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2023 **Matěj Bělín**

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References

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Abstract

These three chapters contain three applications of econometric techniques on empirical data describing the behavior of networks of actors. Chapter 1 shows that much of the commonly reported decline in international trade associated with increasing distance between countries is attributable to cultural (in-)compatibility among trading partners. The estimates show that cultural closeness is nearly as important for trade as is geographical proximity. Chapter 2 investigates the imposition of sanctions by the Western coalition against Russia over the annexation of Crimea, showing their limited impact compared to Russian counter-sanctions. These results align with the theoretical literature emphasizing the difficulty of balancing political interests in punishing the target country and the economic interests of domestic voters and firms. The final chapter studies small groups of prisoners during WWII and reveals how the presence of friends increased chances of surviving in concentration camps.

Abstrakt

Tyto tři kapitoly obsahují tři aplikace ekonometrických technik na empirická data popisující chování síťově provázaných aktérů. Kapitola 1 ukazuje, že velkou část běžně uváděného poklesu mezinárodního obchodu spojeného s rostoucí vzdáleností mezi státy lze připsat kulturní (ne)kompatibilitě mezi obchodními partnery. Odhady ukazují, že kulturní blízkost je pro obchod téměř stejně důležitá jako blízkost geografická. Kapitola 2 zkoumá uvalení sankcí západní koalicí proti Rusku kvůli anexi Krymu, přičemž ukazuje jejich omezený dopad ve srovnání s ruskými protisankcemi. Tyto výsledky jsou v souladu s teoretickou literaturou zdůrazňující obtížnost vyvažování politických zájmů o potrestání cílové země a ekonomických zájmů domácích voličů a firem. Poslední kapitola studuje malé skupiny vězňů během druhé světové války a odhaluje, jak přítomnost přátel zvýšila šance na přežití v koncentračních táborech.

Keywords

Econometrics; Networks; Panel data; Survival analysis; Measurement error

Klíčová slova

Ekonometrie; Sítě; Panelová data; Analýza přežití; Chyba měření

Length of the work:

144,667 characters with spaces, without abstract and appendices

Declaration

1. I hereby declare that I have compiled this thesis using the listed literature and resources only.
2. I hereby declare that my thesis has not been used to gain any other academic title.
3. I fully agree to my work being used for study and scientific purposes.

In Prague on 24/05/2023

Matěj Bělín

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Contents

Introduction

These three chapters contain three applications of econometric techniques on empirical data describing the behavior of networks of actors. In the first chapter, we show that cultural closeness between countries is approximately as strong a predictor of their bilateral trade as is the physical distance between them. The second chapter delves into the behavior of the Western coalition of countries as it imposed sanctions against Russia over the annexation of Crimea, showing how difficult cooperation can be in the face of countervailing economic incentives. The final chapter focuses on the behavior of small groups of prisoners during the Second World War and reveals how mutual assistance appreciably increased a prisoner's chances of surviving in times of severe hardship. All of the chapters grapple with the vexing problem of measurement error, and show that even noisy data can reveal meaningful signals.

In Chapter One (which appeared in an earlier form as CERGE-EI Working Paper 624 and was subsequently published in Economics Letters, 2020), I discuss an important class of problems in which the effect of a time-invariant regressor is sought in panel data. Examples include the effect of distance on trade flows, of institutions on economic stability, of individual's education on voting behavior, of ownership type on firms' investment choices, and of the gender of a researcher on the number of citations garnered by her published work. These types of problems are typically dealt with either by utilizing instrumental variables or by imposing restrictions on the correlations between the regressors of interest and the unobserved heterogeneity across panel units. As the latter may be difficult to defend in practical contexts from a charge of arbitrariness, and the former places high demands on data availability, the approach I take in this work presumes the availability of a proxy variable for the unobserved unit-specific heterogeneity.

In this context, I consider the problem of estimating the elasticity of international trade flows with respect to distance. It is a well-replicated finding that this parameter is centred at about negative unity, i.e., a 1 percent increase in distance between trading partners reduces trade flows by about 1 percent. However, this value implies costs of transportation that are far greater than those obtained from direct measurement. Thus, there is a possibility that cultural compatibility between traders plays a role, as countries that are closer together geographically may share more in common in terms of needs and preferences, which increases their trade flows. Because, in the absence of some dramatic shifts, the distance between countries remains constant, unobserved cultural compatibility among traders cannot be eliminated by the common approach of including fixed effects in the regression. We therefore utilize a proxy for cultural differences by including a control for an index of the Worldwide Governance Indicators. On the (testable) assumption that this control measures true cross-country differences with classical measurement error, this chapter shows that one can identify the true distance elasticity by running regressions of trade flows on distance with and without the imperfect control.

The results show that, once corrected for latent cross-country differences and measurement error, the distance elasticity falls by about half, to -0.5, which matches estimates obtained in contexts of minimal cultural differences. Therefore, I conclude that culture is roughly as potent a determinant of trade as the need to traverse physical distances.

Chapter Two (which was co-authored with Jan Hanousek and appeared first as CEPR Discussion Paper No. DP13549 and was subsequently published in the Journal of Comparative Economics, 2021) continues with the discussion of international trade, focusing on export sanctions on mining equipment imposed by a coalition of Western countries (EU, USA, Canada and their allies) on the Russian Federation following the annexation of Crimea in 2014. In response, the Russian government imposed sanctions on food imports from the Western sanctioners. This work contributes to the long line of research that explores whether trade sanctions can compel the target country to make concessions to the senders. Generally, the research in this area has found mixed results, noting that cases in which concessions have been extracted from the target are the minority outcome at best. It has been suggested that sanctions might be seen as a form of signaling, in which the sender country's government seeks to persuade its voters and allies abroad that it is willing to take a tough stance against adversaries. Alternatively, sanctions against imports from a target country into a sender country might result from protectionism of domestic producers against foreign competitors.

Comparing the trade flows of mining equipment and foodstuffs into Russia, we found very limited effect of the Western sanctions, but a pronounced impact of the Russian counter-sanctions. This outcome matches neatly with the theoretical prediction that export sanctions are difficult to sustain against the lobbying of domestic exporters and are likely to be (at least partly) a form of signaling from the government to its voters. In contrast, sanctions against imports create favorable conditions for domestic producers, which have incentives to lobby the government to perpetuate the sanctioning regime and to enforce it thoroughly. We also show that the limited impact of the Western sanctions is not an artifact of measurement error, which could have caused attenuation bias.

With the benefit of hindsight, it may not be superfluous to note that a case for the Western coalition to limit the scope of its sanctions, to minimize the economic costs incurred by imposing sanctions, has been strengthened by subsequent developments. Following the full-scale invasion of Ukraine in February 2022, a far more stringent sanctioning regime has been implemented, with the specific aim of undercutting the Russian defense industry. Had it been the aim of the Western coalition to pressure Russia into abandoning its claim on Crimea in 2014, one would have expected an imposition of this, much harsher, sanctioning regime at that time.

Chapter Three (co-authored with Štěpán Jurajda and Tomáš Jelínek, appeared as CERGE-EI Working Paper 720 and was subsequently published in the Proceedings of the National Academy of Sciences, 2023) focuses on the behavior of networks of Jewish prisoners from the Theresienstadt ghetto, many of whom were later sent to the extermination camp in Auschwitz. This study contributes to the research on survival in deadly internment camps, including POW camps, Soviet

Gulags, and Nazi concentration camps, which has found that the ability of prisoners to form small mutual-support groups is associated with increased chances of surviving extreme circumstances. Unlike much of the existing literature, which is based on survivor testimonies and thus is open to concerns about survivor bias, this work observes all the prisoners who were sent to the extermination camp, including those who did not survive and could not deliver their testimonies.

Using a near-complete database of Theresienstadt prisoners, we construct multiple indicators of social networks with which an individual prisoner was linked; notably, place of residence, family, membership in pre-deportation Jewish self-administration, being deported to the Theresienstadt ghetto on the same transport, and common work assignment, and. Once we identify a prisoner's social linkages, we look at how many of these linked prisoners were assigned to the same transport to Auschwitz. The number of linked prisoners who were transported together from Theresienstadt to Auschwitz can arguably be unrelated to individual prisoners' characteristics, because transport assignments were carried out under considerable time pressure, and there is no evidence in the historical literature suggesting that selection into transports would consider social linkages beyond family ties.

Our results show that indeed, prisoners' chances of surviving the Holocaust were bolstered by the presence of friends, confirming the (selective) survivor testimonies. We found that the effect of a fellow prisoner who had lived in the same street had a particularly large effect, while the effect of an otherwise unlinked fellow prisoner from the same transport into Theresienstadt was quite small. This finding may reflect the measurement error present in the data; that addresses were a more precise indicator of social networks than was deportation from the same town or city. We interpret the results as indicating the impact of a friend with whom a given prisoner travelled to Auschwitz, and thus we account for the measurement error at the interpretation stage. We cross-validate the results by comparing estimates for male prisoners with those for female ones, finding that women benefitted more from friends in the camp, which is consistent with the large literature finding higher degrees of pro-social behavior among women. We provide evidence on the importance of social linkages for survival under extremely severe circumstances. Specifically, our results suggest transferability of social linkages generated in normal social environments to the truly extreme conditions of deadly internment camps.

1. Time-invariant Regressors under Fixed Effects: Simple Identification via a Proxy Variable

1.1 Introduction

The estimation of parameters associated with time-invariant regressors (TIRs) in panel data is often based on strong assumptions. This is because separating the effect of TIRs from the unobserved, time-invariant heterogeneity places either high demands on the data in terms of the availability of instruments (Hausman and Taylor, 1981) or high demands on the restrictiveness of the model in terms of the assumed lack of correlation between TIRs and individual-specific effects (Krishnakumar, 2006; Plümper and Troeger, 2011; Woodcock, 2015).

At the same time, TIRs appear in a range of research questions such as the effect of institutions on economic stability (Acemoglu et al., 2003), individual's education on voting behavior (Denny and Doyle, 2009), ownership type on firms' investing choices (Asker et al., 2015), or the gender of a researcher on the number of citations (Dion et al., 2018). Hence the identification of TIR coefficients under modest restrictions on the data-generating process is a salient question.

In this paper, we show that when a proxy variable for the unobserved heterogeneity is available, it allows identification of the TIR coefficient under arbitrary correlation between the TIR and the latent confounder. Common estimation methods from the literature on mismeasured time-varying regressors in panel data (cf. Meijer et al., 2015, for review) are not applicable since, unlike time-varying regressors, TIRs provide no leads or lags that can be exploited as instruments. Instead, we show that the variance of random effects can be used to compute the measurement error's share in the variance of the proxy, which in turn can be used to correct the bias in the slope estimates. Identification is achieved under the assumption of classical measurement error, which is commonly imposed in cross-sectional settings (e.g. Lewbel, 1997; Lubotsky and Wittenberg, 2006; Bollinger and Minier, 2015). A test of classicality in this setup is provided to allow for verification of this key identifying assumption.

Further, when no suitable such proxy is observed, we derive an expression for the bias in the random-effects coefficient that allows the researcher to quantify the sensitivity of the regression coefficient to potential violations in the random-effects assumption. The intuition behind this approach is the same as in Altonji et al. (2005), but panel data allow us to impose fewer assumptions on the latent variable and hence the sensitivity measure becomes more informative.

To illustrate the utility of the proposed technique, we estimate the elasticity of international trade flows with respect to the distance between trading countries. As documented by a long line of work (Balistreri and Hillberry, 2006; Coe et al., 2007; Disdier and Head, 2008), and recently re-emphasized by Head and Mayer (2014), standard gravity models of international trade imply very large trade costs that are highly persistent over time. This has led to the suspicion that geographic distance reflects not only transportation costs, but also other factors which dampen trade flows, e.g. cultural or institutional dissimilarity, which are likely to correlate with distance and may suppress trade activities (e.g. Blum and Goldfarb, 2006; Guiso et al., 2009; Head and Mayer, 2013; Lendle et al., 2016). Proxying these latent differences with an index of institutional dissimilarity reveals a substantial bias in the naive distance elasticity estimate.

The remainder of this paper is organized as follows: first, it specifies the model under consideration, and then studies its properties under the inclusion of a proxy variable. Further, we extend the discussion to cases when no proxy is available. A Monte Carlo study shows that the estimator presented here is well behaved in small samples. Thereafter, we apply the proposed approach on trade data from OECD countries. The paper closes with brief concluding remarks.

1.2 Structural model

1.2.1 Basic framework

Consider a generic panel model with P TIRs and R time-varying regressors:

$$Y_{it} = U_i + \sum_{p=1}^{P} {}_p Z_{p;i} + \sum_{r=1}^{R} {}_r X_{r;it} + \varepsilon_{it}; \tag{1.1}$$

where Y_{it} is the outcome variable for the i-th panel unit in period t. The data consist of N panel units with T observations per unit (balanced panel is assumed for simplicity, unbalanced panels do not affect the results). U_i is an individual-specific intercept, $Z_{p;i}$ is the p-th TIR and $X_{r;it}$ is the r-th time-varying regressor. The objective is to obtain consistent estimates of the parameters ${}_p$. To allow arbitrary covariance between TIRs and U_i, Assumption 1 specifies a linear projection between these variables.

Assumption 1. Let the data-generating process in (1.1) be governed by the following linear projection:

$$U_i = {}_0 + \sum_{p=1}^{P} {}_p Z_{p;i} + {}_i; \tag{1.2}$$

such that:

$$0 = \operatorname{cov}(\varepsilon; Z_p) = \operatorname{cov}(\varepsilon; X_r) = \operatorname{cov}(\varepsilon; \ ) = E[\varepsilon]$$
$$\text{for } p = 1 \dots P \text{ and } r = 1 \dots R. \tag{1.3}$$

The variance-covariance matrix of all regressors (time-varying and time-invariant) is assumed to be finite, non-singular, and identifiable from the data. Assume further that finite unconditional variance of ${}_i$ exists, denoted by $var(\ )$, which is consistently estimable from the data.

Throughout this paper, observation indices i and t are omitted in expressions denoting unconditional moments of model variables. Condition (1.2) specifies a linear projection between the unobserved heterogeneity (U_i) and the regressors

($Z_{p;i}$). The residuals in this projection are the random effects ($_i$) that are uncorrelated with the regressors by construction. Condition (1.3) requires exogeneity of all regressors with respect to the idiosyncratic shocks ($_{it}$) and rules out correlations between random effects and idiosyncratic errors. Several possible estimators of the variance of the random effects, var(), are available which rely on different assumptions (Nerlove, 1971) and therefore it is only assumed that a consistent estimate is available (Section 1.2.3 provides further discussion on the estimation of this value).

It is worth pointing out that Assumption 1 does not require the time-varying regressors to be uncorrelated with the individual-specific effects U_i. This is because TIRs may include controls that capture these correlations, e.g. the means of time-varying regressors within each panel unit (Mundlak, 1978) or separate TIRs consisting of values of the time-varying regressors in different time periods (Chamberlain, 1984). Indeed, including the "within-means" in a random-effects specification leads to estimates of $_r$ that are identical to those obtained from a fixed-effects model (Baltagi, 2006).

1.2.2 Identification

Plugging (1.2) into (1.1) shows that under Assumption 1, running a panel regression

$$Y_{it} = b_0 + \sum_{p=1}^{P} b_p Z_{p;i} + \sum_{r=1}^{R} d_r X_{r;it} + e_{it} \qquad (1.4)$$

leads in population[1] to $b_p = \ _p + \ _p$. In cases when an instrumental variable can be obtained (Hausman and Taylor, 1981), the bias terms $_p$ can be eliminated easily. However, the approach taken here is to utilize information contained in an imperfect proxy for U_i, thus avoiding the need to defend the proper exclusion of an instrument.

Following the literature on imperfect control variables (Lewbel, 1997; Lubotsky

[1] All estimated quantities in this paper will be treated as population estimates, where $N \to \infty$ and T is fixed. This is to simplify the notation by suppressing the probability limits.

and Wittenberg, 2006; Bollinger and Minier, 2015), let us assume that a proxy variable for U_i is available, which is subject to classical measurement error.

Assumption 2. Suppose that a proxy variable for U_i is available, and obeys the following relationship:

$$U_i = \ _0 + \ _1 U_i + \ _i ; \qquad (1.5)$$

such that:

$$0 = \mathrm{cov}(\ ; Z_p) = \mathrm{cov}(\ ;\ ) = E[\ ] \text{ for } p = 1 \ldots P: \qquad (1.6)$$

Assume further that a nite unconditional variance of $_i$ exists, denoted by $\mathrm{var}(\ )$.

Equation (1.5) speci es a linear projection between U_i and U_i thereby ensures that the measurement error, $_i$, is uncorrelated with the true unobserved heterogeneity, U_i. Condition (1.6) guarantees that the measurement error is classical in the sense that it is uncorrelated with regressors ($Z_{p;i}$) and random e ects ($_i$). Here we follow Lubotsky and Wittenberg (2006) and Bollinger and Minier (2015) in using a more general de nition of classical measurement error by specifying a linear projection between U_i and U_i. The more commonly used setup would constrain $_1$ to unity (e.g. Meijer et al., 2017; Pei et al., 2018). Nevertheless, even this more general version of classicality places restrictions on the data-generating process as condition (1.6) holds by assumption, not by construction. As it turns out, however, this restriction is testable in the framework studied here, and therefore a researcher can check whether the data are consistent with this assumption.

With Assumptions 1 and 2 in place, it is tempting to solve the identi cation problem by including the proxy variable U_i on the right-hand side of the regression equation and to t an augmented model:

$$Y_{it} = b_0 + \sum_{p=1}^{P} b_p Z_{p;i} + b_{P+1} U_i + \sum_{r=1}^{R} d_r X_{r;it} + e_{it} : \qquad (1.7)$$

However, the regression with a mismeasured control introduces bias as well. Asymptotically, the coe cients of interest become a weighted average of $(\ _p + \ _p)$ and $\ _p$ itself:

$$b_p = \frac{(\ _p + \ _p)\mathrm{var}(\ ) + \ _p\ {}^2_1\mathrm{var}(\ )}{\mathrm{var}(\ ) + \ {}^2_1\mathrm{var}(\ )}: \tag{1.8}$$

The derivation is provided in Appendix A.1. Intuitively, the poorer proxy for U_i is available, the smaller proportion of omitted variable bias is eliminated, which is one of the reasons Pei et al. (2018) argue against using U_i as an additional control. Nevertheless, in this panel data setting, b_p leads to the identi cation of $\ _p$. Observe rst that running a regression:

$$U_i = c_0 + \sum_{p=1}^{P} c_p Z_{p;i} + v_i \tag{1.9}$$

yields the following estimates:

$$c_p = \ _1\ _p \text{ for } p = 1 ::: P \tag{1.10}$$
$$s_v^2 = \ {}^2_1\mathrm{var}(\ ) + \mathrm{var}(\ ); \tag{1.11}$$

where s_v^2 is the estimated variance of the error terms v_i. Since $\mathrm{var}(\ )$ is estimable from (1.4) as the variance of random e ects, there are $2P + 2$ unknowns remaining: $\ _p;\ \ _p;\ \ _1$, and $\mathrm{var}(\ )$. These $2P + 2$ unknowns can be recovered from the system of P equations $b_p = \ _p + \ _p$, P equations (1.8), P equations (1.10), and one additional condition in (1.11). Even though it may seem that the system is over-identi ed, this is not so. This is because the system of $3P + 1$ equations is based on only $2P + 2$ moments from the data that contain information on the slope parameters.[2] Therefore, the system may be solved analytically using information from one TIR at a time. The solution is presented in the following proposition:

[2] These are: P covariances between TIRs and Y, P covariances between TIRs and U, one covariance between Y and U, and the variance of U. Variance of Y conveys no additional information on slope parameters, but it is used for computing $\mathrm{var}(\ )$.

Proposition 1. Under Assumptions 1 and 2, the structural parameters $_1$; var( ); $_p$, and $_p$ for $p = 1 ::: P$ can be recovered from the reduced-form parameters b_p; b_p; c_p; and s_v^2 in regressions (1.4), (1.7), and (1.9) as:

$$_1 = \frac{(b_p \quad b_p)s_v^2}{c_p s^2}; \tag{1.12}$$

$$\text{var}(\ ) = s_v^2 \quad {}_1^2 s^2 = s_v^2 \quad \frac{(b_p \quad b_p)^2 s_v^4}{c_p^2 s^2}; \tag{1.13}$$

$$_p = \frac{c_p}{_1} = \frac{c_p^2 s^2}{(b_p \quad b_p)s_v^2}; \tag{1.14}$$

$$_p = b_p \quad _p = b_p \quad \frac{c_p^2 s^2}{(b_p \quad b_p)s_v^2}; \tag{1.15}$$

where s^2 is an estimate of the variance of random e ects from regression (1.4).

Proof: see Appendix A.1. Using Proposition 1 leads to P estimates of $_1$ (and consequently to P estimates of var( )) that will be asymptotically equal to each other since the system is exactly identi ed. The reason identi cation is feasible here and not in the cross-sectional cases without instruments (Lubotsky and Wittenberg, 2006; Bollinger and Minier, 2015; Oster, 2016) is that panel data furnish an additional piece of information about the latent variable, var (), which is unavailable in cross-sections. If var () was not known, it would be impossible to separate the variance of the proxy into the component attributable to the measurement error, and to the variance arising from the true latent confounder. Once the extent of the contamination by measurement error is determined, it is straightforward to correct the attenuation bias.

1.2.3 Postestimation checks

Having established identi cation of the structural parameters from regressions (1.4), (1.7), and (1.9), it may be useful to consider ways to verify whether the data are consistent with Assumptions 1 and 2. Since the variance of random e ects, var( ), plays a crucial role in identi cation, it bears noting that the standard estimator for this value assumes no autocorrelations in η_t (Swamy and Arora, 1972).

To address this limitation, it is possible to check if allowing for these autocorrelations (e.g. MaCurdy, 1982) changes the estimate substantially. Alternatively, s^2 obtained using the Swamy and Arora (1972) estimator may often be treated as an upper bound on var( ). This is due to the fact that positive autocorrelations in $''_{it}$ are likely bias the Swamy and Arora (1972) estimator of var( ) upwards. Thus, the numerator in (1.14) will also be biased upwards, producing an upper bound on $_p$.

In addition to the consistent estimation of var( ), the assumption of classicality also merits closer inspection. Two simple \sanity checks" can be performed, which can detect violations of this key identifying assumption. The first one is to verify that the estimate of var( ) is non-negative. This estimate is computed as the difference of two positive quantities, and therefore it is not guaranteed that var( )   0 if Assumptions 1 and 2 are violated. The second check is to see whether the estimate of $_1$ has the expected sign. Usually, a proxy would be expected to correlate positively with the true latent confounder, implying $_1 > 0$.

Apart from these sanity checks, which can be performed under full generality, suitable restrictions on the data-generating process can provide over-identifying restrictions for an additional test of classicality. Consider a proxy U_i^{UR}, which is subject to an unrestricted measurement error M_i^{UR}. Without loss of generality, let us express U_i^{UR} as:

$$U_i^{UR} = {}_0 + {}_1 U_i + M_i^{UR} \tag{1.16}$$

$$M_i^{UR} = {}_0 + \sum_{p=1}^{P} {}_p Z_{p;i} + {}_{P+1} {}_i + {}_i^{UR}: \tag{1.17}$$

Equation (1.17) projects the unrestricted measurement error on all constituent parts of U_i. In doing so, no assumption has been imposed on the process that generated this error. Indeed, U_i^{UR} can be a completely different variable, which conveys no information about U_i. Using the definition of U_i in (1.2), U_i^{UR} can be rewritten as:

$$U_i^{UR} = {}_0^{UR} + \sum_{p=1}^{P} {}_p^{UR} {}_p Z_{p;i} + {}_{P+1}^{UR} {}_i + {}_i^{UR}; \tag{1.18}$$

where ${}^{UR}_{0}$ ${}_{0} +$ ${}_{0}$, ${}^{UR}_{p}$ ${}_{1}({}_{p} + {}_{p}) = {}_{p}$ for $p = 1 \ldots P$, and ${}^{UR}_{P+1}$ ${}_{1} + {}_{P+1}$. Clearly, classical measurement error is a special case of (1.18), where

$$ {}^{UR}_{1} = {}^{UR}_{2} = \quad = {}^{UR}_{P+1} : \tag{1.19} $$

Hence, classicality could be tested if the scaling factor ${}_{1}$ was estimable separately for multiple TIRs. This is only possible if constraints are placed on the remaining parameters as the model is exactly identified. Fortunately, in many specifications, such restrictions suggest themselves. In models proposed by Mundlak (1978) and Chamberlain (1984), the set of TIRs includes controls that capture the correlation structure between U_i and time-varying regressors, but these controls are not usually expected to influence the outcome variable on their own. In the case of Mundlak's specification, these additional TIRs are means of the time-varying regressors within each panel unit ("within-means"). This specification can be written as:

$$ Y_{it} = U_i + \sum_{p=1}^{P_1} {}_{p} Z_{p;i} + \sum_{r=1}^{R} {}_{r} X_{r;it} + "_{it} \tag{1.20} $$

$$ U_i = {}_{0} + \sum_{p=1}^{P_1} {}_{p} Z_{p;i} + \sum_{r=1}^{R} {}^{w}_{r} \overline{X}_{r;i} + {}_{i} ; \tag{1.21} $$

where P_1 regular TIRs are written separately from the R within-means such that $P_1 + R = P$. $\overline{X}_{r;i}$ denotes the mean of the r-th time-varying regressor within i-th panel unit. To prevent confusion, parameters associated with within-means are marked by the superscript w. Observe that the within-means do not appear in the outcome equation (1.20), and so there are R over-identifying restrictions in this specification, namely ${}^{w}_{r} = 0$ for $r = 1 \ldots R$.[3] These restrictions make it possible

[3] An analogous argument can be made if the model is specified according to Chamberlain (1984). Mundlak's specification is presented here since it does not require balanced panels (unlike Chamberlain's).

to compute $R + 1$ separate estimates of $_1$. Running regressions

$$Y_{it} = b_0 + \sum_{p=1}^{P-1} b_p Z_{p;i} + \sum_{r=1}^{R} d_r X_{r;it} + b_r^w \overline{X}_{r;i} + e_{it} \qquad (1.22)$$

$$Y_{it} = b_0 + \sum_{p=1}^{P-1} b_p Z_{p;i} + b_{P+1} U_i + \sum_{r=1}^{R} d_r X_{r;it} + b_r^w \overline{X}_{r;i} + e_{it} \qquad (1.23)$$

$$U_i = c_0 + \sum_{p=1}^{P-1} c_p Z_{p;i} + \sum_{r=1}^{R} c_r^w \overline{X}_{r;i} + v_i \qquad (1.24)$$

produces one baseline estimate of $_1$ using (1.12) and R additional ones computed as $b_{1;r} = c_r^w / b_r^w$. Rejection of the null hypothesis that all of these $R + 1$ estimates of $_1$ are equal to each other will defeat either the assumption of classicality or the over-identifying restrictions $^w_r = 0$ for $r = 1 \ldots R$. Thus, unless economic theory suggests that the within-means influence the outcome in their own right, then the test of equality of scaling factors can serve as a test for the classicality of measurement error in U_i.

In sum, the assumption of random effects (i.e. regressors are uncorrelated with the unobserved heterogeneity) can be replaced by the assumption of a classically mismeasured control for the unobserved heterogeneity (i.e. the regressors are uncorrelated with the measurement error). The advantage of invoking the assumption of classical measurement error is its testability. The customary way of testing the random-effects assumption would be the Hausman test, which compares coefficients of time-varying regressors obtained from a random-effects specification with those from a fixed-effects specification (Hausman, 1978). As TIRs are wiped out in the fixed-effects model, no test relying on a fixed-effects specification can detect correlations between TIRs and the unobserved heterogeneity. On the other hand, as shown above, correlations between TIRs and the measurement error are indeed detectable. Hence, estimating a Mundlak-type model with a proxy for the unobserved heterogeneity offers three benefits compared to a standard random-effects model: first, coefficients on the time-varying regressors coincide exactly with a fixed-effects model; second, TIR coefficients are identified; and finally, the credibility of the TIR coefficients can be assessed by testing whether the data are consistent with the classical measurement error setup.

1.3 Alternative approach without a proxy variable

If Assumption 2 is not justifiable, either because of the rejection of the over-identifying restrictions or the unavailability of a proxy variable, useful information can still be extracted from the data. After estimating model (1.4), one may follow the intuition of Altonji et al. (2005) to quantify how strong the endogeneity has to be to produce bias ${}_p$ large enough to change conclusions drawn from the random-effects coefficient b_p. In other words, when compelled to assume that the unobserved heterogeneity is uncorrelated with the regressors, one may measure how sensitive the estimates are to potential violations of this assumption.

Therefore, let us express the correlation between the TIR of interest (Z_p) and the unobserved heterogeneity (U) after conditioning on all remaining regressors:

$$\operatorname{corr}(\tilde{U};\tilde{Z}_p) = {}_p \sqrt{\frac{\operatorname{var}(\tilde{Z}_p)}{{}^2_p \operatorname{var}(\tilde{Z}_p) + \operatorname{var}(\ )}}; \qquad (1.25)$$

where $\tilde{U}$ ($\tilde{Z}_p$) denotes residuals from a regression of U (Z_p) on all remaining regressors (see Appendix A.1 for derivation). If the estimated coefficient is wholly attributable to the endogeneity of Z_p, then ${}_p = b_p$ and ${}_p = 0$. Hence, substituting ${}_p$ in Equation (1.25) with b_p can be used to calculate how far $\operatorname{corr}(\tilde{U};\tilde{Z})$ has to deviate from the assumed value of zero to account for the estimated coefficient in its entirety. Should even small values of $\operatorname{corr}(\tilde{U};\tilde{Z})$ lead to high biases, then it would be ill-advized to rely on the regression results. Conversely, if even high levels of $\operatorname{corr}(\tilde{U};\tilde{Z})$ produce only moderate biases in b_p, then the random-effects coefficient can be deemed "robust" to the potential presence of fixed effects. Thus, as an alternative to using (1.25) under the assumption of some fixed value of ${}_p$ (e.g. ${}_p = b_p$), $\operatorname{corr}(\tilde{U};\tilde{Z})$ may be fixed at some value to compute the implied bias:

$${}_p = \operatorname{corr}(\tilde{U};\tilde{Z}_p) \sqrt{\frac{\operatorname{var}(\ )}{\left(1 - \operatorname{corr}^2(\tilde{U};\tilde{Z}_p)\right) \operatorname{var}(\tilde{Z}_p)}}: \qquad (1.26)$$

Since (1.25) and (1.26) are equivalent, the choice of one over the other would depend on their respective interpretive transparency in a given application. Panel data again provide a signi cant advantage here, since the calculation in (1.25) and (1.26) needs no further assumptions, while in the cross-sectional settings considered by Altonji et al. (2005), an additional assumption is needed on the extent to which residual variance in regression (1.4) is attributable to the latent variable (cf. Oster, 2016, sec. 3.3.1 for discussion).

1.4 Monte Carlo evidence

To assess the performance of the proposed technique in small samples, we recover using Proposition 1 from simulated datasets. The data-generating process was set up as $Y_{it} = 2 + U_i + (1/2)Z_i \quad X_{it} + \varepsilon_{it}$, $U_i = \quad Z_i + 2W_i + \quad_i$, $Z_i = W_i + \quad_i$ and $X_{it} = W_i + 0.3Z_i + \omega_{it}$. Parametrization $= 1/2$ and $= \quad 1$ represents a situation in which the omitted variable bias is prominent. This is a natural choice, since in situations when the bias is thought to be minor, a researcher would rely on a random-e ects model, rather than contemplate using Proposition 1. The second TIR, W_i, is the within-mean of the time-varying regressor, which correlates with the unobserved heterogeneity as well as with the TIR of interest. Since in practical contexts, this value is unknown, W_i will be replaced by $\overline{X}_i = \hat{W}_i = T^{\ 1}\sum_{t=1}^{T} X_{it}$ in regressions on the simulated data.

The proxy variable $U_i = 1 + (1/\sqrt{5})U_i + \quad_i$ was generated with varying values of var( ) while keeping var(U) = 5 xed. Hence var( ) is equal to the noise/signal ratio in U_i, i.e. var( )/var($_1$U) = var( ). We vary this ratio from 0 to 4 in 0.5 increments generating 1,000 simulated samples at each step. In each sample, regressions $Y_{it} = b_0 + b_1 Z_i + b_2\overline{X}_i + d_1 X_{it} + e_{it}$ (i.e. 1.4), $Y_{it} = b_0 + b_1 Z_i + b_2\overline{X}_i + b_3 U_i + d_1 X_{it} + e_{it}$ (i.e. 1.7), and $U_i = c_0 + c_1 Z_i + c_2\overline{X}_i + v_i$ (i.e. 1.9) were run, recording the values of b_1, b_1, b_1 ^{b}, $^b{}_1$, and the corresponding relative biases. Relative bias in the estimate of t is de ned in percentage terms as $100(\hat{t} \quad t)/t$.

Figure 1.1: Quantiles $p5$, $p50$, and $p95$ of the relative bias in the estimated TIR coefficient as functions of measurement error in the proxy variable.

(a) $T = 2$, $N = 200$

(b) $T = 10$, $N = 5{,}000$

Figure 1.2: Quantiles $p5$, $p50$, and $p95$ of the relative bias in the estimated scaling factor $_1$ as functions of measurement error in the proxy variable. $^{b}{}_{1;A}$ was constructed from coefficients associated with regressor A only.

(a) $T = 2$, $N = 200$

(b) $T = 10$, $N = 5{;}000$

Thus we obtain distributions of the bias present in b_1, b_1, b_1 ^{b}, and $^b{}_1$ for each value of the noise/signal ratio. Figure 1.1 plots the median along with p5 and p95 of these three distributions. Encouragingly, ^{b} = b_1 ^{b} performs quite well even in very small samples (T = 2, N = 200) as the median bias remains consistently at zero, unaected by the variance of the measurement error. However, the noise/signal ratio does impact the variance of ^{b}, which grows in response to heavier measurement error. This is an intuitive result since estimating the bias term, , becomes increasingly more dicult as U_i becomes less informative about U_i. In contrast, b_1 is very sensitive to the measurement error because the value of b_1 is determined by var( ) according to (1.8). With increasing noise/signal ratio, b_1 converges to b_1 because heavier measurement error diminishes the proxy variable's ability to eliminate the omitted variable bias. As expected, the measurement error of the estimated within-means inuences the bias in b_1 and b_1 (if within-means were known, relative bias in b_1 would be -200%) but it is irrelevant for the consistency of ^{b}.

Estimates of the scaling factor, $_1$, also perform well as shown in Figure 1.2. In this model, the within-mean of X_{it} only aects U_i without having a direct impact on Y_{it}. This restriction allows us to obtain two separate estimates of $_1$: one using parameters associated with Z_i, and another one using coecients multiplying $\overline{X}_i$, i.e. $^b{}_{1;\overline{X}}$ = $c_2 = b_2$. Even though the scaling factor may not be economically interpretable, it can serve for testing of the classicality of the measurement error and hence it is reassuring that seems well behaved.

The simulation results, therefore, show that the estimator in Proposition 1 will eliminate the bias at the cost of eciency. In cases when U_i is is a very poor proxy for U_i, the denominator in (1.15) approaches zero, which leads to inated variances in the estimates of . In large samples, however, even poor proxies can lead to an estimate of that is precise enough to be informative (Figure 1.1b).

1.5 Empirical application

We estimate a simple gravity model of international trade between OECD countries for the period 2005–2014. The parameter of interest, b_1, is the elasticity of international trade flows with respect to the distance between the trading partners. This setup is particularly suitable as an illustration for the procedure proposed here for three reasons: (i) the regressor of interest is a TIR, which precludes a fixed-effects model, (ii) it is suspected that countries that are close to each other geographically are also more similar in cultural dimensions, which facilitates trade thus causing omitted variable bias (see e.g. Guiso et al., 2009; Möhlmann et al., 2010), and (iii) the suspected latent confounder is difficult to control for, since cultural measures are likely to be imperfect. Whether the cultural compatibility affects trade via shared tastes, lower informational frictions, or greater trust, all of these influences can be measured by rough proxy variables at best.

The proxy for these latent cross-country differences used here is an index of institutional dissimilarity within each exporter-importer dyad. After obtaining b_1 from the baseline gravity equation, and b_1 from a model augmented by the index of institutional dissimilarity, Proposition 1 is used to reconstruct the true distance elasticity by accounting for the presence of measurement error in the proxy variable.

1.5.1 Data and empirical specification

Trade flows of goods were extracted from the STAN database (OECD, 2018). A dataset by Gurevich and Herman (2018) was used as a source of contextual variables. Following Möhlmann et al. (2010), the proxy for the unobserved cross-country differences was constructed as a Kogut and Singh (1988) index of the Worldwide Governance Indicators (World Bank, 2017). Summary statistics and other details are available in Appendix A.2.

The model was specified as:

$$
\begin{aligned}
\ln(\text{Trade flows})_{ijt} = {} & b_0 + b_1 \ln(\text{Distance})_{ij} + b_2 \overline{\ln(\text{GDP})}_i + b_3 \overline{\ln(\text{GDP})}_j + \\
& + b_4 \overline{\ln(\text{Population})}_i + b_5 \overline{\ln(\text{Population})}_j + d_1 \ln(\text{GDP})_{it} + \\
& + d_2 \ln(\text{GDP})_{jt} + \\
& + d_3 \ln(\text{Population})_{it} + d_4 \ln(\text{Population})_{jt} + \\
& + \sum_{s=2006}^{2014} a_s 1[t = s] + e_{ijt} : \qquad (1.27)
\end{aligned}
$$

Subscripts i, j, and t index exporter, importer, and time respectively. The overline specifies the mean within a country dyad. Following Mundlak (1978), we include the within-means as controls for the correlations between the time-varying regressors and the unobserved heterogeneity. Due to collinearity, within-means of year dummies were omitted. In addition, a second regression was fitted where (1.27) was augmented by the inclusion of the proxy variable described above. The third regression projected the proxy on ln(Distance) and the within-means. These three models correspond to regressions (1.4), (1.7), and (1.9) that provide the necessary reduced-form coefficients, which will be used to reconstruct the structural parameters using Proposition 1.

To measure sensitivity of b_1 to endogeneity as suggested in Section 1.3, we obtained the conditional variance of ln(Distance) by regressing it on within-means and isolating the residual variance. Variance of the random effects, var(χ), was calculated by the Swamy and Arora (1972) method since allowing autocorrelated idiosyncratic disturbances has small effect on the resulting estimate. Standard errors were obtained by bootstrapping with 10,000 replications clustered by the country dyads and significance was computed from symmetric bootstrap p-values (MacKinnon, 2009).[4]

In this dataset, taking logarithmic transformation of the dependent variable has minor impact on the estimated coefficients, see Beln (2018) for results from Poisson

[4]Alternatively, the system can be estimated jointly by MLE or GMM and the variance-covariance matrix of the structural parameters can be calculated by the Delta Method. However, since the expressions for computing the structural parameters involve ratios of the reduced-form coefficients, Delta Method approximations may yield unreliable standard errors.

and Gamma models with the dependent variable in levels (cf. Santos Silva and Tenreyro, 2006; Blackburn, 2007; Head and Mayer, 2014, for further discussion), which produces parameter estimates similar to those from OLS with dependent variable in logarithms (Waugh, 2010, reports similar experience).

1.5.2 Results

Table 1.1 reports the key parameters pertaining to the distance elasticity of international trade ows.

Table 1.1: Estimated distance elasticity of international trade ows.

Param.	Description	Est.	SE
b_1	Distance el. from baseline model (1.4)	-1.055***	0.034
b_1	Distance el. controlling for the proxy (1.7)	-1.040***	0.034
$b_1 \quad b_1$	Naïve est. of bias in b_1	-0.014**	0.006
	Est. of bias in b_1, corrected for meas. error	-0.501*	0.308
b_1	Distance elasticity sans bias	-0.554**	0.308
$^{0}_{U;Z}$	corr (U; ln(Distance)|X) if b_1 =	-0.826***	0.012

Notes: Est. = Point Estimate, SE = Standard error. Standard errors are clustered by country dyads. Signi cance codes: *** 1%, ** 5%, and * 10%.

The model estimates trade elasticity at roughly negative unity, which is well in line with the published literature (see Disdier and Head, 2008, for a survey). While adding the proxy variable leads to an almost imperceptible change in the estimated elasticity ($b_1 \quad b_1$ is less than 2% of the baseline estimate b_1), once Proposition 1 is used to account for the measurement error, the estimated becomes much more pronounced (albeit rather imprecisely estimated) amounting to about 47% of b_1. Hence, the correction for the presence of measurement error leads to a revision of the distance elasticity estimate from about -1 to roughly -1/2.

The large negative bias in the estimated distance elasticity is consistent with the empirical literature (Head and Mayer, 2013, provide a survey). In particular, Feyrer (2009) calculates distance elasticity in the range from -0.5 to about -0.2

exploiting the temporal variation in shipping distance due to the closure of the Suez Canal in 1967. In this model, xed e ects for country dyads are included and therefore the e ects of cultural compatibility are plausibly eliminated from the estimates. Similarly, Lendle et al. (2016) nd distance elasticity in the -0.5 to -0.3 range utilizing data on eBay transactions. Due to eBay's highly standardized marketplace, cultural compatibility has much less room to assert itself, thus producing an estimate of distance elasticity with less bias than in commonly used trade data. Using data on trade in electronic goods, Blum and Goldfarb (2006) attempt to estimate the omitted variable bias directly as these transactions incur no transportation costs at all. Thus, any distance elasticity in electronic goods is due to the cultural compatibility alone. Their results suggest that the cultural component for these goods may cause bias of 100% of the commonly obtained values of distance elasticity, however, due to wide con dence intervals this value cannot be pinned down precisely. Moreover, it is unlikely that the entirety of the estimated parameter is attributable to omitted variable bias in this setting, as $b_1 =$ implies a correlation between log-distance and the latent confounder ($^0_{U;Z}$ in Table 1.1) of more than {0.8. Thus, the unobserved variable would have to be a \clone" of distance, which is scarcely plausible. A probable range for distance elasticity would therefore be from -0.5 to -0.2, which is roughly within one standard deviation from the point estimate obtained here.

1.5.3 Tests of classicality

Finally, we turn to the test of classicality of the measurement error in order to check whether the data are consistent with this critical identifying assumption. Examining the scaling factor, $_1$, the preliminary \sanity check" would be to verify that the estimate is negative and signi cant. This is because in our model, U_i represents unobserved compatibility within a country dyad (as it appears with coe cient +1 in the model), whereas U_i is measured as an institutional distance. Hence, the proxy in this instance is negatively correlated with the suspected latent confounder. As Table 1.2 reports, the baseline estimate of $_1$, computed using (1.12) on parameters associated with log-distance, is indeed negative and signi cant.[5]

Table 1.2: Estimates of the scaling factor and tests for classicality

Param.	Regressor	Est.	SE	Ind. p-val	Joint p-val.
$_{1;1}$	$\ln(\text{Distance})_{ij}$	-0.521	0.187	|	|
$_{1;2}$	$\overline{\ln(\text{GDP})}_i$	-0.607	0.233	0.774	|
$_{1;3}$	$\overline{\ln(\text{Population})}_i$	-0.521	0.226	0.998	0.912
$_{1;4}$	$\overline{\ln(\text{GDP})}_j$	7.401	6621.1	0.165	0.165
$_{1;5}$	$\overline{\ln(\text{Population})}_j$	2.33	4.408	0.04	0.163

Notes: Est. = Point Estimate, SE = Standard error. Di erent estimates of $_1$ were obtained using coe cients associated with di erent regressors in (1.27) under the restriction that only $\ln(\text{Distance})_{ij}$ a ects the trade ows on its own. Standard errors are clustered by country dyads. Column Ind. p-val reports the p-values for the test that $_{1;1} = {}_{1;p}$ for $p = 2 \ldots 5$. Column Joint p-val provides p-values for the hypothesis that $_{1;1} = {}_{1;2} = \quad = {}_{1;p}$ for $p = 3 \ldots 5$.

Even more encouragingly, within-means of the exporters' time-varying regressors lead to very similar estimates of $_1$. This is consistent with the classical measurement error which demands that all TIRs ought to be multiplied by a single $_1$ (cf. Section 1.2.3), which indeed seems to be the case in this dataset. Unfortunately, importers' within-means produce to extremely noisy estimates of the scaling factor, making it di cult to draw any rm conclusions from them (although we note that their equality with the remaining estimates is not rejected in joint tests). Therefore, the conclusion made on the basis of the three most precisely estimated values of $_1$ is that the data are consistent with the structural model in this paper.

[5] The model passes the other sanity check since out of the 10,000 boostrap replications (not reported), the estimated variance of the measurement error was never negative (with minimum at 3.6).

1.6 Concluding remarks

This paper offers a method for accommodating time-invariant regressors into linear panel models when the presence of fixed effects is suspected. To this end, it shows that it is possible to identify the relevant TIR coefficient, provided that a proxy for the unobserved heterogeneity can be procured. The procedure hinges on correcting the bias arising from classical measurement error in the proxy variable. A specification test has been derived that allows checking whether the proposed technique is suitable for a given application. Monte Carlo evidence shows that the estimator preforms well even in small samples, though it can be very noisy when the proxy is a poor substitute for the true omitted variable.

To illustrate the use of the method, we estimate a simple gravity model of international trade. The literature suggests that the large estimated distance elasticity in international trade flows is in part due to omitted variable bias. In our application, adding an imperfect control for a prime-suspect omitted variable changes the estimated elasticity by less than 2%. However, once measurement error in the proxy is accounted for using the method proposed here, the coefficient changes by more than 47%, resulting in an estimate that is in accord with other lines of evidence. These results emphasize the point, raised recently by Oster (2016) and Pei et al. (2018), that adding an imperfect control variable into a model creates an illusion of coefficient stability, which in turn leads to a substantial underestimate of the true omitted variable bias.

In addition, this paper proposes a technique for measuring the robustness of results from random-effects models to the potential correlations between TIRs and unobserved heterogeneity. This approach is attractive as it avoids the need to assume exogeneity of the TIR in question, or to use instruments, or indeed to find a proxy for the omitted variable.

2. Which Sanctions Matter? Analysis of the Western and Russian Sanctions of 2014

co-authored with Jan Hanousek

2.1 Introduction

International trade, along with other macroeconomic phenomena, is of prime interest to policy makers, and, at the same time, poorly accessible to empirical scrutiny due to the rarity of natural experiments that would reveal changes in trade flows in response to a change in a single variable *ceteris paribus*. In this paper, we utilize trade sanctions imposed by the EU and US on (specific) trade exchanges with Russia, and broader sanctions that Russia, in response, imposed on imported EU food products. This offers a unique opportunity to analyze trade sanctions: first, the data have been generated by a natural experiment; second, we can compare and analyze the effectiveness of narrow versus broadly defined sanctions, and finally, we can empirically test the effectiveness of sanctions imposed on exports and imports, respectively.

For identification purposes, the dramatic events of 2014 in Ukraine, which precipitated rounds of sanctions on imports into the Russian Federation, are an extraordinary opportunity to analyze the dynamics of international trade flows in response to restrictive measures. This episode has a solid claim to be a natural experiment due to the geo-political considerations that drove the imposition of sanctions. If the sanctions had been imposed on primarily economic grounds, then there would have been strong reasons to suspect that countries selected endogenously into the sanctions regime. In this case, the alliance between the US and the EU created powerful incentives to cooperate against Russia, despite the misgivings of individual states. Thus, the selection into sanctions may be viewed as nearly-randomly assigned, opening a unique window into the e ectiveness of international sanctions.

The motivation for focusing on economic sanctions and their e ectiveness is the puzzlingly mixed evidence on the topic. Hufbauer et al. (2007) conclude that economic sanctions appear to be e ective in compelling the target country to make concessions to the sender countries in about one third of cases (earlier versions of this analysis are Hufbauer et al., 1985, 1990). However, these ndings have been found to be sensitive to the choice of econometric speci cation (Drury, 1998, whose criticisms against Hubauer et al. (1990) were not addressed in the updated version). Furthermore, Pape (1997) pointed out that Hufbauer et al. (1990) seemed to have coded episodes as \successes" of economic sanctions in several cases which did not warrant it, [1] thereby compounding the uncertainty surrounding their empirical ndings.

In a long stream of economic literature, several hypotheses have been put forward that might explain the limited e ectiveness of sanctioning measures. Galtung (1967) suggests that curtailing international trade may stimulate the target country's internal markets and potentially provoke perverse political responses in the target country (see also Brooks, 2002, for a similar view). Another salient threat to the e ectiveness of sanctions is the formation and enforcement of a multilateral agreement on the imposition of measures that create costs to the sender countries (e.g. Mans eld, 1995; Kaempfer and Lowenberg, 1999; Drezner, 2000). In fact, since securing the universal enforcement of sanctions appears so di cult, it has been suggested that economic sanctions ought not to be viewed as a means of punishing the target country, but rather as a way of advancing the agendas of lobbying groups within the sender countries (Kaempfer and Lowenberg, 1988). Alternatively, sanctions might be seen as a form of signaling, in which the sender country's government seeks to persuade its voters and allies abroad that it is willing to take a tough stance against adversaries (Lindsay, 1986).

We contribute to this line of inquiry by studying the most fundamental rela-

[1] These charges remain unanswered in Hufbauer et al. (2007). For a partial rebuttal, see Elliott (1998), which is addressed in Pape (1998).

tionship associated with economic sanctions. Instead of searching for an e ect of sanctions on changes in the target country's policy, we estimate their on trade ows into the target country since any e ects on other variables (such as policy outcome or consumer welfare) are necessarily predicated on the sanctions' e ect on trade ows. This allows us to observe the direct e ect of the restrictive measures instead of an e ect mediated through a host of other variables, thereby o ering a clearer view of the dynamics induced by sanctions. Our identi cation strategy exploits the recent episode of EU and US sanctions against the Russian Federation in response to the Russian annexation of the Crimean peninsula. This situation is particularly interesting as it consists of two di erent sanctions packages: rst the Western restrictions on exports of equipment for oil and gas extraction into Russia, and second the Russian counter-sanctions against the imports of Western foodstu s. To our knowledge, this paper is the rst to analyze a natural experiment that involves a dual sanctions episode. The signi cance of this pairing of the restrictive measures in this instance is the plausibility of claiming that other other factors are held constant. Both of these twin sanctions packages involve the same set of countries, they occur in the same time frame, and they were both prompted by the same sequence of events. Therefore, the di erences in the sanctions' e ectiveness can be securely attributed to the di erences in their implementation.

Indeed, these two sets of sanctions were imposed in very di erent ways and appear to have di erent e ects. Data on imports of goods that have been sanctioned, and on goods very similar to them,[2] show that the Western sanctions have a very modest impact on trade in ows of sanctioned goods into Russia, while the Russian counter-sanctions cut the imports of foodstu s substantially.

This state of a airs can be naturally attributed to the di erence in scope of the two sanctions packages. The Western sanctions targeted a very narrow class

[2] Our dataset resolves trade ows by product categories at the 6-digit Harmonized System level. Some of the goods were sanctioned at the 8-digit level and therefore we observe blocks of goods that contain some sanctioned items and some items that are very similar, but nevertheless not subject to the sanctioning regime. This \granularity" problem is noted also by Crozet and Hinz (2016, pp. 24{25) who use the same data source. See the discussion below and Appendix A.3 for further details.

of goods, thereby allowing Russian importers to find very close substitutes (so close that we are unable to distinguish them from the actual sanctioned goods in our data), which is reflected in the data as a minuscule effect of sanctions on trade flows. In contrast, the Russian sanctions restricted trade in much broader terms, so that no similar substitution was feasible. The more modest approach of the Western coalition to sanctions likely stems from concerns that more radical measures against the Russian oil and gas industry would harm European energy sectors (e.g. Wagstyl, 2017).

In addition to this mechanism, Russia, by exercising sanctions against imports into its territory may be in an inherently stronger position of enforcement since imports are easier to control and verify in comparison to exports (Feenstra et al., 2005). The asymmetry between sanctions on imports from the target country as opposed to sanctions on exports to it also lies in the fact that while the former removes foreign markets for the sanctioned exporters, the latter creates new markets for the domestic producers in the target country (Brooks, 2002). Therefore, Russian food producers have been provided with incentives to support the perpetuation of sanctions, while Western producers of mining equipment gain a motivation to lobby their government to relieve the restrictions.

The findings of this paper complement several earlier works that attribute the decline of trade with Russia to a decline in oil prices and weakening of the Russian ruble, as well as finding that sanctions on their own had a rather modest impact (Dreger et al., 2016; Ahn and Ludema, 2016; Crozet and Hinz, 2016). The closest work to this paper is the analysis by Crozet and Hinz (2016), who use the same dataset but focus on the effects of sanctions on non-sanctioned trade flows. In addition, unlike Crozet and Hinz (2016), who use a gravity model of trade, we opt for a differences-in-differences specification. We select a sample of very similar goods and compare trade flows of those that are subject to sanctions with those that are not. In this way, we are estimating the effect of sanctions without general equilibrium effects, but at the same time, our estimates exploit the natural experiment that occurred in this episode: by comparing similar goods, some of which were assigned into sanctions treatment, we argue that we obtain causal estimates.[3] Even though it could be argued that the sectors selected for sanctions are endogenous with respect to the trade dynamics, we avoid this problem by creating counterfactuals from goods that belong to the same sector as the sanctioned ones.

Finally, we note that analysis of other components of the Western sanctions package, such as restrictions imposed on specific firms and individuals is beyond the scope of this study, which focuses on aggregate trade flows. Indeed, there is evidence that entities subject to these specific sanctions were impacted significantly even though the impact on the Russian economy as a whole was likely modest (Ahn and Ludema, 2016). These findings are consistent with the theoretical literature, which stresses the importance of aiming sanctions on politically important groups within the target countries (Galtung, 1967; Brooks, 2002). As emphasized by Ahn and Ludema (2016), the main focus of the Western sanctioning measures were highly politically connected Russian firms and individuals, while the sectoral sanctions were largely complementary to the individual-specific sanctions. In contrast, Russian sanctions target agricultural production, which is of major concern to American and European politicians.

The remainder of this paper is structured as follows: first we survey the 2014 Russian sanctions episode and describe data that are analyzed to estimate the impact of the sanctions packages. We follow the empirical analysis with robustness checks and brief concluding remarks.

2.2 Background

The historical and political circumstances surrounding the imposition of sanctions are well summarized in recent literature (e.g. Dreger et al., 2016; Crozet and Hinz, 2016; Moret et al., 2016) and hence the discussion here will be limited to the essential facts that are pertinent to the discussion of economic sanctions. Following the annexation of the Crimean peninsula by the Russian Federation in March of 2014, a coalition of Western countries (EU, US, Canada and their allies) imposed

[3]It is nevertheless encouraging that the parameters obtained here lead to a similar estimate of lost trade as the one obtained by Crozet and Hinz (2016).

a series of measures restricting trade with Russia. Initially, these measures targeted speci c Russian citizens and entities but from mid-2014, the restrictions were expanded to curtail trade in military technology and equipment for the oil and natural gas industry.[4] In response to the Western sanctions, in August of 2014, the Russian Federation imposed retaliatory measures restricting imports of foodstu s from the EU, US, and their allies.[5]

Several features of these sanctioning measures are noteworthy. The rst important point to note is that the Western sanctions were imposed at the 8-digit level of the Harmonized System (HS) for classi cation of goods, whereas Russian counter-sanctions were imposed at the 4-digit level, thereby covering signi cantly wider product categories. Thus, nding close substitutes for the sanctioned imports is arguably more di cult under the Russian sanctions. Similarly, there is a much smaller potential for re-classi cation of goods into non-sanctioned categories. This practice has been documented in the context of tax evasion, where products subject to higher import taxes are re-classi ed as similar, but less taxed products (Fisman and Wei, 2004). In a more extreme version of this scheme, the same shipment is imported under a low-tax classi cation and exported under a high-tax classi cation multiple times, each time allowing the fraudulent exporter to reclaim the tax upon export, which in reality has never been incurred (Baloun and Scheinost, 2002).

A related concern to be raised in this context is the limited retroactivity of the EU sanctions. For contracts made prior to the imposition of sanctions, the sanctioned goods may still be exported to Russia, even if sanctions are in place, provided that the exporters obtained permission from a relevant authority in their home country. Analogous provisions exist in the US sanctioning measures.[6] This

[4] For the EU sanctions, see Council Regulation (EU) No 833/2014 (http://data.europa.eu/eli/reg/2014/833/oj). US trade sanctions are imposed by Directive 4 of the O ce of Foreign Assets Control under Executive Order 13662 (https://www.treasury.gov/resource-center/sanctions/Programs/Pages/ukraine.aspx).

[5] The relevant measure is the Decree of the President of the Russian Federation No. 560, English translation is available from http://en.kremlin.ru/events/president/news/46404.

[6] For EU sanctions, cf. Council Regulation No 833, sections 2.2, 3.5, and 4.2. For the US analogue, cf. Sectoral Sanctions Identi cations List of the O ce of Foreign Assets Control.

discretionary element of the Western sanctions has the potential of facilitating exports of sanctioned goods into the Russian Federation, thereby reducing the effectiveness of the Western sanctions. A remarkable example of this limited retroactivity was the sale of two French Mistral warships to Russia. The delivery of the warships would be permissible despite sanctions being in place, since the deal was struck in 2010, but due to political considerations, the warships were not delivered and the Russian Federation was reimbursed (Tavernise, 2015). To our knowledge, there is no parallel limited retroactivity implemented in the Russian sanctions. In addition, even if some limited retroactivity provisions had been implemented on the part of the Russian Federation, it is doubtful that there would have been many pre-sanctions contracts covered by these hypothetical provisions. Contracts for the supply of foodstuffs are unlikely to be arranged long in advance due to concerns with production uncertainty and food safety (Starbird, 2005). As a result, most of the pre-sanctions contracts would have expired soon after the sanctions were imposed. Therefore, this adds another layer of difficulty for parties wishing to avoid the Russian sanctions, although anecdotal reports have been made of schemes that have managed to sidestep them (e.g. Kiselyova and Popova, 2016).

2.3 Data

We use data from the *Base pour l'Analyse du Commerce International* (BACI), which is constructed from the COMTRADE database maintained by the United Nations Statistics Division (see Gaulier and Zignago, 2010, for a detailed description of these data). This is a panel dataset of bilateral trade flows disaggregated by 6-digit Harmonized System (HS) product categories. For each pair of countries in a given year, typically several trade flows are recorded, reflecting flows of different commodities. These trade flows are recorded both in terms of the value traded (in thousands of US dollars) and as the quantity traded. Since quantities reflect the unit of measurement for each type of good separately (tons, meters, etc.), they are not comparable across different commodities and hence we will use trade values in our models. Mindful of the fact that prices may reflect changes in exchange rates as well as changes in quantity traded, we account for potential exchange rate fluctuations by including period fixed effects.

The sample to be analyzed in this paper consists of trade flows into the Russian Federation between 1995 and 2016. We further limit the sample to products in the same 4-digit HS category as those that are subject to sanctions. In this way, we will be comparing sanctioned and non-sanctioned products that nevertheless belong to a broadly comparable category thereby avoiding contamination of results stemming from different dynamics of trade flows of very different product categories (Bena and Jurajda, 2011).

Having a panel dataset of trade inflows into Russia of sanctioned goods and their near-substitutes, we then construct two dummy indicators of the sanctioned status: one for the extraction equipment sanctioned by the US, the EU, and their allies, and a separate dummy for the retaliatory sanctions against foodstuffs imposed by the Russian Federation (see Appendix for further details). These dummies are equal to one if the exporter i is under the sanctioned regime and the good category j is subject to sanctions under that regime. For the main sample, only US and EU member states are classified as exporters subject to the sanction regime, while their allies who also joined the sanctions are excluded from the sample altogether[7]. The reasoning for this twofold: Firstly, we wish to limit potential problems with sample selection by eliminating countries that joined the sanctions regime on their own accord following the move by the EU and the US. Secondly, these excluded observations contribute relatively little to the trade flows studied. Including these observations predictably leads to a minuscule change in the estimates (see robustness checks below).

Due to the data limitations, some measurement error is present in these dummies for sanctioned goods. Since the BACI data resolve product categories only at

[7] We code the following countries as "sanctioning exporters:" Austria, Belgium-Luxembourg, Bulgaria, Croatia, Cyprus, Czech Rep., Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Latvia, Lithuania, Netherlands, Poland, Portugal, Romania, Slovakia, Slovenia, Spain, Sweden, USA, and United Kingdom. Sanctioning exporters that were dropped from the main sample but are included in the sample for robustness checks are: Albania, Australia, Canada, Iceland, Montenegro, Norway, Switzerland, and Ukraine.

the 6-digit HS level while the sanctions are imposed at an 8-digit level, we will be falsely categorizing some products as sanctioned. At the same time, however, this measurement error affects the interpretation of the results rather subtly. To the extent that there was a decrease in the imports of the sanctioned items without a contemporaneous increase in non-sanctioned imports in the same 6-digit category, this decrease will be visible in the data. Therefore, a decrease in imports at the 6-digit level can be interpreted as a consequence of the sanctions. On the other hand, failure to detect a change can mean that importers are substituting away from the sanctioned items to the non-sanctioned ones, or that exporters are able to ship sanctioned goods to Russia under contracts that pre-date the sanctions.

Another limitation of the data is the presence of missing values. While BACI data do contain information about zero trade flows, this information is not available for all country dyads and all years. For our main specifications, we do not replace missing trade flows with zeros, but in robustness checks we show that the addition of zeros makes only a modest difference in the estimated effects.

Furthermore, it is worth noting that trade inflows of extraction equipment vary quite wildly in time. Seasonality in extraction equipment is to be expected as these goods are imported in large one-off deliveries when oil and gas producers expand their capacity (Crozet and Hinz, 2016). The pronounced, but short-lived peaks in Figure 2.1 indicate the presence of these dynamics. In addition, trade flows are very unequally distributed across different exporters, which further increases their variances.[8]

[8] These variances are provided in summary statistics for individual product categories reported in Tables A.2 and A.3, which are relegated to Appendix A.3 in the interest of space.

Figure 2.1: Timeline of trade in ows of sanctioned goods into Russia. The rst year of sanctions (2014) is indicated by a vertical line.

(a) Extraction equipment

(b) Foodstu s products

Note that Figure 2.1 also shows that the inflows of extraction equipment do not seem to have responded to the imposition of sanctions in 2014 (indicated by the vertical line), while at the same time, imports of foodstuffs have declined sharply. We do observe a decline in goods (extraction equipment as well as foodstuffs) imported from non-sanctioned exporters. This fact makes it very difficult to argue that the trade flows were diverted via non-sanctioning countries in order to bypass sanctions. While it is still possible that there were instances when sanctioned goods were indeed imported despite the restrictions (Geller et al., 2014; Kiselyova and Popova, 2016; Yeliseyeu, 2017), the general trend shows a clear decline of imports even from non-sanctioning exporters. As documented by Dreger et al. (2016) and Crozet and Hinz (2016), a decline in imports is likely a consequence of the weakening of the Russian ruble and falling oil prices.

2.4 Empirical specification

In order to evaluate the indications from Figure 2.1 rigorously, we analyze the trade data using a differences-in-differences model. The model is specified as a panel regression with fixed effects for each exporter-good pair (at 6-digit resolution) as well as time fixed effects. In the main specification, time fixed effects are interacted with a Sanctions dummy taking a value of one if a given exporter-good pair is subject to a sanctioning regime and zero otherwise. This dummy is time-invariant, which allows us to interact pre-treatment year dummies with it to test the parallel trend assumption. A simpler model was estimated which replaces the interactions with time fixed effects by interaction with a dummy indicating the post-sanctions period. Formally, the main specification can be written as:

$$Y_{ijt} = \alpha_{ij} + \gamma_t + \sum_{s=2011}^{2016} \beta_s \text{Sanctions dummy}_{ij} \times 1(t = s) + \varepsilon_{ijt}; \qquad (2.1)$$

where Y_{ijt} are imports originating in country i of commodity j in year t. Analogously, the simplified specification is:

$$Y_{ijt} = \alpha_{ij} + \gamma_t + \beta \text{Sanctions dummy}_{ij} \times 1(t \geq 2014) + \varepsilon_{ijt}. \qquad (2.2)$$

Both models were estimated for 2010{2016, even though data is available since 1995. The reason for exclusion of the older portion of the dataset is a concern for the stability of the data-generating process which might invalidate the estimation results. For example, the crisis of 2008 would be one potential point that could have altered trade dynamics. Standard errors were clustered by each exporter yielding a sample with 100 clusters for sanctions on the extraction equipment and 126 clusters for food products.

The di erences-in-di erences model rests on the crucial assumption that observations after the imposition of sanctions for exporter-good pairs outside the sanctions regime constitute the appropriate counterfactual for the exporter-good pairs that are subject to sanctions. For example, if the treatment group of imports exhibited a pro-cyclical dynamics, while the control group was counter-cyclical, then this model would simply detect this di erence in cyclical behavior rather than the e ect of sanctions. This \parallel trend" assumption can be defeated by data if there are signi cant pre-intervention interactions between the Sanctions dummy and year e ects. Intuitively, this test uses the fact that in the pre-intervention period, we observe the trend in both treated and control groups. As a result, it is possible to test whether these observed trends are the same. Hence, we conduct a joint test of the signi cance of three pre-sanctions interactions by an F-test using the cluster-robust variance-covariance matrix. The signi cance of pre-treatment interactions would indicate a rejection of the crucial \parallel trend" assumption.

2.5 Results

Russian imports that are subject to the Western sanctions, as well as imports that are subject to the Russian counter-sanctions, were modeled separately by the the main differences-in-differences specification (2.1) and by its simplified version (2.2). Results from all four of these models are reported in Table 2.1. In the interest of space, pre-intervention interactions are omitted and the results of the joint test of their significance are presented instead. Figure 2.2 plots all the coefficients, including the pre-treatment periods, for a convenient evaluation of the treatment effect and possible pre-trends. The test of the joint significance of pre-intervention interactions as well as the graphical representation indicate that the data are consistent with the parallel trends assumption. Failure to reject the significance of pre-trend dummies also suggests that either there is no endogenous selection into the sanctioning regime, or it if there is such a selection it is too weak to manifest itself in the data. Hence, we find that the differences-in-differences methodology seems appropriate for this dataset.

Table 2.1: Di erences-in-di erences model of trade in ows into Russia (millions USD).

Sanctioned goods:	Extraction equip.		Foodstu s	
	(1)	(2)	(3)	(4)
Sanctions 1(t = 2014)	-1.653	|	-3.664**	|
	(1.080)		(1.162)	
Sanctions 1(t = 2015)	-2.874*	|	-6.964***	|
	(1.342)		(1.564)	
Sanctions 1(t = 2016)	-2.732	|	-6.184**	|
	(1.381)		(1.987)	
Sanctions 1(t 2014)	|	-2.821	|	-3.697***
		(1.588)		(0.739)
Good Exporter FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Observations	12,914	12,914	19,782	19,782
Clusters	100	100	126	126
Tests for joint signi cance of Sanctions year interactions (p-values)				
Test 2011{2013	0.73		0.59	
Test 2015{2016	0.17		$<$0.01	
Test 2014{2016	0.11		$<$0.01	

Notes: First panel of Table 2.1 shows the e ect of sanctions in particular year (i.e., the interaction of sanctions with the year dummies). Columns (1) and (3) refer to models in which we consider di erent e ects of sanction in each year, while columns (2) and (4) correspond to the case when we assume same sanction e ect across all years. In all models we control for good and exporter xed e ects, as well as time xed e ects.
Bottom panel presents joint tests of signi cance of the sanctions in the speci ed periods. While the e ect on extraction equipment was negative, it was signi cant only in 2015 on 10% signi cance level, when considered in joint tests it lost its signi cance. On the other hand, food-products sanctions shown to be highly signi cant in all cases, including joint testing.
Standard errors clustered at the exporter level are reported in parentheses. Signi cance codes: * 10%, ** 5%, *** 1%

Figure 2.2: Di erences-in-Di erences plots indicating the mean trade in ows into Russia per exporter-good dyad (in millions USD). First year of sanctions, 2014, is indicated by a vertical line.

(a) Extraction equipment

(b) Foodstu s

Post-intervention interactions show a remarkable pattern: Western sanctions against extraction equipment do not seem to have an effect on trade inflows into the Russian Federation. The interaction coefficients are negative, indicating that trade flows were lower than they would have been in the absence of sanctions, but this difference is statistically insignificant. Only at the 10% level, are we able to find a single significant interaction, in 2015, but the significance disappears once interactions in 2015 and 2016 are tested jointly. Since the interaction coefficients represent the mean change in trade flows per exporter-good dyad, it is not surprising that they are somewhat limited magnitude. Scaling them up by the number of exporter-good dyads and summing up all these scaled interaction coefficients across 2014–2016 in Column (1) leads to an estimate of the overall value of the lost trade in extraction equipment, which is about 1.3 billion USD (SE = 0.7). This would mean that for 2014–2016, the lost trade value accounts for about 14% of the total trade inflows of sanctioned goods for the three-year period prior to sanctions (2011–2013). However, this value is also only significant at the 10% level. Therefore, the differences-in-differences models indicate that at most, the effect of sanctions is too small to be detected in BACI data.

The failure to find a statistically significant decline in trade flows into Russia after the imposition of sanctions may appear counter-intuitive, but this is easily explained by the fact that the Western sanctions have been imposed on a very specific set of products that we are unable to distinguish from their close substitutes in our dataset. The sanctions apply to products at the 8-digit HS resolution, while data are available only at the 6-digit level. As a consequence, Russian importers may be switching to non-sanctioned alternatives that fall within the same 6-digit HS code giving the appearance of a subdued effect of sanctions at the 6-digit level. It is also possible that Western exporters may be re-classifying their products at the 8-digit level to evade sanctions while keeping the same 6-digit classification. As an example for potential re-classification, one may cite the ban on exports of "seamless drill pipes" (CN 73042200) while exports of seamless "tubing of a kind used for drilling for oil or gas [made of] stainless steel" (CN 73042400) were allowed. A firm trying to evade sanctions might have sold the former kind of tubing as the latter. However, in our companion paper (Bělín and Hanousek, 2021), we show that the exemptions for pre-existing contracts are a more likely explanation, at least in the European context.

On the other hand, the retaliatory sanctions have a very pronounced negative impact on imports, which are overwhelmingly statistically significant. Figure 2.2 clearly shows the notable fall in trade inflows of foodstuffs into Russia, compared to a rather modest dip in the imports of extraction equipment. Similarly, the estimate of the overall value of lost trade is 10.5 billion USD (SE = 2.5), which is statistically significant even at the 0.1% level (constituting about a 16% reduction in trade value compared to the period before sanctions). This value may seem somewhat low,[9] but it does not reflect the full effect of the trade restrictions. In particular, costs incurred by Western firms relying on Russian markets may be substantial. Nevertheless, our model indicates that the Russian sanctions resulted in about 8 times greater loss of trade than the Western ones (SE of this ratio is 3.6).

At this point, it is worth emphasizing that the standard errors for sanctions-year interactions are actually *larger* for foodstuffs than for extraction equipment (compare columns (1) and (3) in Table 2.1). Therefore, the statistical insignificance of Western sanctions is not attributable to insufficient power. Even if standard errors were to be disregarded entirely, the conclusion that the value of lost trade in foodstuffs is roughly 8 times larger than the lost trade in mining equipment is unaffected. Furthermore, in both models (for foodstuffs and extraction equipment), the counterfactual outcome was constructed from goods that are similar to the sanctioned items (falling within the same 4-digit HS classification) and therefore the observed effect can be interpreted as the effect of sanctions alone rather than an artefact arising from different cyclical behavior of foodstuffs as opposed to mining wares.

Our results are consistent with those of Dreger et al. (2016) who fail to find

[9] It is lower than the result obtained from gravity model estimated by Crozet and Hinz (2016) who find an effect of 10.7 billion USD for the period between 2014 to mid-2015. We found an effect of 10.5 billion USD from 2014 to the end of 2016.

an effect of the Western sanctions on the exchange rate of the Russian ruble. A natural interpretation of the small estimated effects of Western sanctions in comparison with the Russian countermeasures is that the Western sanctions are less restrictive than the Russian ones. Indeed, there are several reasons supporting this interpretation. First, Western sanctions target more narrowly specified classes of goods, making it easier for Russian importers to find substitutes or for Western exporters to re-classify their exports in a manner similar to the cases documented by Baloun and Scheinost (2002), Hignett (2004), or Fisman and Wei (2004). A second reason why exporters might be able to send sanctioned goods into Russia may be the provisions of EU and US sanctions packages that allow exemptions from sanctions. These exemptions may be claimed for delivery of sanctioned goods that were ordered prior to the imposition of sanctions. Both of these mechanisms make it possible for goods classified at 6-digit HS codes to be imported into Russia and thus we observe only a minuscule effect of sanctions on extraction equipment. In contrast, Russian sanctions, which are imposed at the 4-digit level and do not appear to contain provisions for limited retroactivity cut the trade inflows much more effectively.

2.6 Robustness checks

In the differences-in-differences models above, all exporting countries have been treated identically, i.e. a single average change in trade flows was estimated, from which the total trade losses were computed. This raises the question whether some heterogeneity among the exporters is neglected in our baseline models. In particular, since the Western sanctions afforded a significant degree of discretion to the authorities within each sanctioning country in the enforcement of sanctions (see Section 2.2), one might expect countries with stronger trading ties to Russia to be more reluctant to adopt strict enforcement policies.

For this reason, we have partitioned the treated group of exporters into five subsamples with different characteristics: the first subsample consists of the top 5 exporters that accounted for nearly 77% of the value of imports of sanctioned goods into Russia between 2010 and 2013 (i.e. Germany, USA, Italy, France, and Sweden). The second subsample consists of the USA plus 3 largest European countries in terms of GDP (Germany, United Kingdom, France). These three large European exporters constitute the treatment group in the third subsample, while all the remaining European exporters are coded as the treated group in the fourth sample. The fth sample consists of eastern European countries and the Baltic nations. In each subsample, we have kept only observations belonging to the non-sanctioning exporters (control group) and the relevant subroup of sanctioning exporters. This was done to prevent misclassi cation of sanctioning exporters as \controls."

Furthermore, since the statistical power of the baseline models is a ected by the measurement error in our classi cation of sanctioned exports, we arti cially increase the power here by using classical standard errors assuming iid error terms. The purpose of this exercise is to check to what extent statistical inference changes when a less conservative approach is taken. Non-clustering is likely produce overly narrow con dence intervals since it ignores correlations induced by the need to transport the goods by similar routes and making other logistical arrangements that depend on the country of origin.

Table 2.2: Parameter heterogeneity (Baseline model)

	All	Top 5	Large	Large EU	Small EU	Eastern EU
Total change in trade	-1.31	-1.03	-1.06	-0.94	-0.31	-0.28
Classical 95% CI	-2.35	-1.74	-1.72	-1.51	-1.04	-0.84
	-0.27	-0.32	-0.4	-0.38	0.43	0.28
Clustered 95% CI	-2.59	-2.22	-2.25	-2.11	-0.69	-0.63
	-0.03	0.16	0.12	0.22	0.08	0.06

Notes: Di erences-in-di erences models speci ed by Eq. (2.1) estimated on di erent subsamples of the sanctioning exporters. EU+USA = replication of results in Table 2.1; Top 5 = 5 countries that accounted for the greatest propotion of imports of sanctioned goods into Russia in 2010{2013 (i.e. Germany, USA, Italy, France, and Sweden); Large = largest exporters in terms of GDP (USA, Germany, United Kingdom, France); Large EU = largest European exporters in terms of GDP (Germany, United Kingdom, France); Small = smaller exporters (Austria, Belgium, Bulgaria, Cyprus, Czech Rep. Denmark, Estonia, Finland, Greece, Croatia, Hungary, Ireland, Italy, Lithuania, Latvia, Netherlands, Poland, Portugal, Romania, Slovakia, Slovenia, Sweden); Eastern EU = Eastern European and Baltic countries (Bulgaria, Czech Rep., Estonia, Hungary, Latvia, Lithuania, Poland, Romania, Slovakia).

All speci cations include time xed e ects as well as xed e ects for the exporter-good dyads. All parameter estimates are in billions USD. Classical con dence interval (CI) assumes iid disturbances while clustered CI assumes clustering by the exporting country.

Table 2.3: Parameter heterogeneity (Simpli ed model)

	All	Top 5	Large	Large EU	Small EU	Eastern EU
Total change in trade	-1.54	-1.46	-1.37	-1.19	-0.24	-0.15
Classical 95% CI	-2.23	-1.93	-1.8	-1.56	-0.73	-0.52
	-0.86	-0.99	-0.94	-0.82	0.24	0.22
Clustered 95% CI	-3.09	-2.92	-2.81	-2.53	-0.56	-0.38
	0	0.01	0.06	0.16	0.07	0.09

Notes: Di erences-in-di erences models speci ed by Eq. (2.2) estimated on di erent subsamples of the sanctioning exporters. Row and column labels are maintained from Table 2.2.

As Tables 2.2 and 2.3 show, these additional results not change the conclusions from the baseline models in a substantial manner. Re-estimating equations (2.1) and (2.2) using inappropriately small standard errors leads to a modest narrowing of the con dence intervals. While some parameter heterogeneity can be inferred from Tables 2.2 and 2.3, it is di cult to draw any rm conclusions. If anything, it appears that the top 5 exporters accounted for slightly larger fraction of the lost trade than the smaller exporters. Taking the baseline speci cation Table 2.2, the top 5 exporters lost 1.03 billion USD in trade, which is about 91% of the total trade lost among all sanctioning exporters. In contrast, these 5 exporters accounted for 77% of exports in the three years prior to the imposition of sanctions. It would therefore appear, that the countries with stronger trade ties bore higher costs of sanctions, in comparison with the other sanctioning countries.

As another robustness check, we run a di erences-in-di erences model without the exclusion of countries that joined the sanctions regime other than the US and the EU. In addition, missing values in the dataset have been replaced by zeros. A comparison of results in Table 2.4 with the main speci cation reported in Table 2.1 shows that the di erences in estimates are almost negligible. Even though the interaction coe cients in the extraction equipment model do become signi cant in this expanded dataset, their signi cance is marginal. Therefore, even though the models are consistent with a decline in the imports of extraction equipment, the decline is modest in comparison with the other uctuations in the data.

Table 2.4: Di erences-in-di erences model of trade in ows into Russia (millions of USD) with added zeros and full sample of exporters.

Sanctioned goods:	Extraction equip.		Foodstu s	
	(1)	(2)	(3)	(4)
Sanctions 1(t = 2014)	-1.058	|	-0.411**	|
	(0.643)		(0.125)	
Sanctions 1(t = 2015)	-1.372*	|	-0.921***	|
	(0.756)		(0.198)	
Sanctions 1(t = 2016)	-1.788*	|	-0.920**	|
	(0.742)		(0.205)	
Sanctions 1(t 2014)	|	-1.538*	|	-0.877***
		(0.758)		(0.183)
Good Exporter FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Observations	78,155	78,155	230,405	230,405
Clusters	145	145	145	145
Tests for joint signi cance of Sanctions year interactions (p-values)				
Test 2011{2013	0.53		0.1	
Test 2015{2016	0.19		$<$ 0.01	
Test 2014{2016	0.13		$<$ 0.01	

Notes: The results presented here test the robustness to the replacement of missing values by zero and expanding the sample to include sanctioning exporters beyond EU and US. These results lead largely to the same conclusions as the estimates from the baseline models (cf. Table 2.1).

Standard errors clustered at the exporter level are reported in parentheses. Signi cance codes: * 10%, ** 5%, *** 1%

In the expanded dataset, the coe cients are closer to zero than in the baseline models in Table 2.1 because here we are including many exporter-good dyads with zero trade in ows into Russia for the entire period 2010 { 2016. Hence the e ect of sanctions appears \diluted" but the conclusions remain largely unchanged: just as before, the e ect of Western sanctions led to a loss in traded value of about 2 billion USD (SE = 1.1) while Russian sanctions cost about 17.9 billion USD (SE = 3.9). One slight change compared to the baseline results is that the test for pre-trend in Column (3) narrowly rejects parallel trends, albeit only at the 10% level. Given the fact that the signi cance is marginal and that our main results survive even if the parallel trend assumption is dropped altogether (see results in Table A.4 above), the signi cance of pre-intervention terms seems to be of little consequence. As the main results presented in Section 2.5 survive even under a very di erent modeling methodology and using a very di erent sample, there is a solid basis for claiming their robustness.

Finally, we note that even though the data in this case do not reject the common trend assumption, which is crucial for the validity of the di erences-in-di erences methodology (see test of pre-trend in Table 2.1), we nevertheless conduct a robustness check that relaxes this assumption. This is achieved by estimating a time-series model for each exporter-good dyad in the period before sanctions and using the predictions from this model as counterfactuals in the period after sanctions. Encouragingly, even this modeling methodology agrees with the main results above: in this model, trade lost due to the Western sanctions amounts to about 0.8 billion USD, while the Russian counter-sanctions resulted in a loss of 5.1 billion USD (details are available in Appendix A.4).

2.7 Conclusion

This paper contributes to the long stream of economic literature on sanctions by analyzing their impact on trade flows into the target country. Specifically, we examine the Russian sanctions imposed on European and American food imports and the impact of EU and US sanctions on exports of extraction equipment to Russia.

Using a difference-in-difference approach on data covering the imports of sanctioned goods into Russia at the 6-digit HS resolution, our results indicate that the Russian sanctions decreased imports by about 10.5 billion USD, while the EU and US sanctions led to about 1.3 billion USD of lost imports of the sanctioned goods (about 8 times smaller effect). We find no evidence that this result is driven by substitution between different trade channels. Under this explanation, we would have found statistically significant effects in the differences-in-differences models of the Western sanctions. Furthermore, in the aggregate terms, non-sanctioning exporters would have to supply more of the sanctioned goods after 2014. Instead, the opposite is the case: exports of the sanctioned goods into Russia declined even from non-sanctioning countries, which can be seen in Figure 2.1 (a).

The reason for the differential impact of the sanctioning measures might be due to the different ways these sanctions were imposed. While the EU and US sanctions targeted the exports of a very narrow class of goods with close substitutes, the Russian sanctions restricted trade imports in much broader terms. This policy difference may have enabled Russian importers to find close substitutes for the sanctioned products, and therefore the impact on goods that have been sanctioned at the 8-digit HS level is undetectable in data at the 6-digit level. It is also possible that in tandem with this substitution plan, Western exporters may be re-classifying their products, thereby sending the same exports as before under labels that do not fall within the sanctions regime.

In addition, the Western sanctions allow the exporters to honor the contracts made prior to the imposition of export restrictions. Hence, it may be that no substitution and no re-classification is necessary to explain our results, provided that the Russian importers have made sufficiently long-term contracts for the supply of extraction equipment and the Western exporters obtained permissions form the relevant authorities in their home countries. Since it is improbable that many contracts for the food imports last more than a year, data on Russian sanctions are unlikely to contain much of these \grandfathered" imports, providing another possible explanation of the increased e ectiveness of Russian counter-measures compared to the Western sanctions. Finding out which e ect dominates requires a more nely-resolved dataset, which is left for future research.

3. Social Networks and Surviving the Holocaust

co-authored with Stepan Jurajda and Tomas Jelnek

3.1 Introduction

Experiencing violent conflict has been shown to support within-group cooperation (Trivers, 1971; Choi and Bowles, 2007), but it is not clear whether humans cooperate when survival chances are extremely low. Social networks have been shown to mediate the health effects of stress (House et al., 1988) and to matter in high-stakes contexts (e.g., for soldiers in a war as in Shils and Janowitz, 1948; Costa and Kahn, 2007), but it is not clear whether pre-existing social ties in broad populations are transferable to life-and-death situations.

Survival in deadly internment camps, including POW camps, Soviet Gulags, and Nazi concentration camps has been linked to the ability of prisoners to form small mutual-support groups (Davidson, 1984; McElroy, 1957; Schmolling, 1984; Applebaum, 2003), which points to the importance of social networks in extreme circumstances. However, much of the existing literature (for Holocaust research, see Eitinger, 1964; Luchterhand, 1967; Dimsdale, 1974; Sofsky, 1999; Suderland, 2013) is based on survivor testimonies, which are fundamentally selective, particularly given low survival rates. It is plausible that those who did not survive also formed mutual-support groups; therefore, statistical analysis based on all prisoners is needed to provide a new type of evidence and to complement qualitative research. Such evidence is also needed to assess whether pre-existing social ties are valuable in extremity within demographically diverse populations (i.e., among victims of the Holocaust), or whether only soldier camaraderie established on battlefield is valuable in deadly internment (as in Costa and Kahn, 2007).

In this paper, we examine the importance of social networks (linkages, potential friends) for the Holocaust survival of Theresienstadt ghetto prisoners entering

Auschwitz-Birkenau. Theresienstadt was an in-transit ghetto, while Auschwitz was a complex of labor and extermination camps. Our analysis is based on the near-complete database of a well-defined group of prisoners and thus avoids survival biases by incorporating information on those who did not survive the Holocaust. Pre-existing friendships, social and family ties may be particularly valuable in the extreme environment of a Nazi concentration camp, where there are few market substitutes for social resources. For each Theresienstadt prisoner on a transport to Auschwitz, we construct a variety of proxies for the availability of potential friends on their transport. We then ask how pre-existing social linkages affect Holocaust survival. We study several types of social ties that did not require strong social skills to be formed, and exploit variation in the availability of potential friends that was outside of a prisoner's control.

We find a survival advantage conferred by entering Auschwitz with several socially-linked fellow prisoners based on measures of family links, camaraderie among prisoners, as well as based on social linkage proxies corresponding to pre-deportation administrative and residential ties.

We build on the historical research devoted to Theresienstadt (Hajkova, 2020; Adler and Adler, 2017; Frankl, 2005; Lagus and Polak, 2006) and on the few statistical analyses of deadly internment camps and ghettos (Costa and Kahn, 2007; Suderland, 2013; Finkel, 2017). Our results confirm the findings of qualitative work based on selective survival testimonies that being socially isolated was particularly costly during the Holocaust. Our findings from a situation of extremity also fit well into the literature highlighting the importance of social links in high-stakes (but not deadly) contexts (Battiston, 2018; Fisman et al., 2018; Kelly and Grada, 2000; Stuart and Taylor, 2021).

3.2 Theresienstadt and Its Records

Theresienstadt (Terezn) was a ghetto established by the SS in 1941 in the garrison city of the same name in German-occupied Czechoslovakia. The ghetto held mainly Czech, German, and Austrian Jews, and most of the ghetto's population was eventually deported to extermination camps in occupied Poland. The ultimate destination of transports to the East was not disclosed to the ghetto self-administration. Auschwitz, the focus of our study, was the most frequent destination. We do not study prisoners deported to Treblinka and Maly Trostinec, the other two chief destinations of transports from Theresienstadt, because virtually none of these survived the Holocaust. Unlike Auschwitz, Treblinka and Maly Trostinec had no labor camp, they were solely extermination camps.

Our analysis is based on the near-complete database of individual histories of Theresienstadt prisoners compiled by the Terezn Initiative Institute (TII), a non-pro t organization founded by an international association of surviving prisoners of the ghetto. The database covers information on 139,769 incoming prisoners, for 99.8% of whom we have information available on their Holocaust survival. We also have data on all transports out of Theresienstadt covering 88,059 of prisoners. The TII data covers prisoners' names, gender, age, and academic titles including medical doctor titles. We amended the database by measures of social linkages among prisoners collected in various archives.

3.3 Social Linkages and Estimation Strategy

The literature on the coping strategies of concentration camp prisoners includes only a few statistical analyses that investigate what characteristics or strategies helped prisoners to survive. It is important that such analysis is multivariate|in order to compare otherwise comparable prisoners|and that it explores speci c mechanisms that underpinned survival. Our focus on a mechanism based on social-linkage resources is motivated by the testimonies of survivors (e.g., Davidson, 1984), which also guide our focus on several dimensions of social linkages. Finding similar e ects for multiple measures would be suggestive of systematic forces. Appendix A.5 provides details on the Theresienstadt data, sources of the archival data on social ties, and examples of relevant survivor testimonies.

A prime social-resource group is that of one's family and so it forms the basis of our rst social-linkage measure. Second, prisoners from the same pre-deportation *place of residence* can form a natural mutual-support group. We define such groups based on pre-deportation street addresses of the Theresienstadt prisoners deported from Prague (there are 1,917 Prague street addresses available in the TII data and 14,791 prisoners with this information on transports to Auschwitz). For other prisoners, we do not have street address data, only the city from which their transport to Theresienstadt came. Third, we form a measure of *administrative ties* based on the self-organization of national Jewish communities, i.e., based on membership of the official pre-deportation Jewish self-administrations (Jüdische Kultusgemeinde in Prague, Israelitische Kultusgemeinde in Vienna, Berlin; henceforth referred to as JKG/IKG). We obtained lists of the members of the three organizations in 1941 and merged them with the TII data. The majority of the 2,680 members of JKG Prague, 677 members of IKG Vienna, and 371 members of IKG Berlin who entered Theresienstadt ended up on transports to Auschwitz. The three types of social linkages described above were formed prior to internment.

Next, we consider social linkages formed during internment. A measure of social linkages corresponding to *camaraderie* is based on the case of young Czech Theresienstadt prisoners, who, according to post-war testimonies discussed in Appendix A.5, had often formed strong friendships (based, e.g., on sharing food sent from home) during their earlier internment in a low-security all-male agricultural labor camp, which was located in Lípa in a rural area of today's Czech Republic. The Lípa camp is an example of the several thousand small labor camps, in which European Jews were interned before being deported to large ghettos and concentration camps (Megargee, 2009). We merged records of the Theresienstadt ghetto with lists of Lípa prisoners. A total of 1,351 Czech Jews were interned in the Lípa camp, of whom 961 entered Theresienstadt.

Our final social-linkage measures are based on *Theresienstadt networks*. First, we observe members of a chain-mail community (104 women and 126 men, most ended up in Auschwitz) formed within Theresienstadt to share a copy of an underground satiric weekly (`Shalom for Friday', henceforth referred to using the Czech abbreviation `SNAP'). Second, we consider prisoners who came to Theresienstadt on the same in-transport to be potentially socially linked. In-transports often combined residents from a set of pre-deportation neighborhoods; further, within Theresienstadt, prisoners from the same in-transport often shared similar conditions and housing. Hence, it is possible that they formed relevant social ties.

To identify the e ect of social-linkage resources on Holocaust survival in Auschwitz-Birkenau, we use information on the number of potential friends available to prisoners across transports out of Theresienstadt, taking the composition of these transports as a setting in which the social mix of prisoners varied quasi-randomly due to the demographic pressure of transport orders given by the SS. (In the next section, we provide evidence supporting this notion.) Consider the 601 former Lpa prisoners who ended up on 23 distinct transports from Theresienstadt to Auschwitz. To identify the e ect of social-network resources on survival, we ask whether Lpa prisoners traveling to Auschwitz with a di erent number of fellow Lpa prisoners display di erent survival outcomes. We thus use variation in the number of Lpa prisoners across transports and ask whether arriving in Auschwitz with more potential friends improves survival prospects. Our analysis conditions on the average survival chances of all prisoners on a given transport to Auschwitz (by transport xed e ects), which is given by SS decisions in Auschwitz outside of prisoners' in uence. The e ects of social linkages thus correspond to the within-transport gaps in survival chances between a typical prisoner and a `Lpa' prisoner, where this gap is contrasted across transports with a varying number of Lpa prisoners. We similarly condition on the number of potential friends on a transport to Auschwitz based on all of our measures of social linkages. (While the number of Lpa prisoners, JKG/IKG members, and SNAP prisoners traveling together varies only across transports to Auschwitz, there is within-transport variation in the size of an individual's social network based on family size, pre-deportation place of residence, and on groups of prisoners who came to Theresienstadt on the same in-transport.) We measure social resources by gender given that the camp was segregated by gender. The maximum size of the set of potential friends on a transport varies from 4 (for family networks for both genders) to 295 (for men who arrived in Theresienstadt on the same transport).

3.4 Survival after Entering Auschwitz

We model differences in surviving the Holocaust for prisoners entering Auschwitz. We do not observe place of Holocaust death for the Theresienstadt prisoners who perish after entering Auschwitz. It is possible that some of these prisoners left Auschwitz for other concentration camps or ended up in one of the death marches at the end of the war. Our estimates thus speak to the extreme experience of a typical prisoner entering Auschwitz, not only to imprisonment in Auschwitz-Birkenau.

Of the 27 transports from Theresienstadt to Auschwitz, seven had survival rates of under two percent. Three transports (transport Ds in 1943 and Ek and Em in 1944) had survival rates of about twenty percent, as survival rates in Auschwitz improved towards the end of the war. We condition on the transport-specific survival rates, which we consider externally given by the prevailing conditions in Auschwitz, and so we study differences in survival relative to the transport-wide average survival rate. We exploit within-transport as well as across-transport variation in multiple types of social linkages, which we view as quasi-random. The SS specified the demographic composition of transports out of Theresienstadt, but the selection of individual prisoners was under the influence of the ghetto's Jewish self-administration for most transports. In Appendix A.5, we provide a description of the transport selection process in Theresienstadt, and show that there is no systematic relationship between transport survival rates and transport averages of social linkages (Appendix A.7). For eight transports, the selection of prisoners in Theresienstadt was controlled directly by the SS, not by the self-administration; these eight are omitted from our analysis since the selection process may have SS-specific goals (Appendix A.7 provides additional discussion). We thus study the Holocaust survival of the 14,546 male prisoners and 16,200 female prisoners of Czech, Austrian, and German origin on 19 transports.

There is no evidence in the historical literature suggesting that selection into transports would consider social linkages beyond family ties. Further, the ghetto's self-administration, which compiled out-transports of one to two thousand prisoners at a time under significant time pressure, did not have data available on many of the social ties we measure here, with the benefit of hindsight. Nevertheless, it is important to consider the possibility that Holocaust survival of prisoners entering Auschwitz was related to their unobservable pro-social traits reflected in the Theresienstadt out-transport selection. Below, we therefore assess the sensitivity of our baseline findings to unobservables by estimating sample selection models.

We test whether the improved ability to form close friendships by prisoners with access to pre-existing social linkages on their transport to Auschwitz improves chances of Holocaust survival (binary indicator S_{it}). For each prisoner i on transport t belonging to social networks of type j, we condition on the number of prisoners from his/her social network traveling on the same transport, denoted N_{ij} (e.g., for the number of Lpa prisoners|`N Lpa'). We also condition on transport indicators α_t capturing transport-wide survival levels. Finally, we condition on a set of prisoners' characteristics X_i consisting of prisoners' age, length of Theresienstadt imprisonment prior to out-transport to Auschwitz, a Prague-deportation indicator, nationality indicators, and indicators for being a member of the JKG/IKG organizations, having been in the Lpa camp, and for having family members on transport. We thus estimate the following OLS binary-outcome regression:

$$S_{it} = \alpha_t + \sum_j \beta_j N_{ij} + X_i \gamma + \varepsilon_{it} \qquad (3.1)$$

and its Probit equivalent. We cluster standard errors by transports out of Theresienstadt. Wild bootstrap inference (Cameron and Miller, 2015) confirms traditional asymptotic inference.

In Table 1 we show results for both male and female prisoners; specifically, we present average marginal effects (AMEs) from Probit and OLS models of Holocaust survival. For our measures of available social linkages, the AMEs represent the effects on survival chances of one additional linked fellow prisoner on a transport. (See Appendix A.6 for details on the calculation of the AMEs). Several types of available social linkages we observe imply that arriving in Auschwitz with a larger group of male potential friends supports survival in extreme circumstances. The estimates in the first two columns of Table 1 suggest that having been imprisoned together earlier, having resided together, and arriving in Theresienstadt together (thus sharing a network in Theresienstadt) generates social ties that confer a survival advantage in a deadly concentration camp. One of the measures of pre-deportation administrative social ties is similarly helpful. The advantage grows with the size of the group of potential friends, as this increases the chances that prisoners with social links stay together. The estimates for women in the right panel of Table 1 imply that all social linkage measures, including family ties and the SNAP linkages as well as administrative ties, are increasing survival chances. The estimated effect magnitude is consistently larger for women than for men.

In richer specifications presented in Appendix A.7, we confirm that the survival effects of social linkages are linear in the size of social networks, and we find that they are stronger for younger prisoners, who may form mutual-support `communes' more readily. Alternatively, this finding may mechanically correspond to the fact that younger prisoners had generally higher survival chances, so that there was more `scope' for social networks to support survival.

Table 3.1: Average marginal e ects from survival models

	Males				Females			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Probit	OLS	Probit	OLS	Probit	OLS	Probit	OLS
N Lipa	0.00101**	0.00330***	0.000598**	0.00325***	|	|	|	|
	(0.000416)	(0.000322)	(0.000266)	(0.000343)				
N Family	0.00280	0.00672	0.00170	0.00694	0.0102**	0.0349**	0.00428**	0.0373**
	(0.00439)	(0.00695)	(0.00251)	(0.00693)	(0.00469)	(0.0139)	(0.00208)	(0.0133)
N Same street	0.0160***	0.0327***	0.00948***	0.0328***	0.0240***	0.0978***	0.0111**	0.0954***
	(0.00587)	(0.00921)	(0.00286)	(0.00904)	(0.00337)	(0.00896)	(0.00470)	(0.00818)
N SNAP	0.00171*	0.00227	0.00101**	0.00216	0.0101***	0.0712***	0.00494**	0.0692***
	(0.000911)	(0.00165)	(0.000473)	(0.00163)	(0.00262)	(0.0103)	(0.00212)	(0.00986)
N in-transport	0.000195***	0.0000920	0.000111***	0.0000761	0.000131	0.000511	0.0000984*	0.000403
	(0.0000547)	(0.0000584)	(0.0000309)	(0.0000619)	(0.0000923)	(0.000345)	(0.0000506)	(0.000306)
N JKG Prague	0.0000941	0.0000232	0.0000558	0.0000327	0.000862***	0.00782***	0.000412**	0.00773***
	(0.000106)	(0.000104)	(0.0000648)	(0.000100)	(0.000302)	(0.00143)	(0.000201)	(0.00130)
N IKG Vienna	-0.00107	-0.00119	-0.000666	-0.00116	0.00382***	-0.000281	0.00200***	-0.000496
	(0.00104)	(0.00126)	(0.000634)	(0.00127)	(0.000744)	(0.00151)	(0.000474)	(0.00192)
N IKG Berlin	0.0338***	0.00108	0.0253***	0.00116	0.0388	0.0395	0.0184*	0.0375
	(0.00162)	(0.00149)	(0.00423)	(0.00146)	(0.0238)	(0.0438)	(0.0102)	(0.0437)
Ac. title (non-medical)	-0.0166	-0.0248	-0.00928	-0.0229	0.0289	0.120	0.0114	0.125
	(0.0177)	(0.0223)	(0.0105)	(0.0231)	(0.0198)	(0.0953)	(0.00757)	(0.0961)
Doctor	0.0529**	0.0400**	0.0330***	0.0423**	0.0505**	0.128	0.0190	0.139
	(0.0234)	(0.0142)	(0.0121)	(0.0155)	(0.0256)	(0.0870)	(0.0138)	(0.0821)
Age (in years)	-0.00185***	-0.00177***	-0.000709***	-0.00188***	-0.00129***	-0.00153**	-0.000586**	-0.00162***
	(0.0000828)	(0.000492)	(0.000118)	(0.000452)	(0.0000558)	(0.000540)	(0.000240)	(0.000520)
pval = 0	|	|	0.647	0.168	|	|	0.074	0.028
Clusters	19	19	19	19	19	19	19	19
Selected	14,546	14,546	14,546	14,546	16,200	16,200	16,200	16,200

Notes: Standard errors clustered by transports in parentheses; signi cance codes: $p < 0{:}1$, ** $p < 0{:}05$, *** $p < 0{:}01$. = correlation coe cient between residuals in the selection and survival equations. Transport xed e ects, e ects of time spent in Theresienstadt and of age in years and its square and cube, group membership xed e ects including the Austrian/Czech/German nationality indicators, Prague residency xed e ect, and selection equations not shown.

We also find in Table 1 that medical doctors were more likely to survive, possibly thanks to their valuable skills, or thanks to doctors arriving at Auschwitz in better-than-average health. Other academic titles were not helpful. The effect of age on survival is non-linear (see Appendix A.7), which cannot be conveyed by the age AME. Conditional on transport-wide survival rates and other controls, prisoners in their mid-twenties were most likely to survive among both men and women. Survival chances then decline steeply with age, such that prisoners aged 45 were about 15 percentage points less likely to survive compared to those aged 25.

We rely on variation in social linkages across transports that is driven by the transport selection pressure in Theresienstadt. We thus minimize potential omitted variable bias where prisoners who are more pro-social tend to have more friends and are more likely to survive. However, prisoners in poorer health may have found it more difficult to join social networks in Theresienstadt, such as the network corresponding to the satirical magazine distribution chain, and their poorer heath may have also reduced their survival chances in Auschwitz. We therefore assess the sensitivity of our baseline findings to unobservables. First, we estimate specifications additionally controlling for prisoners' ability to evade transport selections out of Theresienstadt before eventually ending up on a transport to Auschwitz. This control likely captures unobservable social capital and thus allows us to explore the sensitivity of our baseline findings to transport selection on unobservables. In Appendix A.7, we indeed find that Theresienstadt prisoners who were better able to evade transports (conditional on observable controls) were more likely to survive after arriving at Auschwitz, which suggests the importance of some unobservable, including potentially social linkages, for both avoiding selection and survival. Importantly, our main coefficients of interest are not materially affected by whether we control for the evaded-transport-risk regressor. Second, we estimate sample selection models linking unobservables across the transport selection in Theresienstadt and the Auschwitz survival equations. We fit the Probit model with sample selection proposed by Van de Ven and Van Praag (1981). In this model, residuals from the transport selection equation are allowed to correlate with residuals in the survival equation, allowing the model to account for the presence of unobserved heterogeneity. Mindful of the fact that this Probit model assumes joint normality of the residuals, which may be violated, we also t a distribution-free alternative by OLS. Speci cally, in the OLS model of survival in Auschwitz, we include a step function with 10 dummy variables controlling for each decile of the selection probability estimated by a rst-stage OLS. To identify these models, we rely on exclusion restrictions corresponding to the transport selection pressure in Theresienstadt that arose due to the combination of demographic-type SS transport orders with the size of prisoner groups of a given type in the ghetto at the time. When the selection pressure was higher, prisoners of a given type arriving in the ghetto on recent in-transports were often selected for the next out-transport (see Beln et al. (2022)). This resulted in a prisoner's deportation origin (the city where the in-transport originated) predicting a prisoner's out-transport selection chances. Selection pressure also led to prisoner ordering within in-transports (transport numbers assigned to each prisoner on a transport corresponded to their position on transport lists) predicting out-transport selection: prisoners at the top of an in-transport list were assigned to the next out-transport. In our Appendix A.7, we thus estimate transport selection speci cations where we control for quartile xed e ects in prisoner order on in-transports and for in-transport origins. We exclude these e ects from the Auschwitz survival equation based on our assumption that the grouping of prisoners entering Theresienstadt, which a ected their probability of being selected for an out-transport due to demographic-level selection pressure, has no predictive power for survival in Auschwitz after prisoners were re-grouped by the out-transport selection process in Theresienstadt. We thus assume that in-transport ordering of prisoners and the availability of di erent prisoner types for out-transports are independent of prisoner survival chances after entering Auschwitz.

The estimated speci cations presented in the last two columns of each panel of Table 1 are consistent with selection on unobservables having little e ect. While the estimated correlation coe cient between residuals in the Probit model is insigni cant, the joint test of signi cance of the control function dummies in the OLS model (reported under p-val: = 0) suggests statistical signi cance for women, which may indicate a violation of the joint normality. Even with a signi cant contribution from the selection equation, however, the results are similar to our baseline estimates, with the Probit estimated e ects being somewhat smaller. We conclude that both in terms of the historical literature on Theresienstadt, and in terms of our estimated models, there is no evidence that would undermine our interpretation of di erences in prisoners' social linkages across transports being quasi-random.

Figure 3.1: Expected survival advantage due to social networks larger by one standard deviation

Notes: The graph plots the expected survival advantage (in percentage points, with 95% con- dence intervals) based on survival e ects reported in columns (1) and (5) of Table 3.1 and corresponding to a 1-standard-deviation increase in the number of linked prisoners around the mean size of each social network type. E ects that are not signi cant at the 10% level are not shown.

Our baseline ndings are also robust to adding transports bound for camps-ghettos in Riga and Raasika to the analysis (see Appendix A.7 for results based on 31 transports).

The AMEs in Table 3.1 are particularly large for prisoners who resided at the same street address prior to deportation|a measure of social linkages that is both more precise and available for more prisoners than those based on other approaches. Further, the female AMEs in Table 1 are all larger than the corresponding male e ects. In Figure 3.1, we o er an alternative assessment of e ect magnitudes across all social-network measures, one based not on adding one socially linked prisoner, but on adding one standard deviation in the size of a given network type. Figure 3.1, which only visualizes the e ects of social networks from Table 1 with p values under 0.1, suggests that the Lpa social ties for men and IKG Vienna ties for women were more helpful than other social linkages, when evaluated based on a standard deviation change. The e ect of family linkages appears to be small, even though the family coe cient in Table 3.1 for women is sizeable; this is due to the fact that family groups traveling together were the smallest social networks we measure (with a maximum network size of 4). Figure 3.1 implies that a typical e ect of a one-standard-deviation increase in the size of a prisoner's social network is to improve his/her survival chances by about 2 percentage points. A similar improvement in survival chances would result from a reduction in a prisoner's age of about 3 years (within the 25-45 age bracket, where survival chances decline rapidly with age). The average of transport-wide survival rates across the 19 transports we study is 6%; hence, the 2% e ect of social networks corresponds to increasing survival chances by a third of this base rate. These are sizeable e ects, especially given that our estimates are likely lower bounds to the extent that our measures of social linkages contain measurement error, and because we do not measure all social ties between prisoners, so the base-group prisoner is not fully isolated in the social space of the camp.

Overall, we interpret our estimates as implying that the availability of potential friends supports survival in the extreme conditions of a Nazi concentration camp and that groups of socially-linked prisoners generate valuable opportunities to form small mutually-supportive `communes'.

Which mechanisms could correspond to the survival effects we uncover? Survival testimonies do not imply that small `communes' would *enforce* pro-social behavior within groups. Testimonies of Auschwitz survivors (e.g., Levi, 1947) imply that it was crucial to get *advice* on the operations of the camp from more experienced prisoners. However, our measures of social ties capture linkages among prisoners who arrive in Auschwitz together and who are similarly inexperienced in the camp's operations. Importantly, we find survival effects for essentially *all* social networks we measure, for men and women, and for youth and prime-aged adults, based on social networks corresponding to prisoner linkages and links based on pre-deportation ties. This pattern of our findings suggests that there is a common mechanism at play, one that is not based on a particular advantage such as physical strength, which would be more applicable to ties among young male Lpa prisoners than administrative or residential linkages among women. (Even though Table 1 implies that medical doctors were more likely to survive, in Appendix A.7 we do not find any survival advantage of having a medical doctor in one's social network, which is also consistent with the notion that particular advantages are not behind the pattern of our estimates.) Instead, our results are consistent with the widespread appearance of the `muselmann' phenomenon in survivor testimonies (Frankl, 1946; Levi, 1947; Agamben, 1999), where prisoners who gave up hope and the *will* to live quickly perished in the extreme conditions of the camps. Survival testimonies (we list in Appendix A.5) imply that small groups of friends were formed based on pre-existing social ties, where friends shared food and provided emotional support to each other in moments of despair, not only in moments of weakness and ill health, i.e., that such groups also helped to stimulate the will to continue fighting to survive.

The survival effects we estimate in Table 1 are larger for women, suggesting that women collaborate more or are motivated more by their friends and relatives. Future work is needed to confirm gender differences in mutual-support cooperation under extreme stress, and to shed light on the sources of such differences, including, potentially, evolutionary underpinnings. For example, Taylor et al. (2000) argue that women are predisposed toward dyadic tend-and-befriend interdependence as stress (threat) levels increase, because selection pressures for caregiving in the face of threat have operated more strongly on women than on men. Our research design is not well suited to ask whether women are di erentially providing material versus emotional support; this distinction is also absent from recent research that nds more supportive behavior among women during high-stress episodes (e.g., Haller et al., 2022).

3.5 Conclusions

Deportation and killing of civilians was prevalent in Europe throughout the 20th century (Naimark, 2001), and continues throughout the world today. Investigating the social structure of internment camps is thus relevant not only as a study of history. We assess the importance of social networks in an important Holocaust setting. In absence of direct information on prisoner friendships, we employ social-linkage proxies based on various pre-existing networks.

Our analysis generates complementary evidence to, and a statistical check on the large part of the Holocaust literature based on fundamentally selective survival testimonies. It supports this literature in its emphasis on the importance of mutual-support groups as a key survival strategy of prisoners facing extreme survival pressure. Social ties corresponding to shared previous residence, earlier and current shared imprisonment, as well as pre-deportation ties all generated a signi cant survival advantage.

The evidence we provide extends the literature on the importance of social links in high-stakes contexts. While Costa and Kahn (2007) study the e ect of social bonds formed among soldiers in battle for their survival in a deadly POW camp, we study a demographically diverse civilan prisoner population (including women, for whom we nd particularly strong e ects). Further, we study the e ects of pre-war social ties as well as linkages formed within prisoner societies in underpinning survival in a deadly camp. Our ndings imply that a variety of social ties outside of the close bonds of family or `brothers in arms' support survival, and that life-supporting cooperation arises even when survival chances are extremely low.

Our evidence is also relevant to the literature studying parochial altruism| the notion that experience of violent con ict supports within-group cooperation among survivors (Trivers, 1971; Choi and Bowles, 2007). An alternative mechanism highlighted here is that those more prone to cooperation (having larger social networks) are more likely to survive violent con icts. Finally, our analysis contributes to the large literature on the importance of social networks for health outcomes (e.g., House et al., 1988) by providing evidence on the transferability of social linkages generated in normal social environments to the truly extreme conditions of deadly internment camps.

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A. Appendices

A.1 Proofs for Chapter 1

Proof of Equation (1.8). Assume for simplicity that $Y_{it}; U_i;$ and Z_i have zero means. OLS therefore estimates parameters in (1.7) as:

$$\begin{bmatrix} b_1 \\ b_2 \end{bmatrix} = \begin{bmatrix} var(Z) & cov(Z; U\;) \\ cov(Z; U\;) & var(U\;) \end{bmatrix}^{1} \begin{bmatrix} cov(Z; Y) \\ cov(U\;; Y) \end{bmatrix}: \tag{A.1}$$

Carrying out the matrix multiplication yields:

$$b_1 = \frac{cov(Z; Y)var(U\;) \quad cov(U\;; Y)cov(Z; U\;)}{var(U\;)var(Z) \quad cov^2(Z; U\;)}: \tag{A.2}$$

Under Assumptions 1 amd 2, the requisite variance and covariances can be expressed in terms of the structural parameters as:

$$cov(Z; Y) = (\quad + \quad)var(Z) \tag{A.3}$$

$$cov(Z; U\;) = \quad {}_1 var(Z) \tag{A.4}$$

$$cov(U\;; Y) = \quad {}_1[var(\;) + \quad (\quad + \quad)var(Z)] \tag{A.5}$$

$$var(U\;) = \quad {}_1^2 \;{}^2 var(Z) + \quad {}_1^2 var(\;) + var(\;): \tag{A.6}$$

Plugging (A.3){(A.6) into (A.2) yields:

$$b_1 = \frac{var(Z)[(\quad + \quad)var(\;) + \quad {}_1^2 var(\;)]}{var(Z)[var(\;) + \quad {}_1^2 var(\;)]}: \tag{A.7}$$

Cancelling out var(Z) delivers Equation (1.8). □

An alternative derivation via the formula for omitted variable bias can be found in Oster (2016), and Pei et al. (2018). Note that when there is no measurement error (i.e. var( ) = 0), then $b_1 =$ since in that case U_i properly controls for the omitted variable irrespective of the scaling factor ${}_1$. At the other extreme, when the proxy carries no information about the true latent confounder (i.e. ${}_1 = 0$), then no omitted variable bias is eliminated and $b_1 = b_1 = (\quad + \quad)$.

Proof of Proposition (1). Recall that regression (1.4) produces $b_1 = \; + \;$ and the augmented model (1.7) yields b_1 given in (1.8). Therefore:

$$b_1 \quad b_1 = \frac{{}^2_1\mathrm{var}(\;)}{{}^2_1\mathrm{var}(\;) + \mathrm{var}(\;)}: \tag{A.8}$$

Since (A.8) eliminates , what remains is a system of three simultaneous equations (1.10), (1.11), and (A.8) with three unknowns ; $_1$, and var ( ). The solution for is given in Proposition 1 after replacing var () with its estimate s^2. □

Proof of Equation (1.25). Note that $U_i = \; Z_i + \; _i$ as above. Hence the correlation between Z_i and U_i is:

$$\mathrm{corr}(U; Z) = \frac{\mathrm{cov}(U; Z)}{\sqrt{\mathrm{var}(U)\mathrm{var}(Z)}} = \frac{\mathrm{var}(Z)}{\sqrt{{}^2\mathrm{var}^2(Z) + \mathrm{var}(\;)\mathrm{var}(Z)}}; \tag{A.9}$$

where the second equality holds due to assumed uncorrelatedness between Z_i and $_i$. Cancelling out $\sqrt{\mathrm{var}(Z)}$ yields the required result. □

A.2 Details of the empirical application in Chapter 1

We estimate a simple gravity model of international trade between OECD countries for the period 2005{2014. The parameter of interest b_1, is the elasticity of international trade ows with respect to the distance between the trading partners. Since distance elasticity is a TIR coe cient, this model is a suitable illustration for the procedure proposed here. To obtain b_1, an index of institutional similarity is used as a proxy for the latent cross-country di erences (see e.g. Guiso et al., 2009; Mohlmann et al., 2010, for gravity models augmented by similar proxy variables). After obtaining b_1 from the baseline gravity equation, and b_1 from a model augmented by the index of institutional similarity, (1.14) is used to reconstruct the true distance elasticity.

A.2.1 Data and empirical specification

Trade flows of goods were extracted from the STAN database (OECD, 2018). A dataset by Gurevich and Herman (2018) was used as a source of contextual variables. Following Möhlmann et al. (2010), the proxy for the unobserved cross-country differences was constructed as a Kogut and Singh (1988) index of the Worldwide Governance Indicators (World Bank, 2017).

The model was specified as:

$$
\begin{aligned}
\ln(\text{Trade flows})_{ijt} = b_0 &+ b_1 \ln(\text{Distance})_{ij} + a_1 \ln(\text{GDP})_{it} + a_2 \ln(\text{GDP})_{jt} + \\
&+ a_3 \ln(\text{Population})_{it} + a_4 \ln(\text{Population})_{jt} + \\
&+ a_5 \overline{\ln(\text{GDP})}_i + a_6 \overline{\ln(\text{GDP})}_j + a_7 \overline{\ln(\text{Population})}_i \\
&+ a_8 \overline{\ln(\text{Population})}_j + \sum_{s=2006}^{2014} a_s 1[t = s] + e_{ijt} : \qquad \text{(A.10)}
\end{aligned}
$$

Subscripts i, j, and t index exporter, importer, and time respectively. The overline specifies the mean within a country dyad. Following Mundlak (1978), we assume that $Y_{ijt} = U_{ij} + Z_{ij} \; + X_{ijt} \; + \varepsilon_{ijt}$ and $U_{ij} = Z_{ij} \; + \overline{X}_{ij} \; + \; _{ij}$, which is why within means were included in the specification. Due to collinearity, within means of year dummies were omitted. In addition, a second regression was fitted where (A.10) was augmented by the inclusion of the proxy variable described above. The third regession projected the proxy on ln(Distance) and the within means. To measure sensitivity of b_1 to endogeneity by (1.25), we obtained the conditional variance of ln(Distance) by regressing it on within means and isolating the residuals. Variance of the random effects, var( ), was calculated by the Swamy and Arora (1972) method since allowing autocorrelated idiosyncratic disturbances has small effect on the resulting estimate. After estimating these four models, the results were plugged into (1.14) to calculate . Standard errors were obtained by bootstrapping with 1000 replications clustered by the country dyads.

In addition, the system of four regressions was fitted in a single MLE model which avoids the need to bootstrap, but instead it assumes that the random effects are normally distributed. Nevertheless, both point estimates and standard errors match closely between the MLE and bootstrapped OLS.

A.2.2 Results

Table A.1 reports the key parameters from both bootstrapped OLS regressions and a joint maximum-likelihood estimation (MLE).

Table A.1: Estimated distance elasticity of international trade ows.

		Bootstrap OLS		MLE	
Param.	Description	Est.	SE	Est.	SE
b_1	Distance elasticity (Eq. 1.4)	-1.055	0.035	-1.058	0.034
b_1	Distance elasticity (Eq. 1.7)	-1.040	0.035	-1.044	0.033
	Bias in b_1	-0.501	0.246	-0.533	0.194
b_1	Distance elasticity sans bias	-0.554	0.246	-0.525	0.187
$^{0}_{U;Z}$	corr $(U;Z)$ if $b_1 =$	-0.826	0.012	-0.828	0.014

Note: Standard errors are clustered by country dyads.

The model estimates trade elasticity at roughly negative unity, which is well in line with the published literature (see Disdier and Head, 2008, for a survey). While adding the proxy variable leads to an almost imperceptible change in the estimated elasticity, once (1.14) is used to account for the measurement error, the estimated   becomes much more pronounced (albeit rather imprecisely estimated). Hence b_1 b_1 on its own severely understates the omitted variable bias. The large negative bias in the estimated distance elasticity is also consistent with the empirical literature (Blum and Goldfarb, 2006; Head and Mayer, 2013; Lendle et al., 2016). However, it is unlikely that the entirety of the estimated parameter is attributable to omitted variable bias, as $b_1 =$ implies a correlation between distance and the latent confounder ($^{0}_{U;Z}$) of more than {0.8. Thus, the unobserved variable would have to be a \clone" of distance, which is scarcely plausible.

A.3 Further data characteristics for Chapter 2

The full dataset records trade in ows into the Russian Federation from the following countries and territories: Afghanistan, Albania, Algeria, Andorra, Anguilla, Argentina, Armenia, Australia, Austria, Azerbaijan, Bangladesh, Belarus, Belgium-Luxembourg, Benin, Bolivia, Bosnia Herzegovina, Br. Virgin Islands, Brazil, Brunei, Bulgaria, Burkina Faso, Cameroon, Canada, Chile, China, China Hong Kong SAR, Colombia, Congo, Costa Rica, Croatia, Cuba, Curacao, Cyprus, Czech Rep., Cote d'Ivoire, Dem. Peoples Rep. of Korea, Dem. Rep. of the Congo, Denmark, Dominican Rep., Ecuador, Egypt, El Salvador, Estonia, Ethiopia, FS Micronesia, Falkland Islands Malvinas, Fiji, Finland, France, Georgia, Germany, Ghana, Greece, Greenland, Guatemala, Guinea, Guinea-Bissau, Honduras, Hungary, Iceland, India, Indonesia, Iran, Iraq, Ireland, Israel, Italy, Japan, Jordan, Kazakhstan, Kenya, Kuwait, Kyrgyzstan, Laos, Latvia, Lebanon, Libya, Lithuania, Macedonia, Madagascar, Malawi, Malaysia, Maldives, Mali, Mauritania, Mauritius, Mexico, Mongolia, Montenegro, Morocco, Mozambique, Myanmar, Netherlands, New Zealand, Nicaragua, Niger, Nigeria, Norway, Oman, Pakistan, Panama, Paraguay, Peru, Philippines, Poland, Portugal, Rep. of Korea, Rep. of Moldova, Romania, Rwanda, Saint Helena, San Marino, Saudi Arabia, Senegal, Serbia, Seychelles, Singapore, Slovakia, Slovenia, So. African Customs Union, Spain, Sri Lanka, State of Palestine, Sweden, Switzerland, Syria, Tajikistan, Thailand, Trinidad and Tobago, Tunisia, Turkey, Turkmenistan, USA, Uganda, Ukraine, United Arab Emirates, United Kingdom, United Rep. of Tanzania, Uruguay, Uzbekistan, Venezuela, Viet Nam, Yemen, and Zimbabwe.

For our main speci cation, we code the following countries as \sanctioning exporters:" Austria, Belgium-Luxembourg, Bulgaria, Croatia, Cyprus, Czech Rep., Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Latvia, Lithuania, Netherlands, Poland, Portugal, Romania, Slovakia, Slovenia, Spain, Sweden, USA, and United Kingdom.

The remaining exporters under the sanctioning regime (excluded in the main speci cation, but included in Robustness checks): Albania, Australia, Canada, Iceland, Montenegro, Norway, Switzerland, and Ukraine.

The following two tables provide summary statistics for individual product categories disaggregated at 4-digit HS codes. For clarity, these codes are accompanied by brief descriptions of the relevant product category.

Table A.2: Summary statistics for annual trade ows into Russia of extraction equipment across di erent exporters (in thousands of USD)

HS4	Description	Mean	SD	N
7304	Tubes, pipes and hollow pro les, of iron/steel	2307	11930.8	5683
7305	Other tubes with diam.>406.4mm (iron/steel)	6067.9	35253	1236
7306	Other tubes of iron or steel	1054.4	4580.7	3585
8207	Interchangeable tools for hand tools	893.3	3792.7	6583
8413	Pumps for liquids; liquid elevators	2228.3	10129.2	9015
8430	Other moving, extracting or boring machinery	3378.8	17115.7	3405
8431	Parts for the machinery in 8425 to 8430	2461.2	9910.9	6097
8705	Other tubes and pipes	4398.8	13937	1523
8905	Light-vessels, re- oats, oating cranes etc.	20841.4	126221.3	273

Table A.3: Summary statistics for annual trade ows into Russia of foodstu s across di erent exporters (thousands of USD)

HS4	Description	Mean	SD	N
0201	Meat of bovine animals, fresh or chilled	8141.2	36776.1	512
0202	Meat of bovine animals, frozen	33433.8	125367.6	850
0203	Meat of swine, fresh, chilled or frozen	19160.8	63320.1	1223
0207	Poultry meat and o al, chilled or frozen	13910.4	64058	1165
0210	Meat and edible meat o al, salted, or smoked	433.9	1090.2	643
0301	Live sh	266	934.5	432
0302	Fish, fresh or chilled	5802.2	42593.8	981
0303	Fish, frozen, excluding sh llets	3639.2	14236.2	2972
0304	Fish llets, fresh, chilled or frozen	3023.8	11357.6	1192
0305	Fish, dried, smoked, salted or in brine	1355.1	6311.1	766
0306	Crustaceans	2568.1	10074.3	1089
0307	Molluscs	648.3	2560.1	1287
0401	Milk and cream, not concentrated	3601.3	18517	547

Table A.3 continued from previous page

0402	Milk and cream, concentrated	6848.3	28901.5	965
0403	Buttermilk, curdled milk and cream etc.	2684.7	10284.6	579
0404	Whey	2023.1	6988.8	531
0405	Butter and other fats and oils derived from milk	15293.6	40089.7	513
0406	Cheese and curd	10533.5	44747.4	1965
0701	Potatoes, fresh or chilled	3701.5	12297.5	762
0702	Tomatoes, fresh or chilled	10841.9	34915.4	765
0703	Onions, shallots, garlic etc.	2160.2	7122.8	1295
0704	Cabbages, cauli owers, etc.	1255.5	3453.9	1024
0705	Lettuce and chicory	497.7	1752.4	726
0706	Carrots, turnips, salad beetroot etc.	1542.4	5355.2	978
0707	Cucumbers and gherkins, fresh or chilled	3929.6	10524.6	496
0708	Leguminous vegetables	41	93.6	417
0709	Other vegetables, fresh or chilled	1584.7	6581	2517
0710	Vegetables, frozen	783.9	2659.2	1631
0711	Vegetables, not for immediate consumption	341.5	1439.8	315
0712	Dried vegetables	532.7	2155.4	1086
0713	Dried leguminous vegetables	257.1	893.4	1524
0714	Roots and tubers with high starch content	23	75.2	193
0801	Coconuts, Brazil nuts and cashew nuts	1212	5465.8	772
0802	Other nuts, fresh or dried	1805.2	8760.4	1681
0803	Bananas, including plantains, fresh or dried	24496.6	96360.4	325
0804	Dates, gs, pineapples etc.	741.6	2932.4	1999
0805	Citrus fruit, fresh or dried	5453.4	18332	2617
0806	Grapes, fresh or dried	5014.3	15483.1	1211
0807	Melons and papaws, fresh	1208.6	5979.6	853
0808	Apples, pears and quinces, fresh	7622.3	21080.8	1602
0809	Apricots, cherries, peaches etc, fresh	2034.3	7075.3	2514
0810	Other fruit, fresh	2266.6	7639	1999
0811	Fruit and nuts, frozen	673.1	2048	1118

Table A.3 continued from previous page

0813	Fruit, dried, including mixtures	859.3	3592.6	1967
1601	Sausages and similar products	5380	16648.6	503
1901	Malt extract	2426.6	7731.3	1859
2106	Food preparations not elsewhere specified	4996.5	15612.3	1950

A.4 Autoregressive model for estimating lost trade in Chapter 2

Differences-in-differences models rely on the assumption of parallel trends, which implies that the correct counterfactual for the treated group after the intervention is the outcome in the control group. Although the data here seem consistent with the parallel trend assumption, we use a different method of creating counterfactuals here to check the baseline results.

To that end, we construct counterfactuals for the treated outcome using information only for the trade flows within each exporter-good pair. This approach is similar to event studies: we estimate an auto-regressive model for the time series of the trade flows prior to the imposition of sanctions and generate predictions from this model for the period after sanctions. Deviations between the observed trade flows and the predicted ones can serve as alternative estimates of the effect of the intervention. The empirical specification is:

$$Y_{ij;t} = \alpha_{i;0} + \beta_{j;1} + \beta_{j;2} Y_{ij;t-1} + \beta_{j;3} t + \beta_{j;4} (t \times Y_{ij;t-1}) + \varepsilon_{ijt} \tag{A.11}$$

where $Y_{ij;t}$ are imports into Russia of goods specified at the 6-digit HS code j from exporter i in year t. Apart from exporter-specific fixed effects ($\alpha_{i;0}$), coefficients are allowed to vary by the product HS classification.

We estimate (A.11) on the sample from 2010 to 2013 and generate one-step ahead predictions and their corresponding standard errors. Despite its parsimony, the model shows an impressive fit both in-sample as well as out-of-sample. The in-sample R^2 for extraction equipment is 0.78 and out-of sample R^2 is 0.69 for the period 2014–2016. For the foodstuffs, the in-sample R^2 is 0.93 and out-of sample R^2 for the sanctioning period is 0.56[1].

The predicted values are then used to compare the true levels of inflows with the predicted values. In order to test the predictive performance formally, we compare the distributions of the deviations between the true and predicted values. The equality of these distributions was tested by the Kolmogorov-Smirnov test (KS). The null hypothesis in this test is that the prediction errors in treated and control groups are drawn from identical distributions. To complement the KS test, we also regress the observed values on the predicted ones:

$$\begin{aligned}\text{observed values}_{ijt} = \ & \beta_0 + \beta_1 \text{predictions}_{ijt} + \beta_2 \text{sanctions}_{ij} + \\ & + \beta_3 \text{predictions}_{ijt} \times \text{sanctions}_{ij} + \varepsilon_{ijt}. \end{aligned} \qquad \text{(A.12)}$$

After estimating regression (A.12), we test the null hypothesis that $\beta_2 = \beta_3 = 0$. Rejection of this null hypothesis indicates a difference in the predictive power of the model between the sanctioning and control regimes. This test follows the procedure for evaluating forecasts advocated by Mincer and Zarnowitz (1969), and hence we abbreviate it as an MZ test[2].

Table A.4 shows that for the extraction equipment, the model is equally successful at predicting inflows into Russia whether there are sanctions in place or not. The only statistically significant difference is detected by the MZ test in 2014 (at 5% level) but the KS test fails to detect a difference. This outcome could be consistent with either a small effect or a potential Type I error. In contrast, sanctions against foodstuffs are reflected in the model's predictive power, which can be seen on the test results as well as on the more negative means of the prediction errors in the sanctioning group. Both of these results are consistent with the differences-in-differences model indicating that even if the parallel assumption

[1] Reported values are "within" R^2, i.e. they represent fit within each product category.

[2] We prefer this test over a test of structural break as the latter tests a over-restrictive null hypothesis. Null hypothesis for test of structural break is the same as above ($\beta_2 = \beta_3 = 0$) with two additional restrictions: $\beta_0 = 0$ and $\beta_1 = 1$. Intuitively, test of structural break tests whether forecasts are unbiased for both treatment and control group, while we only test whether the forecasts are "equally good" between the two groups.

is relaxed, the conclusion remains unchanged.

It is also noteworthy that this AR model broadly agrees with our main speci - cation in terms of the estimate for the lost trade. Western sanctions led to about 0.8 billion USD (bootrstrap SE = 0.5) of lost trade in extraction equipment under this model while lost trade in foodstu s is estimated at 5.1 billion USD (bootrstrap SE = 1.7). Even though the point estimates are lower than in the di erences-in-di erences speci cation, the conclusion that Russian sanctions were much more costly remains unchanged.

Table A.4: Comparison of deviations between observed and predicted values of trade ows in sanctioned goods (in millions USD).

Exporters:		Control		Sanctioning		Test (p-values)	
	Year	Mean	SD	Mean	SD	MZ	KS
Extraction eq.	2014	0.45	11.41	-1.94	6.36	0.37	0.02
	2015	-1.12	10.82	-2.22	8.93	0.74	0.12
	2016	0.00	8.98	-0.50	10.60	0.69	0.97
Foodstu s	2014	0.26	27.78	-2.08	15.56	< 0.01	< 0.01
	2015	-2.49	49.23	-6.10	20.68	< 0.01	< 0.01
	2016	-0.43	50.52	-0.43	6.88	< 0.01	< 0.01

Notes: Table shows means and standard deviations (SD) of the deviations between observed and predicted values of trade ows of sanctioned goods into Russia.
The last two columns depict the test of the null hypothesis that predictive performance is the same for control and sanctioning exporters. MZ stands for the procedure of Mincer and Zarnowitz (1969), while KS is a standard Kolmogorov-Smirnov test of the equality of distributions. Rejection of the null hypothesis in both tests would indicate a di erence in the predictive power of the model between the sanctioning and control regimes. At the 1% level, we do not reject the di erence between sanctioning and control regimes for extraction equipment, while we found strong evidence of di erences for the food products.

A.5 Data sources and historical information for Chapter 3

A.5.1 Archival Sources on Social Linkages

Social Linkages - Family ties

Transport lists available in the Theresienstadt Initiative Institute (TII) database[3] provide names and transport numbers (IDs) for each individual on a transport to and from Theresienstadt. Family members typically came to Theresienstadt on the same transport with consecutive transport IDs. We therefore proxy family linkages based on sharing the same surname and holding consecutive transport numbers, and estimate that 82,000 prisoners arrived with family members, making up about 28,000 (mostly Czech) families. The TII also collected direct information on family linkages for over 4,000 prisoners. Our approximation based on transport numbers captures over two thirds of these linkages. Among the 4,000 prisoners, only 5% of the family ties we approximate based on transport numbers are contradicted by the direct measure of family ties.

Social Linkages - Lpa camp

The male Czech Jews interned at the Lpa camp were engaged in agricultural labor. The camp was guarded by only one or two members of the SS, it was not deadly and the atmosphere was conducive to friendship formation (Jindrova, 2009). After the daily agricultural work, prisoners (whose average age was 26) organized their own free time, played chess tournaments and shared books. Stransky and Ullmann (1990, p.15) report that Lpa prisoners formed small `communes' where they shared food (sent by mail from home), etc., and that these `communes' later on helped their members survive the Holocaust. We merge the TII database (based on name, age, and place of residence) with the complete list of Lpa camp prisoners compiled by Jindrova (2009). Out of the 1,351 prisoners of the Lpa camp, 961 entered Theresienstadt. Of these, the median length of time they spent in the Lpa camp was about half a year, which allows for strong social links to be built.[4]

[3] The database is searchable online at https://www.holocaust.cz/databaze-obeti

[4] While many Lpa prisoners came directly to Theresienstadt, i.e., those on transports AE5

Ultimately, 842 of the 961 Lpa prisoners in Theresienstadt ended up in transports to the East; 601 were sent to Auschwitz in 23 separate transports.

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and Dn coming directly from Lpa, most Lpa prisoners were rst released and only later deported to Theresienstadt.

Social Linkages - IKG/JKG

Self-governing bodies of the Jewish communities in Prague, Vienna, and Berlin, were misused during the Holocaust by Nazi Germany as administrative bodies supporting the extermination of Jewish populations. The o cials of these organizations set up deportation lists based on Nazi instructions, and also organized social help for those in need, as well as educational and sport activities of pre-deportation local Jewish societies. The list of JKG Prague members and managers was digitized from Krejcova et al. (1997) and merged with the TII data. The Berlin and Vienna lists were obtained from archives (by T. Fedorovic). We then digitized these and merged them with the TII database based on TII data assistance. Sources:

- IKG Vienna: Personalkartei der MitarbeiterInnen der IKG Wien (1925-1945), Archiv der Israelitischen Kultusgemeinde Wien[5]
- JKG Berlin + Reichvereinigung der Juden in Deutschland: CJA, 2 B 1, Nr. 6, Mitarbeiterverzeichnis der Reichsvereinigung der Juden in Deutschland, 1. 9. 1941. Das Museum der Neuen Synagoge Berlin { Centrum Judaicum, https://centrumjudaicum.de/

Social Linkages - SNAP

An underground satiric weekly (Shalom for Friday, Salom na patek in Czech, SNAP) was shared by Theresienstadt prisoners forming a chain-mail community. Source: Yad Vashem Archives O.64/64.

Academic Titles

In addition to measures of social ties, we also condition on the information on academic titles available in the TII database. In total, three per cent of the prisoners in Theresienstadt held an academic title, including about 1 thousand doctors of medicine. Most of the 5 thousand prisoners with an academic title held the generic \Dr." title in the data.

[5] http://www.archiv-ikg-wien.at/

A.5.2 Theresienstadt Out-Transport Selection

The Theresienstadt ghetto's self-administration consisted of its Council of the Elders and of departments that administered various aspects of life in the ghetto (kitchens, youth welfare, etc.). A key function of the self-administration was to assemble transports out of the ghetto based on the SS demographic-group-level orders corresponding to age and/or nationality[6]. The Council compiled lists of prisoners suggested for transports that it requested from departments within the self-administration, and in some cases it combined large out-transport groups from specific in-transport groups (Hájková, 2020). Its stated objective was to spread the 'burden of transports' evenly across the various groups present in the ghetto. We know of no historical evidence suggesting that the transport selection process would aim to optimize the social-network composition of the out-transports, which typically consisted of one to two thousand prisoners and were organized under significant time pressure, with often only two to three days to assemble the transport after receiving the SS order. Furthermore, the ghetto's prisoner registry did not contain information on social networks, such as Lpa-camp linkages, limiting the feasibility of reflecting such linkages. The influence of the self-administration over the individual-level composition of transports (within demographic groups) was terminated in the fall of 1944 (with transport 'En'), when the SS started selecting individual prisoners for transports.

A.5.3 Survivor Testimonies on Social Linkages

Family ties

Jiří Franěk (born 1922) recalls help from his aunt in Auschwitz. “When my beautiful little cousin left for the gas chamber, her mother, my aunt, started taking care of me, and every day she brought me an extra portion of soup scraped from the bottom of the barrel.” Jiří Franěk. 1994. Like sheep to the slaughter. [Jako ovce na porážku], in *The Theresienstadt family camp in Auschwitz-Birkenau*

[6]For many transports, the SS decided that they be composed of only one nationality/age group.

[Tereznsky rodinny tabor v Osvetimi-Birkenau], The Foundation of the Theresienstadt Initiative: Prague, p. 83.

Lpa-camp ties

\...their camaraderie led to the formation of small `communes' where they shared everything, from food received by mail to labor in the eld. There were also those who kept to themselves, never helped anybody, never shared. Their fate was not good later on, when conditions got worse." Testimony by prisoners of the Lpa camp who eventually ended up in the Auschwitz-Birkenau concentration camp. O. Stransky and O. Ullmann. 1990. Lpa 1940-1945, Prague, p. 15.

Administrative JKG/IKG ties

Lea Rachman recalls arriving in the Lodz ghetto with her father, who was formerly the chief editor of the newspaper of the JKG in Prague: \When we came to Lodz, we were contacted by local members of the Jewish leadership JKG/IKG, who took care of dad. Soon we got assigned a at in the ghetto that was larger than we needed." Richard Seemann. 2000. Ghetto Litzmannstadt 1941-1944, Institute of International Relations: Prague, p.27 and p.74.

Mutual-support groups { mechanisms

\In the extreme conditions of the camps, inter-personal relationships were critical . . . linkages from the past: family or local ties." \Mutual solidarity of prisoners in Auschwitz was not unusual, including sharing food..." Peter Salner. 1997. They Survived the Holocaust. [Prezili Holokaust], Slovak Academy of Sciences: Bratislava, p. 146 and p. 150.

\In these di cult times, friendship saved lives. Many times, it helped me to have someone to talk to and share my troubles." A young female Theresienstadt prisoner writes how she became a close friend with another female prisoner arriving on the same transport from Theresienstadt to Auschwitz. Hana Sterlicht, Alzbeta Langova. 2021. Hana's road: from Holice to the Holy Land [Hanicina cesta. Z Holic az do Svate zeme], pp.54{60.

\Without such friendship, life here would be impossible." A female Theresienstadt prisoner describes in her memoir how she survived the initial Auschwitz selection together with two other women of the same rst name, who she knew from Theresienstadt and based on pre-deportation linkages. During the initial selection, the three women (\drei Grete") shared food and promised each other to stay together, and indeed later helped each other during periods of illness, with nding clothing, etc. Margarete Jansky. 1990.Such was my life in good and bad times [So war mein Leben in schonen und bosen Zeiten], pp. 38 { 43.

\Boys aged 14-16 in the family camp in Auschwitz who survived recalled the importance of mutual help, including risking one's life to save that of a friend, which helped them survive the harsh months until liberation." Ruth Bondyova. 1994. The children's block in the family camp in Auschwitz. [Detsky blok v rodinnem tabore v Osvetimi], in The Theresienstadt family camp in Auschwitz-Birkenau [Tereznsky rodinny tabor v Osvetimi-Birkenau], The Foundation of the Theresienstadt Initiative: Prague, p. 60.

\That's when the system of designated couples (communes) was useful; when I found some food, I shared it with Rocko and so did he.\ Toman Brod, who was under 14 when he entered the family camp in 1943 and who ended up in the adult male section of Auschwitz in 1944 together with 90 other boys from the family camp, describes how the group formed designated pairs where the two boys would consistently help each other. Toman Brod. 2007It's good one does not know what lies ahead: My life between 1929 and 1989, [Jeste ze clovek nev, co ho ceka. Zivota beh mezi roky 1929 a 1989], pp. 150 { 185.

\40 boys (14 to 16 years old) from Room 7 in Building L417 in Theresienstadt proudly called themselves the Nesarim (eagles). Living together under the tutelage of their youth leader Franta had an unusual impact on everyone's lives: the creation of an extended family of brothers." Jan Strebinger testimony: \One of the many things that Franta taught us was to depend on each other, and that contributed to Robin's and my survival in the various camps we went through.\ Thelma Gruenbaum, 2004.Nesarim: Child Survivors of Terezn, London { Portland, p. 1-2 and p. 194.

The `muselmann' phenomenon

Muselmann was a slang term used by prisoners of Nazi concentration camps to refer to those prisoners who were resigned and passive, a ected by apathy and inertia, and likely to perish soon: \ . . the divine spark dead within them, already too empty to really su er" Primo Levi. 1947. If This Is a Man [Se questo e un uomo]; \ . . a being from whom humiliation, horror and fear had so taken away all consciousness and personality as to make him absolutely apathetic", \ . . mute and absolutely alone ... without memory and without grief. . . " Giorgio Agamben. 1998. Remnants of Auschwitz: The Witness and the Archive. For the notion that a positive attitude made one better equipped for surviving the camps, see also Theresienstadt and Auschwitz survivor testimony in Frankl (1946).

A.6 Statistical methods in Chapter 3

Alongside simple OLS and probit models, the main text estimates probit models with sample selection (Van de Ven and Van Praag, 1981) speci ed as:

$$P[Y_1 = 1 | Y_2 = 1; \quad] = \frac{(X_1 \quad _1; X_2 \quad _2; \quad)}{(\ X_2 \quad _2)}; \tag{A.13}$$

where Y_1 is the dependent variable (survival after entering The Auschwitz concentration camp); Y_2 is an indicator equal to one if a prisoner was selected for an out-transport from the available Theresienstadt population; is the conditioning set consisting of the matrix of observed regressors X; (; ;) is bivariate standard normal cumulative distribution function with a correlation coe cient ; ( ) is a univariate standard normal cumulative distribution function; nally, $_1$ and $_2$ are vectors of parameters to be estimated alongside . As an alternative to (A.13), we estimated $_1$ and $_2$ by OLS and controlled for deciles of the estimated probability of selection (i.e. $X_2 \, \hat{}_2$) in the survival equation.

Since the probit parameters are di cult to interpret in terms of their magnitude, we compute *average marginal e ects* (AMEs). The AME for a continuous variable x evaluated at a given level of other regressors z is:

$$\widehat{AME}(x | z = \bar{z}) = \frac{1}{N} \sum_{i=1}^{N} \left. \frac{\partial f(X_i \, \hat{})}{\partial x_i} \right|_{z_i = \bar{z}}; \tag{A.14a}$$

where i indexes observations in the sample from 1 to N, and where X_i is the i-th row of the regressor matrix X, which collectively denotes matrices X_1 and X_2; and $f(\cdot)$ is the function describing the expected probability of survival. For discrete x, AME measures the difference between the function f evaluated at two values of x, say $x^{(1)}$ and the baseline value $x^{(0)}$:

$$\widehat{AME}(x|z = \bar{z}) = \frac{1}{N}\sum_{i=1}^{N}\left[f(X_i\beta)\Big|_{z_i=\bar{z};\,x_i=x^{(1)}} - f(X_i\beta)\Big|_{z_i=\bar{z};\,x_i=x^{(0)}}\right]. \qquad \text{(A.14b)}$$

In other words, $\widehat{AME}(x|z = \bar{z})$ is calculated in three steps: (i) the value of z is set in the entire sample to $\bar{z}$, leaving other elements of X unchanged; (ii) the partial derivative or the discrete difference of $f(x_j\beta)$ with respect to x is calculated at each observation of the modified sample; and (iii) partial derivatives or discrete differences computed in step (ii) are averaged across the sample.

AMEs for the number of prisoners with social linkages on transports to Auschwitz indicate the marginal effect of a single additional fellow prisoner on transport. Since the maximum size of the groups of potentially linked prisoners differs, the expected survival advantage is difficult to compare between different social groups. For this reason, we alternatively evaluate the expected survival advantage of a measure of social linkages x as follows:

$$\text{Survival advantage due to } x = \frac{1}{N}\sum_{i=1}^{N}\left[f(X_i\beta)\Big|_{x_i=\bar{x}+\frac{sd(x)}{2}} - f(X_i\beta)\Big|_{x_i=\bar{x}-\frac{sd(x)}{2}}\right], \qquad \text{(A.15)}$$

where $\bar{x}$ and $sd(x)$ are the sample mean and standard deviation of x, respectively, for observations where $x > 0$. Thus, Equation (A.15) measures the change in the expected survival probability when the number of socially-linked prisoners rises by one standard deviation around its sample mean (taken at the level of individual prisoners).

A.7 Supplementary Results for Chapter 3

A.7.1 Additional summary statistics

Table A.5: Summary statistics for variables in the Survival Model in Auschwitz. Standard deviations (SD) for binary variables omitted.

Variable	Females		Males	
	Mean	SD	Mean	SD
N Family	0.197	0.469	0.404	0.665
N in-transport	5.890	13.472	22.950	52.219
N SNAP	0.005	0.136	0.041	0.888
N Same street	0.084	0.321	0.160	0.503
N Lipa	|	|	0.939	7.184
N JKG Prague	0.278	3.082	5.200	25.513
N IKG Vienna	0.027	0.626	0.501	5.151
N IKG Berlin	0.003	0.081	0.119	1.913
Age in 1940	44.311	18.399	38.141	17.768
Selection risk evaded	0.657	0.785	0.680	0.775
Ac. title (non-medical)	0.003	|	0.048	|
Doctor (medical degrees)	0.002	|	0.024	|
JKG Prague	0.027	|	0.091	|
IKG Vienna	0.004	|	0.015	|
IKG Berlin	0.001	|	0.005	|
Arr. with family	0.669	|	0.687	|
SNAP reader	0.001	|	0.003	|
Austrian origin	0.097	|	0.071	|
German origin	0.196	|	0.119	|
Czech origin	0.667	|	0.728	|
Observations		16,200		14,546

Figure A.1: Prisoners' social linkages by out-transports to Auschwitz

Notes: The graph plots transport-wide survival rates (in percentage points) for transports from Theresienstadt to Auschwitz where selection of prisoners was a ected by the ghetto self-administration, sorted by date of departure, with transport means of social-linkage variables that display within-transport variation (the Number of prisoners with ties to each other on transport, averages taken for prisoners with non-zero values). Wild-bootstrap p-values for correlations between survival and social linkages are as follows: N same in-transport= 0.26; N same street = 0.08; N family = 0.27.

A.7.2 Age and nationality e ects

Figure A.2: Predicted survival probability as a function of prisoner's age and nationality, by gender

(a) Age

(b) Nationality

Notes: Estimated probabilities from the Probit model in columns (1) and (5) of Table 1 in main text. These age and nationality e ects are conditional on transport-wide survival di erences, time spent in Theresienstadt, and prisoner characteristics including all of our social networking controls.

A.7.3 A measure of unobserved social capital

In an effort to explore the sensitivity of our baseline estimates to selection into transports to Auschwitz based on unobserved social capital of prisoners, we also estimate our Auschwitz-survival specifications with an additional control summarizing prisoners' ability to evade selection for transports out of Theresienstadt before ultimately being selected for a transport to Auschwitz. For transports out of Theresienstadt, the SS determined which demographic groups (by age, nationality, and gender) would be included in the next transport. In a typical selection episode, 6% of prisoners of a given demographic group was selected; at a given selection episode, a typical (average) prisoner exposed to selection had already evaded 10 transport selections. Perhaps those who did particularly well on this measure (before being selected for a transport to Auschwitz) have unusually strong (unobservable) social ties that may not have been captured by our selection models estimated in the main text. We add up the demographic-group-specific selection risks for each prisoner across all selection episodes successfully avoided. The average value of the accumulated avoided selection probability for Theresienstadt prisoners entering Auschwitz is 0.66 (66%) for women and 0.68 (68%) for men.

Specifically, we first compute the selection risk faced by prisoners in a given demographic category:

$$\widehat{SR}_{c;t} = \frac{1}{N_{c;t}} \sum_{i=1}^{N_{c;t}} \text{Selected}_{i;t}; \tag{A.16}$$

where a given demographic category c is defined by prisoners' gender, nationality and 10-year age bracket (with a single bracket for prisoners aged 70 and above); $N_{c;t}$ is the total number of prisoners belonging to category c who are present in Theresienstadt when out-transport t departs; and $\text{Selected}_{i;c;t}$ is a binary indicator equal to one if prisoner i, belonging to category c is selected into out-transport t and zero otherwise. Therefore, $\widehat{SR}_{c;t}$ is the mean probability of selection on transport t for the demographic category c. In the second step, we compute the cumulative selection risk, which each prisoner has managed to evade prior to a given transport:

$$\text{Selection risk evaded}_{i,c,\tau} = \sum_{t=1}^{\tau-1} \widehat{SR}_{c,t}. \tag{A.17}$$

Equation (A.17) thus computes the accumulated selection risk faced by prisoner i who belongs to a demographic category c, not including the transport on which prisoner i departed (transport τ).

Table A.6 reports the results for models augmented by this measure of unobserved heterogeneity. While this additional control ('Transport-selection risk evaded') predicts survival after entering Auschwitz, its inclusion in the estimated specifications has no material effect on our coefficients of interest.

Table A.6: AMEs from survival models including a measure of unobserved social capital

	Males				Females			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Probit	OLS	Probit	OLS	Probit	OLS	Probit	OLS
N Lipa	0.00101**	0.00327***	0.000614**	0.00322***	—	—	—	—
	(0.000422)	(0.000326)	(0.000275)	(0.000339)				
N Family	0.00245	0.00667	0.00154	0.00608	0.00940**	0.0348**	0.00524*	0.0373**
	(0.00442)	(0.00698)	(0.00267)	(0.00700)	(0.00478)	(0.0140)	(0.00308)	(0.0132)
N Same street	0.0160***	0.0331***	0.00985***	0.0326***	0.0235***	0.0978***	0.0140***	0.0963***
	(0.00582)	(0.00919)	(0.00322)	(0.00892)	(0.00338)	(0.00889)	(0.00474)	(0.00821)
N SNAP	0.00171**	0.00229	0.00106**	0.00210	0.00975***	0.0713***	0.00607***	0.0691***
	(0.000872)	(0.00160)	(0.000505)	(0.00155)	(0.00268)	(0.0103)	(0.00233)	(0.00971)
N in-transport	0.000201***	0.0000899	0.000116***	0.0000513	0.000136	0.000517	0.000130**	0.000387
	(0.0000553)	(0.0000554)	(0.0000320)	(0.0000652)	(0.0000884)	(0.000338)	(0.0000542)	(0.000295)
N JKG Prague	0.0000988	0.0000221	0.0000611	0.0000258	0.000872***	0.00781***	0.000526**	0.00767***
	(0.000108)	(0.000105)	(0.0000686)	(0.000104)	(0.000304)	(0.00144)	(0.000247)	(0.00131)
N IKG Vienna	-0.00104	-0.00114	-0.000675	-0.00117	0.00360***	-0.000227	0.00236***	-0.000204
	(0.00106)	(0.00128)	(0.000632)	(0.00127)	(0.000699)	(0.00153)	(0.000448)	(0.00194)
N IKG Berlin	0.0322***	0.00115	0.0253***	0.000969	0.0427*	0.0395	0.0257*	0.0411
	(0.00169)	(0.00148)	(0.00340)	(0.00154)	(0.0253)	(0.0435)	(0.0135)	(0.0445)
Ac. title (non-medical)	-0.0165	-0.0252	-0.00952	-0.0222	0.0278	0.120	0.0152*	0.121
	(0.0177)	(0.0223)	(0.0110)	(0.0231)	(0.0193)	(0.0950)	(0.00882)	(0.0946)
Doctor	0.0538**	0.0411***	0.0350**	0.0445***	0.0540**	0.129	0.0278	0.142
	(0.0233)	(0.0142)	(0.0141)	(0.0148)	(0.0260)	(0.0876)	(0.0193)	(0.0833)
Age (in years)	-0.00189***	-0.00180***	-0.000808***	-0.00190***	-0.00153***	-0.00153**	-0.000869**	-0.00167***
	(0.0000897)	(0.000489)	(0.000144)	(0.000453)	(0.0000955)	(0.000537)	(0.000351)	(0.000507)
Selection pressure evaded	0.0164***	0.0135*	0.0119**	0.0227***	0.0209***	0.00500	0.00820	0.0149*
	(0.00580)	(0.00693)	(0.00544)	(0.00477)	(0.00420)	(0.00812)	(0.00601)	(0.00842)
pval = 0	—	—	0.483	0.001	—	—	0.237	0.019
Clusters	19	19	19	19	19	19	19	19
Observations	14,546	14,546	14,546	14,546	16,200	16,200	16,200	16,200

Notes: Standard errors clustered by transports in parentheses; signi cance codes: $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. = correlation coe cient between residuals in the selection and survival equations. Transport xed e ects, e ects of time spent in Theresienstadt and of age in years and its square and cube, group membership xed e ects, Prague residency xed e ect, and selection equations not shown.

A.7.4 First stage of sample selection models

Table A.7 reports rst-stage coe cients from the baseline models reported in Table 1 in the main text that control for sample selection into transports to Auschwitz. The selection equation speci cations consider each moment (episode) when a prisoner faces non-zero out-transport risks as one observation, and they condition on the SS-speci ed demographic composition of out-transports, i.e., on the average out-transport risks faced by a given demographic group in a given moment (for further discussion, see BeIn et al., 2022). Predicted values of selection probability in OLS speci cations have been partitioned into deciles, serving as a ten-stepped control function in the second stage. For Probit models, sample-selection Probit was used (Van de Ven and Van Praag, 1981). All speci cations contain xed e ects for prisoners' origin de ned by the city year-tuples of their transports into Theresienstadt. (This is a relatively parsimonious speci cation given that there are 97 origin xed e ects for men and 84 for women, while there are 368 in-transport xed e ects for men and 374 for women.) Speci cations, which control for the accumulated selection risk evaded in survival equation also control for this variable in the rst stage. As an additional excluded variable, we include a step function for the ordering of prisoners' ID from their transports to Theresienstadt. Prisoners' ordering within an in-transport was partitioned into four groups based on the hypothesis that prisoner in-transport lists could have served as a tool for selection into out-transports: Prisoners listed lower on the lists of arrivals may have faced lower chances of being selected into out-transports. Results for groups 3 and 4 in Table A.7 (i.e., for prisoners with higher in-transports IDs who were sorted lower on the lists) indeed show lower chances of selection for those at the bottom of the in-transport lists.

Table A.7: Selection on transports from Theresienstadt| rst-stage coe cients for models used in the main text and Table A.6.

	Males				Females			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Probit	OLS	Probit	OLS	Probit	OLS	Probit	OLS
Age	0.000127	0.00126***	0.00864	0.00170***	0.00850	0.000566***	0.0156**	0.000720***
	(0.00517)	(0.000216)	(0.00606)	(0.000217)	(0.00619)	(0.000162)	(0.00639)	(0.000162)
Age^2	0.000414***	-0.0000107*	0.0000283	-0.0000360***	0.000249	0.00000536	-0.000177	-0.0000143***
	(0.000160)	(0.00000565)	(0.000157)	(0.00000566)	(0.000199)	(0.00000409)	(0.000185)	(0.00000408)
Age^3	-0.00000561***	-7.24e-09	-0.00000224	0.000000202***	-0.00000513**	-0.000000120***	-0.00000115	6.29e-08**
	(0.00000170)	(4.33e-08)	(0.00000150)	(4.34e-08)	(0.00000203)	(3.05e-08)	(0.00000178)	(3.04e-08)
Months spent in Theresienstadt	0.0266***	0.00194***	-0.00525	-0.000165***	0.0243***	0.00162***	-0.00000853	0.000180***
	(0.00772)	(0.0000711)	(0.00590)	(0.0000599)	(0.00765)	(0.0000476)	(0.00657)	(0.0000395)
Ac. title (non-medical)	-0.244***	-0.0178***	-0.266***	-0.0199***	-0.283***	-0.0153***	-0.262***	-0.0141***
	(0.0386)	(0.00156)	(0.0415)	(0.00156)	(0.0912)	(0.00360)	(0.0943)	(0.00361)
Doctor	-0.369***	-0.0305***	-0.279***	-0.0230***	-0.473***	-0.0296***	-0.375***	-0.0257***
	(0.0716)	(0.00243)	(0.0698)	(0.00244)	(0.110)	(0.00507)	(0.108)	(0.00509)
JKG Prague	-0.0381	0.00126	0.138***	0.0184***	0.0276	0.00559**	0.149**	0.0145***
	(0.0404)	(0.00211)	(0.0387)	(0.00209)	(0.0723)	(0.00234)	(0.0649)	(0.00235)
IKG Vienna	-0.0653	-0.00836***	-0.0364	-0.00455	-0.140**	-0.00982***	-0.122*	-0.00686**
	(0.0854)	(0.00299)	(0.0884)	(0.00301)	(0.0688)	(0.00331)	(0.0642)	(0.00332)
IKG Berlin	-0.274***	-0.0146***	-0.262***	-0.0130***	-0.346***	-0.0141***	-0.350***	-0.0142***
	(0.0790)	(0.00441)	(0.0809)	(0.00444)	(0.114)	(0.00395)	(0.107)	(0.00397)
Having family in Theresienstadt	0.0444**	0.00292***	0.0161	0.000984	-0.000721	-0.000677	-0.0184	-0.00134**
	(0.0195)	(0.000870)	(0.0188)	(0.000874)	(0.0238)	(0.000604)	(0.0239)	(0.000606)
Selection-risk evaded	-0.831***	-0.0658***	|	|	-0.666***	-0.0470***	|	|
	(0.147)	(0.00121)			(0.134)	(0.000877)		
Exclusion restrictions:								
Demographic selection risk	6.618***	1.061***	6.121***	1.015***	8.233***	1.016***	8.103***	1.009***
(set by the SS)	(0.638)	(0.00588)	(0.633)	(0.00585)	(0.861)	(0.00501)	(0.801)	(0.00502)
In-transp. ID group 2	0.00599	0.000273	0.00913	0.000751	0.0101	0.000728	0.0116	0.00103
	(0.0201)	(0.00111)	(0.0194)	(0.00112)	(0.0105)	(0.000789)	(0.0101)	(0.000792)
In-transp. ID group 3	-0.0221	-0.00188*	-0.0195	-0.00139	-0.0291*	-0.00199**	-0.0284*	-0.00190**
	(0.0235)	(0.00111)	(0.0243)	(0.00111)	(0.0164)	(0.000783)	(0.0160)	(0.000785)
In-transp. ID group 4	-0.0391*	-0.00317***	-0.0399*	-0.00316***	-0.0393***	-0.00265***	-0.0370***	-0.00246***
	(0.0218)	(0.00110)	(0.0219)	(0.00110)	(0.0146)	(0.000773)	(0.0143)	(0.000776)
Deportation origin FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	279,467	279,467	279,467	279,467	429,395	429,395	429,395	429,395
Clusters	19	19	19	19	19	19	19	19

Notes: Standard errors clustered by out-transports in parentheses. Signi cance codes: * 10%, ** 5% *** 1%. Models including `Selection-risk evaded' represent rst stages to models in Table A.6. Columns (3), (4), (7), (8) report rst-stage coe cients in the corresponding columns in Table 1 in the main text.

A.7.5 Alternative specifications of Auschwitz survival models

Alternative samples

The selection of individual prisoners for transports out of Theresienstadt (within demographic orders given by the SS) was under the influence of the ghetto's Jewish self-administration for most transports to the East. This influence of the self-administration was terminated in the fall of 1944 (with transport `En'), when the SS started selecting individual prisoners for transports. For these 8 late transports, it is plausible that the SS made selection decisions that directly linked selection to survival in Auschwitz; for example, if a group of prisoners in Theresienstadt was perceived as dangerous in terms of organizing resistance in the ghetto, the SS could have decided to select the group for transports and to simultaneously order that these prisoners perish upon arrival in Auschwitz. The baseline models in the main analysis therefore exclude these SS-controlled transports. Below, we include the 8 late transports and allow for an interaction between the size of easily observable groups, i.e., the JKG/IKG groups, and the SS-selection-control indicator.

For both males and females, columns (1) and (5) of Table A.8 show AMEs that correspond to the Probit AMEs reported in Table 1, i.e., to our baseline estimates. In both panels of the table, columns (2) and (6) are then based on the extended samples of 27 (19+8) transports with the SS-control interaction implying that larger groups of female prisoners linked to JKG Prague and IKG Berlin and Vienna were at a survival disadvantage when entering Auschwitz on the SS-controlled transports. There is a loss of statistical significance after the inclusion of SS-controlled transports for survival advantage due to presence of linked male JKG Berlin prisoners. Similar loss of significance appears for female prisoners linked in IKG Vienna. It could be that the SS decided on the fate of a specific group of prisoners who were easily observable by the Theresienstadt SS in comparison to other socially linked groups we consider. No other coefficients measuring the impact of the size of social networks on a transport to Auschwitz are sensitive to including the SS-selection-control transports.

In Columns (3), (4), (7), and (8) we additionally explore the sensitivity of our baseline findings to including transports to other deadly (but non-zero-survival) destinations, i.e., Riga and Raasika. Columns (4) and (8) include all SS-selection-control transports. With the exception of the IKG/JKG coefficients discussed above, all other social-linkage coefficients are largely robust to extending the sample as much as possible—to 31 transports from the 19 in our baseline analysis.

Table A.8: Comparison between samples with/without transports controlled by the SS and with/without transports to Riga and Raasika: Probit survival AMEs

	Males				Females			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
N Lipa	0.00101**	0.000800**	0.00101**	0.000813***	|	|	|	|
	(0.000416)	(0.000316)	(0.000405)	(0.000312)				
N Family	0.00280	0.00339	0.00324	0.00377	0.0102**	0.00744	0.0129***	0.0101**
	(0.00439)	(0.00404)	(0.00410)	(0.00383)	(0.00469)	(0.00518)	(0.00422)	(0.00474)
N Same street	0.0160***	0.0138***	0.0148***	0.0129***	0.0240***	0.0259***	0.0207***	0.0235***
	(0.00587)	(0.00475)	(0.00480)	(0.00398)	(0.00337)	(0.00362)	(0.00433)	(0.00381)
N SNAP	0.00171*	0.00108	0.00171*	0.00113	0.0101***	0.00427	0.0112***	0.00472*
	(0.000911)	(0.000752)	(0.000911)	(0.000762)	(0.00262)	(0.00283)	(0.00277)	(0.00280)
N in-transport	0.000195***	0.000182***	0.000111*	0.000106*	0.000131	-0.0000214	0.000154*	0.0000456
	(0.0000547)	(0.0000468)	(0.0000626)	(0.0000593)	(0.0000923)	(0.000194)	(0.0000859)	(0.000162)
N JKG Prague	0.0000941	0.00000993	0.0000974	0.0000147	0.000862***	0.000484	0.000891***	0.000445
	(0.000106)	(0.000104)	(0.000102)	(0.000101)	(0.000302)	(0.000609)	(0.000289)	(0.000589)
N IKG Vienna	-0.00107	-0.000728	-0.00103	-0.000705	0.00382***	0.00101	0.00406***	0.000867
	(0.00104)	(0.000784)	(0.000995)	(0.000763)	(0.000744)	(0.00243)	(0.000783)	(0.00245)
N IKG Berlin	0.0338***	-0.000135	0.0328***	-0.000193	0.0388	-0.00324	0.0418*	-0.00325
	(0.00162)	(0.00180)	(0.00152)	(0.00177)	(0.0238)	(0.00403)	(0.0252)	(0.00406)
Ac. title (non-medical)	-0.0166	0.00379	-0.0121	0.00575	0.0289	0.0157	0.0360*	0.0200
	(0.0177)	(0.0163)	(0.0173)	(0.0159)	(0.0198)	(0.0155)	(0.0187)	(0.0151)
Doctor	0.0529**	0.0436***	0.0516**	0.0432***	0.0505**	0.0389**	0.0520**	0.0393**
	(0.0234)	(0.0163)	(0.0221)	(0.0158)	(0.0256)	(0.0165)	(0.0264)	(0.0165)
Age (in years)	-0.00185***	-0.00139***	-0.00194***	-0.00150***	-0.00129***	-0.00170***	-0.00145***	-0.00179***
	(0.0000828)	(0.0000980)	(0.0000780)	(0.0000894)	(0.0000558)	(0.0000525)	(0.0000545)	(0.0000511)
Clusters	19	27	23	31	19	27	23	31
Selected	14 546	18,913	16,356	20,723	16,200	24,392	18,358	26,550
Included transports:								
Aushwitz	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SS-controlled	No	Yes	No	Yes	No	Yes	No	Yes
Riga Raasika	No	No	Yes	Yes	No	No	Yes	Yes

Notes: Standard errors clustered by out-transports in parentheses. Signi cance codes: * 10%, ** 5% *** 1%. Columns (1) and (5) report identical results to the corresponding columns in Table 1 in the main text.

Richer specifications – shedding light on mechanisms

Specifications reported in the main text and in Tables A.6 and A.8 consider linear dependence between survival chances and the number of linked prisoners travelling together on the same transport, and they do not allow for an interaction between a prisoner's network size and his or her age. Here, we explore non-linear effects of network size and ask whether social networks helped younger and older prisoners differently. We thus modify the baseline specifications as follows:

$$P[\text{Survival}_{it} | j] = \left(\sum_{j} \sum_{k=0}^{K} \sum_{\ell=1}^{L} \beta_{jk\ell} N_{ij}^{\ell} \, age_i^{k} + X_{it} \gamma \right); \qquad \text{(A.18)}$$

where N_{ij}^{ℓ} is the number of prisoners belonging to group j who travel on the same transport as prisoner i, raised to the power ℓ. We perform two separate robustness checks. First, we allow for non-linear effects of the number of socially linked prisoners without any age interactions ($L = 2$ and $K = 0$). Second, we add age interactions ($L = 2$ and $K = 1$).

In both exercises, we focus on three socially linked groups: N Family, N Street address, and N Same in-transport, because these three social linkages exhibit variation within transports to Auschwitz, and so allow for the estimation of richer models. For example, we observe prisoners from different families on the same transport and thus the number of family-linked prisoners varies across prisoners within a transport, while, say, the number of linked prisoners from JKG Prague can only take two distinct values within a transport (zero for non-JKG members and the number of JKG-affiliated prisoners in case prisoner i is a JKG member). When we interact network size (N_{ij}^{ℓ}) with prisoner i's age (age_i), there is high degree of co-linearity between interactions of higher powers of age and higher powers of network size. In any case, the results with higher-order interactions are similar to those reported here, although notably noisier.

In these richer models with non-linearities and interactions, we additionally attempt to be parsimonious in terms of controlling for transport-wide survival rates and prisoners' personal characteristics. Hence, the set of additional controls (X_i) consists of prisoners' age, its square and cube, prisoner's nationality, and the unconditional survival probability calculated as:

$$USP_{it} = \frac{1}{N_t - 1} \sum_{e \neq i}^{N_t} Survival_{et}; \tag{A.19}$$

where the unconditional survival probability for prisoner i travelling on transport t (USP_{it}) is simply the average of all binary survival indicators ($Survival_{et}$) for all prisoners travelling on transport t except for prisoner i. Controlling for USP_{it} is a more parsimonious way to account for the differing conditions in Auschwitz at the time of arrival compared to including fixed effects for each transport separately. By excluding prisoner i from the average, we also avoid problems with endogeneity since prisoner i's error term is absent from USP_{it}.

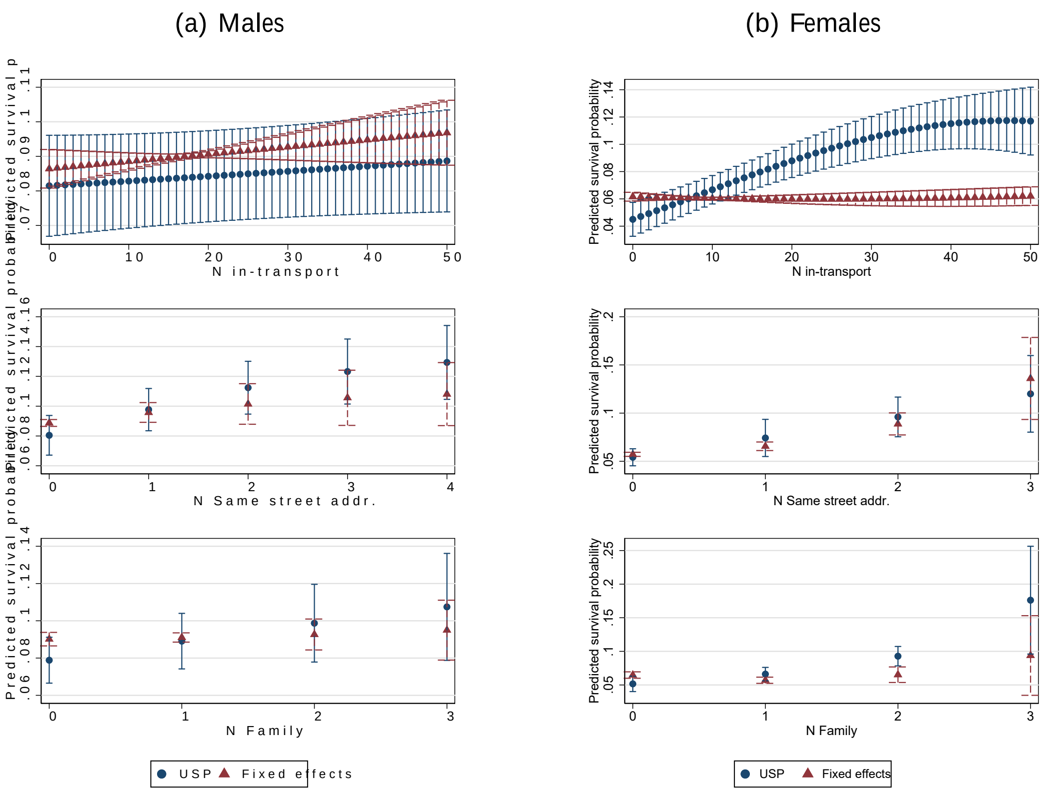
Finally, in an attempt to shed light on potential mechanisms behind our social-network effects, we construct an additional control variable, `Doctor in network', which takes the value of one if prisoner i's social network on transport t contains a medical doctor, and zero otherwise. If social networks operate largely via emotional support, one would expect the coefficient on this variable to be indistinguishable from zero. On the other hand, if the benefits from belonging into a social network stemmed from specific skills, the proximity of a trained physician would be expected to result in a positive, statistically significant effect on survival probability.

Figure A.3 shows that using USP_{it} generally yields similar results to the fixed-effects version of the model utilized in the main text, and that, the estimated effects of social network size are typically near-linear.

Table A.9 reports the AMEs corresponding to non-linear effects of the three network-size variables evaluated in specifications with USP_{it} at two age levels: 25 and 45. The AMEs were computed according to Equation (A.14a) where variable z is age and x-variables are listed Table A.9. Columns (1), (2), (5), and (6) report results shown in Figure A.3 under the heading USP. The remaining columns are shown in Figure A.4. There is no effect of having a medical doctor in a prisoner's social network.

Finally, Figure A.4 plots the age interactions corresponding to model (A.18) with USP_{it}, and with $L = 2$ and $K = 1$. Older prisoners benefit from social net-

Figure A.3: Predicted mean survival probabilities as functions of the number of prisoners from the same transport to Theresienstadt (model (A.18), $L = 2$ and $K = 0$)



works less than younger ones, but they still do benefit from arriving at Auschwitz with a larger group of linked prisoners. The lower effect magnitude for older prisoners may be a consequence of the generally lower survival rates among older prisoners (Figure A.2a).

Table A.9: Average marginal effects from models with quadratic dependence of the network size

	Males				Females			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
N Family	0.0211**	0.00843**	0.0245**	0.00465	0.0387**	0.0140***	0.0489***	0.00583
	(0.00947)	(0.00420)	(0.0113)	(0.00343)	(0.0183)	(0.00444)	(0.0171)	(0.00710)
N Same street addr.	0.0335***	0.0132***	0.0425***	0.00719*	0.0500*	0.0169**	0.0243	0.0279***
	(0.00625)	(0.00263)	(0.0116)	(0.00385)	(0.0263)	(0.00775)	(0.0320)	(0.00828)
N in-transport	0.000258	0.000103	0.000327	0.0000816	0.00518***	0.00161***	0.00589***	0.00131***
	(0.000295)	(0.000122)	(0.000308)	(0.000113)	(0.00101)	(0.000350)	(0.000989)	(0.000435)
Age (in years)	-0.000437	-0.00645***	-0.000365	-0.00656***	-0.000996	-0.00460***	-0.000490	-0.00478***
	(0.000754)	(0.000777)	(0.000757)	(0.000825)	(0.000659)	(0.000616)	(0.000915)	(0.000655)
Doctor in network	-0.0109	-0.00438	-0.00984	-0.00405	0.000741	0.000254	0.000290	0.000101
	(0.0136)	(0.00551)	(0.0132)	(0.00546)	(0.0224)	(0.00768)	(0.0220)	(0.00765)
Age interaction	No	No	Yes	Yes	No	No	Yes	Yes
Age used for calculation	25	45	25	45	25	45	25	45
Clusters	19	19	19	19	19	19	19	19
Selected	14,546	14,546	14,546	14,546	16,200	16,200	16,200	16,200

Notes: Standard errors clustered by out-transports in parentheses. Significance codes: * 10%, ** 5% *** 1%.