre-invasion and post-invasion results in Magnitsky-law countries

	Placebo	*N*	Treatment	*N*	Difference	95% CI	*P* value
Panel A: pre-invasion – Magnitsky round 1 (December 2020)							
Total *N*		1,350		153			
No reply	80.9%	1,092	81.0%	124	−0.002	−0.067 to 0.064	0.963
Refusal	13.3%	179	11.1%	17	0.021	−0.031 to 0.074	0.455
Compliant	3.5%	47	3.3%	5	0.002	−0.028 to 0.032	0.891
Non-compliant	2.4%	32	4.6%	7	−0.022	−0.056 to 0.012	0.104
Panel B: post-invasion – Magnitsky round 2 (May 2022)							
Total *N*		354		361			
No reply	78.8%	279	89.5%	323	−0.107	−0.160 to −0.054	<0.001
Refusal	13.0%	46	6.9%	25	0.061	0.017 to 0.104	0.007
Compliant	2.3%	8	1.7%	6	0.006	−0.014 to 0.026	0.564
Non-compliant	5.9%	21	1.9%	7	0.040	0.012 to 0.068	0.006

Outcomes are reported as percentages with counts. Differences are the placebo–treatment differences in proportions for each outcome, with 95% CIs and two-sided *P* values based on standard tests using the normal approximation. Panel A reports pre-invasion results (November 2019–December 2020), and panel B reports post-invasion results (May 2022) for CSPs located in countries with Magnitsky legislation. Pre-invasion: *n* = 1,503 (1,350 placebo; 153 treatment); post-invasion: n = 715 (354 placebo; 361 treatment). No adjustment for multiple comparisons; Benjamini–Hochberg false discovery rate (FDR)-adjusted *P* values are reported in Supplementary Table 18.

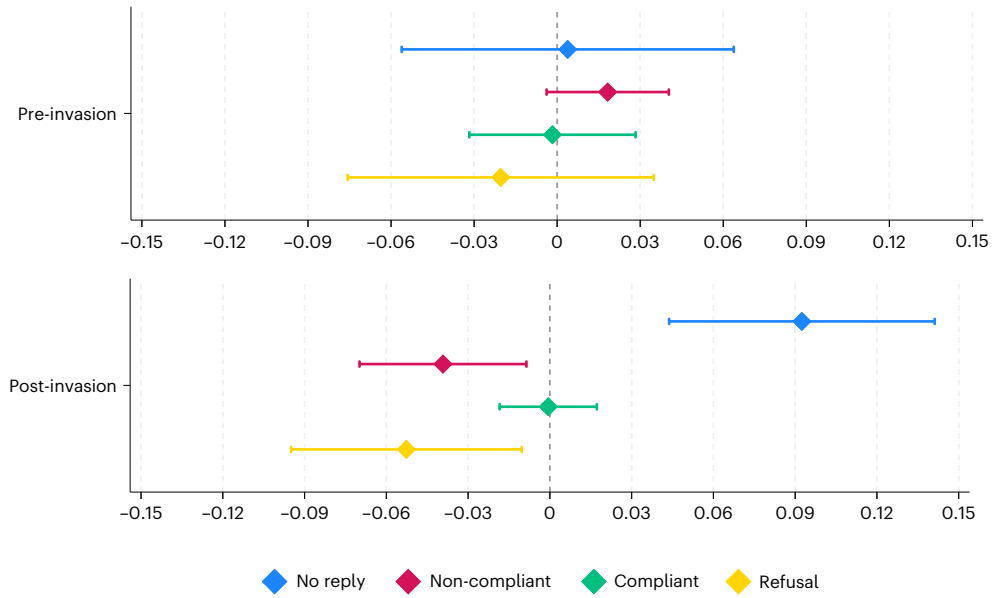


Fig. 1 | Coefficient plot with estimated multinomial probit margins for treatment compared with placebo before and after invasion in Magnitsky-law countries. Treatment effects of adapted sanctioned names (treatment) compared with innocuous names (placebo) for the experiments before and after the Russian invasion of Ukraine. The graph depicts average marginal effects estimated from multinomial probit regressions with covariates (OECD membership, offshore jurisdiction and prior personal response), with standard errors clustered by CSP. Pre-invasion panel: *n* = 1,502 (1,349 placebo, 153 treatment). Post-invasion panel: *n* = 715 (354 placebo, 361 treatment). Diamonds represent point estimates, and lines depict 95% CIs. All tests are two-sided. No adjustments were made for multiple comparisons. See Supplementary Table 16 for Table 1 results adjusted for multiple comparisons. See Supplementary Tables 7 and 8 for estimated marginal effects and *P* values.

sanction-names treatment solicitations compared with the placebo requests. The large increase in no reply, and the corresponding sharp and significant declines in refusal and non-compliant responses, indicates that firms dealt with risky approaches by simply not engaging, rather than by refusing service directly or by more strictly applying KYC procedures. In the critical category of non-compliant, the results indicate that the placebo condition, on average, received a non-compliant response not requiring photo ID of the beneficial owner once in every 16.9 emails. In the treatment condition with the sanctioned Russian names, non-compliant responses occurred at the average rate of once in every 51.6 emails. Table 1 and Fig. 1 compare treatment effects from before and after the Russian invasion of Ukraine.

As an extension of the study, we also tested the effects of treatment in an additional 4,863 emails to equivalent intermediary firms in countries that do not have Magnitsky sanctions legislation (that is, all countries other than Canada, the UK, UK dependencies and the USA for both rounds plus Australia for round 2) using exactly the same control and treatment emails. Once again, we conducted one round before the invasion 2019–2020 and a second afterwards in 2022. Intermediary firms in these non-Magnitsky jurisdictions have the same duty to identify their customers according to the KYC rule, but they have no obligation to screen out the sanctioned names that we used in our treatment emails.

Similar to firms in the Magnitsky-legislation jurisdictions, in pre-invasion round 1 there were no significant differences between

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Table 2 | Difference in differences for all countries and for the subsamples of countries with and without Magnitsky laws

Model	Difference in differences	Standard error	CI	*P* value
All countries				
No reply	0.142	0.030	0.084 to 0.200	0.000
Refusal	−0.076	0.023	−0.122 to −0.031	0.001
Compliant	−0.019	0.019	−0.055 to 0.018	0.310
Non-compliant	−0.050	0.015	−0.079 to −0.020	0.001
Magnitsky laws				
No reply	0.105	0.043	0.020 to 0.190	0.016
Refusal	−0.039	0.037	−0.111 to 0.033	0.288
Compliant	−0.004	0.019	−0.042 to 0.034	0.842
Non-compliant	−0.062	0.019	−0.100 to −0.024	0.001
No Magnitsky laws				
No reply	0.169	0.038	0.094 to 0.244	0.000
Refusal	−0.098	0.030	−0.156 to −0.039	0.001
Compliant	−0.033	0.026	−0.083 to 0.018	0.206
Non-compliant	−0.042	0.020	−0.082 to −0.003	0.037

Each row reports difference-in-differences estimates of differences in outcome probabilities between the pre-invasion (November 2019–December 2020) and post-invasion (May 2022) periods, computed using Villa's 'diff' command in Stata[47], following the standard two-period difference-in-differences framework of Card and Krueger[48]. Estimates are reported separately for all countries and by country subgroups according to Magnitsky-legislation status. Standard errors and two-sided *P* values are obtained from the 'diff' procedure; 95% CIs are reported in brackets. Total $N = 7{,}081$.

treatment and control emails for the outcomes of no reply, refusal, compliant or non-compliant (Supplementary Tables 6 and 9). The conclusion, which might seem obvious, is that sanctions do not have an effect in countries where those sanctions are not legislated, although observers anticipating extraterritorial spillover, 'chain' effects or policy diffusion might have expected otherwise[39]. For example, financial firms whose own government does not impose sanctions might nevertheless potentially refuse business with individuals sanctioned by the US government for fear of themselves falling afoul of extra-territorial secondary US sanctions. Round 1 results provided no evidence to support this kind of indirect effect.

In round 2, however, in countries without Magnitsky laws there was a substantively large and statistically significant decrease in the number of firms willing to engage with treatment emails and a corresponding decline in refusals and compliance. Non-compliance also decreased, but the drop is not significant statistically. The differences for non-response, refusal and compliance are significant both substantively and statistically (Supplementary Tables 6 and 10).

We also conducted difference-in-differences analysis and the results support these conclusions (see Table 2 and additional checks in Supplementary Table 13). The difference-in-differences analysis estimates the causal effect of the treatment on the four outcomes by comparing changes over time between the treatment and placebo groups. It shows statistically significant differences in causal effects between the periods for no reply and non-compliance for the Magnitsky-legislation countries. It also shows significant between-period differences in non-response, refusals and non-compliance for the countries without Magnitsky laws. These results produce substantively similar findings to regression analysis estimating the effects of treatment on outcome observations pooled across both periods while interacting treatment with an indicator variable for before or after the invasion, as shown in Supplementary Tables 14–17. These difference-in-differences and interaction-effect analyses were not preregistered and should therefore be interpreted as exploratory.

Discussion

Sanctions have become an essential tool of statecraft, yet we know surprisingly little about whether and when they are effective, especially for individually targeted smart sanctions imposed via the private sector. In part, this gap persists because existing studies generally focus on direct state-to-state trade sanctions, even though since the turn of the century most sanctions now operate indirectly through firms in the finance industry to target individuals or companies. More importantly still, almost all studies of sanctions rely on non-experimental observational data. To fill the gap, our study evaluates indirectly applied, individually targeted financial sanctions in a natural field experiment.

Comparing the pre- and post-invasion sensitivity to engaging with high-risk treatment sanctioned names versus low-risk placebo approaches indicates that a substantial number of gatekeeper intermediary firms began complying with Magnitsky laws only after the 2022 invasion, even though in many jurisdictions these laws had been on the books for years previously. Here, it appears that factors related to the war rather than law alone caused firms to become more compliant with sanctions in distinguishing between targeted and non-targeted names.

Multiple mechanisms could underlie the change in firm behaviour. Because the experiment did not directly test mechanisms, discussion here must remain speculative and provisional. It is possible that the war caused regulators to heighten their scrutiny of intermediary firms and thereby increased the risk that they would detect illegal actions, which in turn increased firms' application of due diligence. Alternatively, greater threats by the USA and its allies to exercise extraterritorial jurisdiction may have induced greater caution by firms and closer discrimination of customers. Finally, the Russian invasion may have intensified the salience of Russians' economic activity and amplified any stigma or social opprobrium of working with Russian clients.

Conducting a similar field experiment on the sale of shell companies, Allred et al.[40] adopt an institutionalist lens in distinguishing mechanisms that explain when and why private firms will comply with or flout KYC rules regulating customers' access to the financial system. One potential mechanism causing compliance is a fear of formal punishment by law enforcement and regulators through fines and imprisonment. They refer to this mechanism as a 'logic of consequences'[41]. A second potential mechanism relies on an informal social logic of conforming with social expectations of appropriate behaviour and avoiding shame, stigma and ostracism—a 'logic of appropriateness'[42–44].

The comparison of the results in countries with Magnitsky laws versus jurisdictions without such sanctions in place lends some support to the conclusion that it was factors associated with the war that drove the sensitivity to approaches from sanctioned names in round 2. The tests in countries without Magnitsky laws may help to distinguish among the mechanisms listed above. Because servicing the sanctioned names carried no law enforcement or regulatory penalties for firms in countries without Magnitsky laws, it might be inferred that it was more likely to be a social logic of appropriateness, activated by the political salience of the invasion, that made firms less likely to engage with sanctioned names.

After all, firms in countries without Magnitsky laws had no legal obligation to bar named individuals from corporate services or banking, yet they did so nonetheless. The high profile of the war may have made the difference. It may be that, given the wave of political and media sympathy for Ukraine and the corresponding backlash against Russia from February 2022, reputational concerns, social stigma and/or moral qualms induced a greater sensitivity among private firms towards solicitations from sanctioned names. In this interpretation, the causal mechanism centres on social stigma and reputation. The treatment effects in round 2 were larger substantively and more significant statistically for firms in the countries without Magnitsky laws than for countries with them. This arguably provides even greater support for the logic-of-appropriateness mechanism. Nevertheless, it is vital to re-emphasize that, because the field experiment did not directly test causal mechanisms, our conclusions here can only be tentative.

The reduction in responsiveness to sanctioned names was substantively and statistically significant. However, the results suggest that, even when the zeal to sanction Russian officials was arguably at its peak, people on sanctions lists still had considerable access to the worldwide financial system.

Our findings should be interpreted in light of several limitations. The design does not fully separate sanctions status from other cues associated with the treatment emails: the sanctioned identities were Russian officials whose names were readily linked online to Magnitsky sanctions, whereas the placebo identities were low-risk Anglophone controls rather than generic Russian names. At least in part, the estimated effects post-invasion therefore probably reflect aversion to Russian-linked clients. The identical tests in countries without Magnitsky sanctions speak to this to a degree, but do not fully substitute for generic Russian placebo names. In addition, our outcomes capture early-stage gatekeeping behaviour by intermediary firms rather than completed incorporation or banking transactions. Although our follow-through checks on a random subsample suggest that initial responses tracked later compliance demands closely, we do not observe the full set of downstream screening decisions. The sample is also limited to firms with an online presence reachable by email, which is well suited to studying cross-border business initiation but may not represent smaller or more informal providers. Finally, because we observe only two timepoints, with the post-invasion round fielded during the unusually salient environment of mid-2022, we cannot determine whether the change persisted nor can we cleanly distinguish whether it was driven by enforcement risk, extraterritorial pressure, reputational concerns or stigma.

What does our experiment mean in judging the effectiveness of the Magnitsky sanctions in blocking targeted individuals' access to the financial system? At one level, the relatively low rates of non-compliance when intermediaries offer access to the financial system without identifying customers, in single-digit percentage points, is good news. But because it is easy (and effectively costless) to email a large number of intermediaries, even a small minority of non-compliant firms can open important loopholes. For example, even if 95–98% of firms are following the KYC rule and barring sanctioned individuals, those looking to beat the system and remain anonymous may make enough email solicitations in a day to find multiple providers willing to flout the rules. At least some of the firms we corresponded with expressed a willingness to assist sanctioned individuals in a conspiracy to evade sanctions.

Methods

This study complies with all relevant ethical regulations. We obtained IRB clearance at the University of Texas at Austin (8 July 2015, FWA#00002030), the University of Cambridge (4 May 2017, no IRB number issued) and Brigham Young University (13 March 2018, IRB#A18-121). The IRBs reviewed the protocols and determined that human-subject rules did not apply because the subjects are corporate entities, not individuals, and no personally identifiable information was collected; the study was granted the designation 'not human subjects research'.

The choice not to seek informed consent from CSPs is consistent with the American Political Science Association's tenet in its 'Principles and Guidance for Human Subjects Research' that researchers may forego consent "when researchers are studying powerful actors and institutions"[45]. Knowledge that subjects were part of a social science study probably would have caused extensive dissembling in responses that would have eliminated the value of the data from the audit approach and thus undermined the study's beneficence[36]. A detailed discussion of ethics and legality, including considerations of beneficence, autonomy and justice, is provided in Supplementary Information, section 1.2.

Research assistants first compiled a list of the world's CSPs (intermediary firms that set up and on-sell shell companies and corporate bank accounts) and their contact details by performing comprehensive internet searches, extending and broadening the approach in earlier studies[35]. Researchers used an anonymous mode for the searches to avoid repeated search hits from browser caching. For each of the 223 countries and jurisdictions on the master list, in the search box researchers input the country or jurisdiction name and then each of the following search terms one by one in sequence: incorporation, company formation, corporate law, business law.

For each entity produced on the first ten pages of search results, researchers thoroughly examined the entity's website to identify CSPs from the content. Researchers added to the CSP list every unique incorporation service and business law firm found, along with their contact email addresses and website URLs.

The final sample includes 5,084 firms from 202 jurisdictions that form and sell shell companies and commonly assist in establishing bank accounts. The distribution of CSPs by country is enumerated in Supplementary Table 1 (for countries with more than ten approaches). This is a convenience sample of CSPs. Because in many countries, including the USA, CSPs are unregulated, there is no definitive worldwide list that constitutes the entire population. Our internet-based approach of creating this CSP sample would have missed very small businesses without a web presence catering only to local customers. However, because sanctions are directed at foreign, cross-border business (in this case from Russia) rather than regulating local customers, this potential bias in our sample is unlikely to substantively alter the conclusions drawn from the findings.

Ahead of the experiment itself, research assistants sent innocuous 'non-response' checks to all CSPs in the sample to gauge which CSPs are responsive at baseline and which are not. Example text of those emails appears in Supplementary Information, section 2.1.2. It is extremely difficult to sort out exactly what non-response means, so the non-response checks allow the gathering of some initial information on the CSPs and also the inclusion of that information as blocking criteria in randomization to better balance the sample. Roughly 60% of the non-response-check emails received no response in the overall sample, providing a baseline of expected response rates. Non-responding firms are thus included in the results.

The six Magnitsky treatment names used in the experiment are redacted for publication, but they were selected on the basis of their unambiguous association with the Magnitsky scandal from the 18 individuals named in the original 2013 US Magnitsky Act. A Google search of these names in April 2018 revealed that in each case their internet presence was overwhelmingly linked with the Magnitsky Act. Countries with Magnitsky sanction laws are listed in Supplementary Information, section 2.2.

The Magnitsky treatment approaches asked for bank accounts and incorporation services in different ways, although with a similar underlying motive and the same request for our subjects, the CSPs, to disclose the set of documents required. For the bank account requests, the study's placebo companies are actual companies legally incorporated in Australia, New Zealand and the UK—Organisation for Economic Co-operation and Development (OECD) states that pose a low risk of corruption and terrorist financing. Research assistants used their real names in requesting corporate bank accounts.

For company formation, the placebo email approaches come from requesters purporting to hail from Australia, Austria, Denmark, Finland, the Netherlands, New Zealand, Norway and Sweden and using a suitable alias. The alias names were compiled from the most common male names in each country not associated with famous individuals.

Approaches were made via email on specially created email accounts based on the name of the company formed or the name of the company the provider was requested to form. Company names were generated by random four-letter acronyms and thoroughly vetted to verify that the monikers were not already well known. All correspondence was sent using virtual private networks with IP addresses associated with the origin countries.

Researchers drafted 10 base emails for requests for bank accounts and 39 base emails for company formation based on leaked correspondence from the Panama and Paradise Papers. The emails were vetted by practitioners in the incorporation and banking industries to ensure that they matched standard correspondence. The emails were worded differently but made similar requests for bank accounts or incorporation with placeholders for experimental conditions. Example email scripts are found in Supplementary Information, section 2.2.3.

We assigned experimental conditions to the subjects using complete random assignment within blocking strata. The blocking strata are composed of target jurisdiction and prior response in the non-response check.

After extensive training and supervised practice, two different research assistants coded each response across four outcomes: no response, refusal, compliant and non-compliant. Emails were double-coded for accuracy and consistency, with discrepancies adjudicated by a third senior coder. See Supplementary Information, section 2.3, for a detailed description of coding and Supplementary Table 3 for a full enumeration of coding categories. Data collection and analysis were not performed blind to the conditions of the experiments.

Two categories of observations were excluded from analyses. First, 3,484 observations (33% of the dataset) classified as 'hybrid' approaches—in which emails requested both a bank account and incorporation—were excluded because they did not cleanly parallel the Magnitsky rounds. Second, 52 observations flagged as having missing data had all outcome variables set to missing and are excluded from analyses conditioning on non-missing outcomes. All reported results are estimated on the remaining 7,081 requests for bank accounts and company formations.

As preregistered, we report standard difference-in-proportions tests comparing the Magnitsky-treatment and placebo conditions for CSPs located in countries with and without Magnitsky-Act legislation. We present results separately for the pre-invasion and post-invasion periods. Statistical significance is assessed using two-sided tests based on the normal approximation. All statistical analyses were conducted using Stata/SE version 19.5 (StataCorp) on a MacOS platform.

Also as preregistered, estimates from multinomial probit analyses are reported that allow estimation of treatment effects for all four outcomes as well as several covariates within the same model. This addresses the sequence in the outcomes, in which compliance/non-compliance/refusal can be observed only if a CSP responds to our enquiry, by flattening the sequence into a categorical outcome to ensure a suitable counterfactual[46]. Finally, given the interest in differences not only between treatment and placebo conditions, but also before and after the Russian invasion, we report difference-in-differences models[47,48] (Table 2) as well as linear probability and logistic regression models using interaction terms to estimate treatment-effect differences between periods (Supplementary Tables 14–17). Again, the difference-in-differences and interaction models were not preregistered and should be viewed as exploratory.

The study was preregistered as part of a larger project on the Open Science Framework (OSF ID: 20190528AC; https://osf.io/20190528AC). The preregistration covers the broader study; this Article reports specifically on the Magnitsky-treatment approaches. Data collection for the pre-invasion period (round 1) for the placebo approaches took place from November 2019 through December 2020 with Magnitsky treatments all occurring in December 2020, and data collection for the post-invasion period (round 2) took place in May 2022. The gap between collection periods was due to the unanticipated Russian invasion of Ukraine in February 2022, which created a natural-experiment pre/post design. Analyses that were preregistered include the difference-in-proportions tests and multinomial probit models; the difference-in-differences and interaction-effect analyses (Table 2 and Supplementary Tables 14–17) were not preregistered and are reported as exploratory. See Supplementary Information, section 1.3, for full details on consistency with the pre-analysis plan.

Reporting summary

Further information on research design is available in the Nature Portfolio Reporting Summary linked to this article.

Data availability

Data are available via Dataverse with the study number [DVN/LFOQLJ] at https://doi.org/10.7910/DVN/LFOQLJ (ref. 49).

Code availability

All code is available via Dataverse with the study number [DVN/LFOQLJ] at https://doi.org/10.7910/DVN/LFOQLJ (ref. 49).

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Author contributions

All authors contributed equally to the work. M.G.F., D.L.N. and J.C.S. conceived the project, designed the experiment, supervised data collection, interpreted the results and wrote the article. M.G.F. and D.L.N. performed the data analysis.

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Competing interests

The authors declare no competing interests.

Additional information

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Software and code

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Data collection	Data were recorded using Google Sheets.
Data analysis	All statistical analyses were conducted using Stata/SE version 19.5 (StataCorp, College Station, TX) on a macOS platform.

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Population characteristics	N/A - Subjects are organizations (firms) and designated by IRBs as "not human subjects research."
Recruitment	Emails to corporate service provides drawn from systematic internet searches.
Ethics oversight	Institutional review boards at University of Texas at Austin, Cambridge University, and Brigham Young University

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Field-specific reporting

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Behavioural & social sciences study design

All studies must disclose on these points even when the disclosure is negative.

Study description	Global quantitative natural field experiment using and audit-study approach through email correspondence with 5,285 corporate service providers in 202 countries to test the effects of sanctions on Russian individuals' access to the global financial system.
Research sample	The sample was drawn from comprehensive, systematic internet searches to find contact information for corporate service providers. Researchers in the study contacted 5,285 CSPs, which have operations in 202 countries. No sampling frame for CSPs exists, so the internet-search strategy formed the most comprehensive known list, which necessarily omits CSPs without an internet presence.
Sampling strategy	Researchers used an anonymous mode for the internet searches to avoid repeated search hits from browser caching. For each of the 223 countries and jurisdictions on the master list, in the search box researchers input the country or jurisdiction name and then each of the following search terms one-by-one in sequence: incorporation, company formation, corporate law, business law. For example, they first searched for "United States incorporation," then "United States company formation," and so on. For each entity produced on the first 10 pages of search results, researchers thoroughly examined the entity's website. This included careful examination of the content of the site, including the "About Us" page. Researchers added to the CSP list every unique incorporation service and business law firm found, along with their contact email addresses and website URLs.
Data collection	Researchers sent 7,081 emails to 5,084 CSPs in 202 countries. A subset of placebo CSPs were contacted twice in Round 1 with a multi-month washout period between contacts. The emails requested either company formation or a business bank account. Researchers recorded whether the CSPs responded and, if they responded, two researchers independently coded whether the CSPs refused service, complied with global standards by requiring photo ID of the beneficial owner, or failed to comply with standards by exercising full customer due diligence.
Timing	Data collection for the pre-invasion period (Round 1) for the placebo approaches took place from November 2019 through December 2020 with Magnitsky treatments all occurring in December 2020, and data collection for the post-invasion period (Round 2) took place in May 2022. The gap between collection periods was due to the unanticipated Russian invasion of Ukraine in February 2022, which created a natural-experiment pre/post design.
Data exclusions	This study reports results for treatment arms that form part of a larger study, whose other results are reported elsewhere as described in the main text and Supplementary Information. Our full dataset includes 3,484 observations (33%) from institutions classified as "hybrid"— requests that asked for both company incorporation and bank accounts and thus do not fit cleanly into the bank-account or company-formation categories. Because hybrid approaches were not assigned to Magnitsky treatment or placebo conditions and were not included in the Magnitsky rounds, they are excluded from all analyses.
Non-participation	Roughly 70% of CSPs (n = 4,946) did not respond to the email inquiries and are coded as no response. They are analyzed as a key part of the study because non-response is informative and especially sensitive to treatment in Round 2.
Randomization	We assigned experimental conditions to the subjects using complete random assignment within blocking strata. The blocking strata are composed of target jurisdiction (country or territory) and prior response in the non-response check.

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Methods

n/a	Involved in the study
☒	☐ ChIP-seq
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Plants

Seed stocks	N/A
Novel plant genotypes	N/A
Authentication	N/A