Empirical Essays on Inequality

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- Previous publication: Halbmeier, C., Kreutzmann, A.-K., Schmid, T., and Schröder, C. (2019). The fayherriot command for estimating small-area indicators. *The Stata Journal*, *19*(3), 626–644. Copyright © 2019 SAGE Publishing. DOI: `https://doi.org/10.1177/1536867X19874238`.

Chapter 2: Geocoded Tax Data for the German Interwar Period: A Novel Database for Regional Analyses

- Chapter 2 was written in collaboration with Paul Brockmann and Eva Sierminska.
- Previous publication: None.

Chapter 3: The Long-Term Effects of Destruction During the Second World War on Private Wealth in Germany

- Chapter 3 is based on an unpublished article that was written in collaboration with Carsten Schröder.
- Previous publication: None.

Chapter 4: The Role of Characteristics and Behavior for the Development of the Wealth Gap Between Migrants and Natives in Germany

- Chapter 4 was written in single authorship.
- An earlier version of this chapter was published as working paper: Halbmeier, C. (2019). Wealth and Savings of Migrants and Natives in Germany. *SSRN Working paper*. DOI: `http://dx.doi.org/10.2139/ssrn.3315528`.

Rechtliche Erklärung

Erklärung gemäß §4 Abs. 2

Hiermit erkläre ich, dass ich mich noch keinem Promotionsverfahren unterzogen oder um Zulassung zu einem solchen beworben habe, und die Dissertation in der gleichen oder einer anderen Fassung bzw. Überarbeitung einer anderen Fakultät, einem Prüfungsausschuss oder einem Fachvertreter an einer anderen Hochschule nicht bereits zur Überprüfung vorgelegen hat.

Berlin, 15. April 2022
Christoph Halbmeier

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Hiermit erkläre ich, dass ich für die Dissertation folgende Hilfsmittel und Hilfen verwendet habe:

- Editoren: Jupyter, TeXworks, RStudio, Spyder, Sublime Text
- Geodaten: GRASS GIS, Python (Fiona, GeoPandas, Shapely), QGIS
- Schriftsatz, Formatierung und Korrektur: DeepL, LaTeX
- Statistik: Python (Matplotlib, NumPy, Pandas), R, Stata
- Texterkennung: Omnipage, Tesseract

Auf dieser Grundlage habe ich die Arbeit selbstständig verfasst.

Berlin, 15. April 2022
Christoph Halbmeier

1 Estimating Small-Area Indicators to Assess Inequality Between Regions[1]

1.1 Introduction

Various national and international institutions, including the United Nations (Leadership Council of the Sustainable Development Solutions Network, 2015) and the Organisation for Economic Co-operation and Development (Piacentini, 2014), collect comprehensive indicator sets for monitoring purposes. Many indicators refer to subnational areas or domains: federal states, economic sectors, societal groups, etc.

In the socioeconomic context, domain-level indicators are usually derived from population surveys by direct estimation. Direct estimates are based only on the survey data, so small sample sizes can limit their precision. Thus, institutions that provide these indicators usually require a minimum number of observations per domain or impose limits on the variability of the estimates (Eurostat, 2013; Tzavidis et al., 2018). Furthermore, direct estimates cannot be obtained for out-of-sample domains, that is, domains without any observation in the sample.

Small-area estimation techniques use auxiliary data from additional data sources to improve the precision of survey-based direct estimates. Two basic model types can be distinguished: unit- and area-level models. Unit-level models require survey and auxiliary data at the unit level, that is, individual- or household-level information in each domain. Examples are the model proposed by Battese et al. (1988) and the empirical best predictor by Molina and Rao (2010). In comparison, area-level models, such as the Fay-Herriot (FH) model (1979),[2] require only domain-level auxiliary data, hence their popularity in applied research.

`fayherriot` provides empirical best linear unbiased predictors (EBLUP), which are linear combinations of the domain-level direct estimator and a regression-synthetic component based on a linear model. The underlying model can also be expressed as a special linear mixed model. In contrast to a standard linear mixed model [encompassed in `mixed` (Rabe-Hesketh and Skrondal, 2012) or `gllamm` (see

[1]This is a post-peer-review and copy-edited version of the article *The fayherriot Command for Estimating Small-Area Indicators* published in The Stata Journal. The authenticated version is available online at: https://doi.org/10.1177/1536867X19874238. Copyright © 2019 SAGE Publishing. Please cite as Halbmeier, C., Kreutzmann, A.-K., Schmid, T., and Schröder, C. (2019). The fayherriot command for estimating small-area indicators. *The Stata Journal*, *19*(3), 626–644.

[2]Applications include, for example, the estimation of income and poverty rates (Powers et al., 2008; Huang and Bell, 2012) and educational indicators (Schmid et al., 2017).

StataCorp (2017)], the FH model builds on two error terms on the domain level, with domain-specific variances of one error term and a common variance of the other error term. The model assumes linearity and normality of its two error terms. Corral et al. (2018) implement a standard version.

`fayherriot` extends the existing possibilities in Stata and performs the following:

- estimation of the FH model as described in Rao and Molina (2015, pp. 123-129) with restricted maximum likelihood (REML) and maximum likelihood estimation (MLE) of the variance of the random effects,
- estimation of the mean squared error (MSE) as proposed in Datta and Lahiri (2000) and Prasad and Rao (1990),
- prediction and MSE estimation for out-of-sample domains (Rao and Molina, 2015, p. 126 and p. 139),
- estimation with adjusted methods as proposed in Li and Lahiri (2010) and Yoshimori and Lahiri (2014) to deal with nonpositive estimates of the variance of the random effects,
- estimation of the log-transformed FH model including a bias correction by Slud and Maiti (2006) to deal with violations of model assumptions, for example, non-normality of the error terms, and
- estimation of the FH model for proportions defined on the $[0,1]$ interval, that is, with the dependent variable transformed by the arcsine square root transformation. The back-transformation and the corresponding boundaries of a bootstrap confidence interval following Casas-Cordero et al. (2016, pp. 394-397) and Schmid et al. (2017, pp. 1173-1177) are provided.

1.2 The FH Model

1.2.1 Modeling

The FH model (Fay and Herriot, 1979) combines domain-level direct estimates (based on survey data) with aggregated domain-level covariates (for example, from register or administrative data). The direct estimator should be a linear statistic such as an arithmetic mean, total, or share.

The FH model builds on a sampling and a linking model. According to the sampling model,

$$\widehat{\theta}_d = \theta_d + e_d \quad \text{for} \quad d = 1, \ldots, D$$

the observed direct estimator for domains $d = 1, \ldots, D$, $\widehat{\theta}_d$, is composed of the true value, θ_d, and a sampling error, e_d, with mean zero and variance $\sigma^2_{e_d}$. The model assumes that the sampling error variance of each domain is known. In practice, the variance of the direct estimator is used frequently as an estimate for $\sigma^2_{e_d}$ (You and Chapman, 2006). To consider sampling weights in the FH model, one can use the weighted direct estimator and its corresponding variance. For example, one can use the Horvitz-Thompson estimator for the mean (Horvitz and Thompson, 1952). According to the linking model,

$$\theta_d = \mathbf{x}_d^\top \boldsymbol{\beta} + u_d \quad \text{for} \quad d = 1, \ldots, D$$

the true value, θ_d, is explained by domain-specific covariates, $\mathbf{x}_d$; a random effect, u_d; and the regression parameters $\boldsymbol{\beta}$. The random effect is independently, identically, and normally distributed with mean zero and variance σ^2_u. The model assumes interdomain correlations to be zero.

Combining the sampling and the linking model gives the FH model, which is a linear mixed model of the form

$$\widehat{\theta}_d = \mathbf{x}_d^\top \boldsymbol{\beta} + u_d + e_d \quad \text{for} \quad d = 1, \ldots, D. \tag{1.1}$$

The FH estimator (EBLUP) is given by $\widehat{\theta}_d^{\mathrm{FH}} = \mathbf{x}_d^\top \widehat{\boldsymbol{\beta}} + \widehat{u}_d$. It can also be expressed more intuitively as a weighted average of the direct and a regression-synthetic estimator,

$$\widehat{\theta}_d^{\mathrm{FH}} = \widehat{\gamma}_d \widehat{\theta}_d + (1 - \widehat{\gamma}_d)\mathbf{x}_d^\top \widehat{\boldsymbol{\beta}}. \tag{1.2}$$

The estimate $\widehat{\gamma}_d = \widehat{\sigma}^2_u / (\widehat{\sigma}^2_u + \sigma^2_{e_d})$, or the "shrinkage factor", weights the direct estimate and the regression-synthetic part. The weight on the direct estimate decreases with the sampling error variance.

For out-of-sample domains, $\widehat{\gamma}_d$ is not defined, and the regression-synthetic estimate $\mathbf{x}_d^\top \widehat{\boldsymbol{\beta}}$ is used. A domain is treated as out-of-sample if either the direct estimate or the sampling error variance is missing. Missing values in the domain-specific covariates (usually obtained from register or administrative data) are not allowed; that is, each explanatory variable needs to have a value for each domain.

1.2.2 Estimating the Variance of the Random Error

The FH model requires an estimation of the variance of the random error, σ^2_u, and of the regression parameters, $\boldsymbol{\beta}$. Standard estimation techniques for σ^2_u are, among others, REML and MLE. These methods do not guarantee positive variance estimates (Yoshimori and Lahiri, 2014; Li and Lahiri, 2010). Especially if there are few domains, the variance estimates can be negatively biased or even below zero. In the latter case, the variance estimate is set to zero. An underestimation of the variance component could lead to a significant overshrinkage of the direct estimate to the regression-synthetic part; that is, too much weight is put on the regression-synthetic part.

Adjusted estimation methods, such as the adjusted maximum residual-likelihood approach (ARYL) following Yoshimori and Lahiri (2014) and the adjusted maximum-profile likelihood (AMPL) following Li and Lahiri (2010), ensure strictly positive variance estimates. `fayherriot` allows the estimation of σ_u^2 with the REML (as default), MLE, ARYL and AMPL.[3] The method can be specified in the option `sigmamethod()`. The vector of regression parameters, $\boldsymbol{\beta}$, is estimated by the empirical best linear unbiased estimator $\widehat{\boldsymbol{\beta}}$ (Rao and Molina, 2015, p. 124).

1.2.3 Evaluating the Precision

The precision of the EBLUP is evaluated by means of the MSE, defined as

$$\mathrm{MSE}\left(\widehat{\theta}_d^{\mathrm{FH}}\right) = \mathrm{E}\left\{\left(\widehat{\theta}_d^{\mathrm{FH}} - \theta_d\right)^2\right\}.$$

Because the true value θ_d is unobserved, $\mathrm{MSE}(\widehat{\theta}_d^{\mathrm{FH}})$ must be estimated. For in-sample domains, MSE estimators have been proposed for estimates of σ_u^2 relying on REML (Prasad and Rao, 1990, p. 167), MLE (Datta and Lahiri, 2000, p. 619), ARYL (Yoshimori and Lahiri, 2014), and AMPL (Li and Lahiri, 2010, p. 886). For out-of-sample domains, MSE estimators have been proposed only for REML and MLE (Rao and Molina, 2015, p. 139). `fayherriot` automatically selects the appropriate MSE estimator.

1.2.4 Dealing with Model-Assumption Violations

The FH model assumes linearity and normality of its two error terms. If there is a violation of these assumptions, a log-transformation of the direct estimator might be an option (Slud and Maiti, 2006). Choosing this option requires an appropriate transformation of the variance of the original direct estimator.[4] Neves et al. (2013) suggest the transformation,

$$\begin{aligned} \widehat{\theta}_d^{*} &= \log\left(\widehat{\theta}_d\right) \\ \mathrm{var}\left(\widehat{\theta}_d^{*}\right) &= \left(\widehat{\theta}_d\right)^{-2}\mathrm{var}\left(\widehat{\theta}_d\right) \end{aligned} \tag{1.3}$$

[3] See Yoshimori and Lahiri (2014) for a general discussion of the comparative advantages of each method.

[4] It is not appropriate to take the logarithm of the variance. This is because the variance of a log-transformed variable is different from the log-transformed variance of the original variable.

with * indicating the transformed scale.

Equation 1.1 is estimated using $\widehat{\theta}_d^*$ as the direct estimate and $\text{var}(\widehat{\theta}_d^*)$ as the estimate for the sampling error variance. To bring the estimated EBLUP and MSE back from the transformed to the original scale, we advise a bias correction (Slud and Maiti, 2006; Sugawasa and Kubokawa, 2017). `fayherriot` includes two back-transformation methods: the "crude" method, shown in Neves et al. (2013) and Rao and Molina (2015), and (as the default) the bias correction proposed by Slud and Maiti (2006). For the point estimates, these methods are defined as

$$\widehat{\theta}_d^{\text{FH, crude}} = \exp\left\{\widehat{\theta}_d^{\text{FH}^*} + 0.5\text{MSE}\left(\widehat{\theta}_d^{\text{FH}^*}\right)\right\}$$
$$\widehat{\theta}_d^{\text{FH, Slud-Maiti}} = \exp\left\{\widehat{\theta}_d^{\text{FH}^*} + 0.5\widehat{\sigma}_u^2(1-\widehat{\gamma}_d)\right\}$$

with * indicating the transformed scale.

The Slud-Maiti back-transformation relies on MLE for the estimation of σ_u^2. Because it requires an estimate of $\widehat{\gamma}_d$, it is only applicable for in-sample domains. The crude back-transformation can be used for in- and out-of-sample predictions.

For estimating the precision of the back-transformed EBLUPs, Slud and Maiti (2006, p. 248) developed an MSE estimator when using the log-transformation. The crude method uses the estimates in the transformed scale and the following back-transformation:

$$\text{MSE}\left(\widehat{\theta}_d^{\text{FH, crude}}\right) = \exp\left(\widehat{\theta}_d^{\text{FH}^*}\right)^2 \text{MSE}\left(\widehat{\theta}_d^{\text{FH}^*}\right)$$

1.2.5 Overview of Functionalities

Figure 1.1 gives an overview of the functionalities of the `fayherriot` command. Additionally, the arcsine square root transformation can be used for proportions and `fayherriot` returns back-transformed EBLUPs and corresponding boundaries of bootstrap confidence intervals. For a detailed description, we refer to Casas-Cordero et al. (2016, pp. 394-397) and Schmid et al. (2017, pp. 1173-1177).

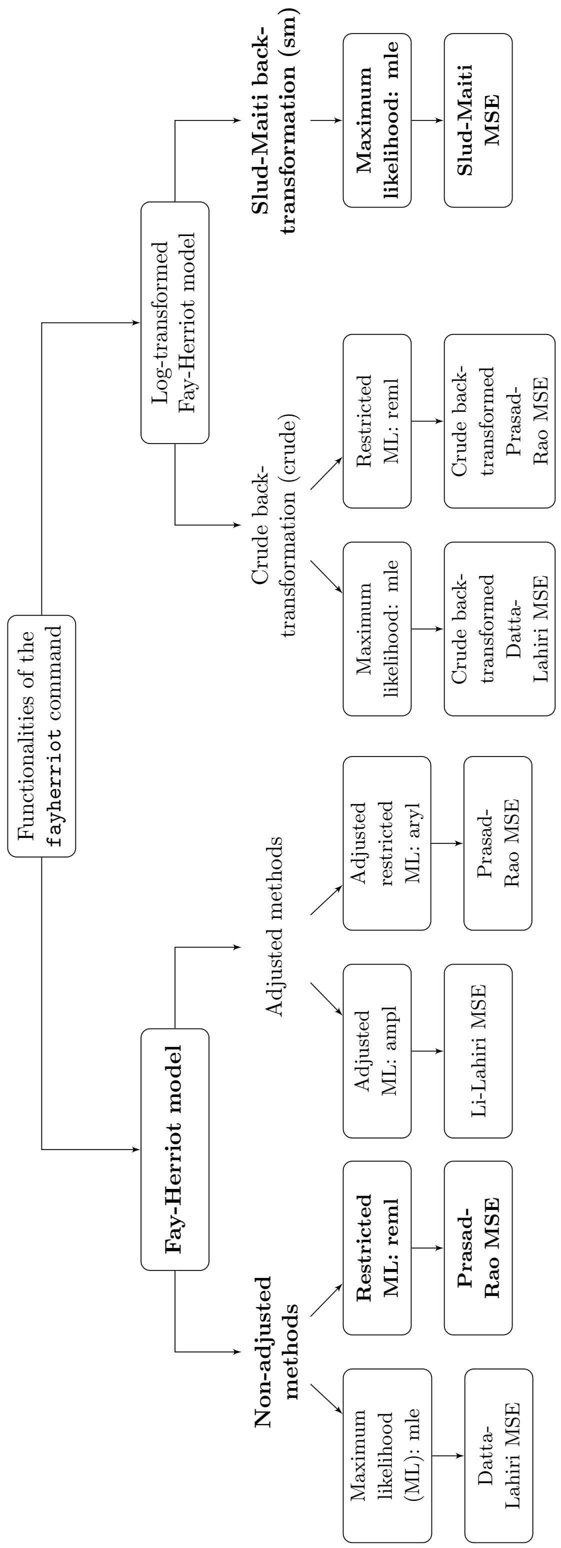


Figure 1.1: Functionalities of the fayherriot command. The two lowest levels describe the estimation methods of σ_u^2 and the corresponding MSE estimators, respectively. The default options are written in bold.

1.3 The fayherriot Command

1.3.1 Syntax

`fayherriot` runs in Stata 12 and later versions. The syntax is

`fayherriot` *depvar* [*varlist*] [*if*] [*in*] , `variance(`*varname*`)`
  [`sigmamethod(`*method*`)` `logarithm` `arcsin` `biascorrection(`*method*`)`
  `initialvalue(#)` `reps(#)` `level(#)` `eblup(`*name*`)` `mse(`*name*`)` `gamma`
  `nolog`]

The command runs on datasets at the domain level with one observation per domain. *depvar* is the direct estimate, $\widehat{\theta}_d$ (in the documentation, theta), and *varlist* corresponds to the auxiliary explanatory variables, $\mathbf{x}_d$ (in the documentation, X).

1.3.2 Options for fayherriot

`variance(`*varname*`)` determines the variable containing the sampling error variances, $\sigma^2_{e_d}$. This variance is assumed to be known in the model. However, it often needs to be estimated from the data. One possibility is to use the estimated variance of the direct estimator theta specified in *depvar* for each domain. Whenever the direct estimator needs to be logarithmized with logdepvar = log(*depvar*), the estimated variance can be modified as logvar = $\sigma^2_{ed}/(depvar^2)$ (Neves et al., 2013). In case the estimate is transformed by the arcsine transformation depvar_arcsin = asin $\left(\sqrt{depvar}\right)$, the estimated variance can be approximated by var_arcsin = $1/(4\times\text{effsample})$ with effsample being the effective sample size (Jiang et al., 2001). The effective sample size is an estimate of the sample size that a survey based on simple random sampling would have to have the same sampling error as the currently used survey with the corresponding sampling design. It can be estimated by the division of the sample size and the design effect (Lohr, 2010, p. 239). `variance()` is required.

`sigmamethod(`*method*`)` specifies the method for the estimation of the variance of the random effect σ^2_u: `reml`, `mle`, `ampl`, or `aryl`. The default is `sigmamethod(reml)`. If a zero estimate is received for the variance—which is more likely when the number of domains is small—the adjusted maximum-likelihood methods `ampl` (Li and Lahiri, 2010) and `aryl` (Yoshimori and Lahiri, 2014) may help to estimate strictly positive variances.

`logarithm` indicates that the dependent variable in *depvar* is the log-transformed direct estimate. A log-transformed FH model is suitable when the linearity or normality assumption of the error terms is not fulfilled. `logarithm` automatically back-transforms EBLUP and MSE to the original scale.

`arcsin` indicates that the dependent variable in *depvar* is the direct estimate transformed by the arcsine square root transformation. This transformation is especially suitable when the indicator of interest is a proportion confined to the [0,1] interval. `arcsin` automatically back-transforms EBLUP and the boundaries of the bootstrap confidence interval to the original scale.

`biascorrection(`*method*`)` determines the method for the back-transformation of EBLUP and MSE in a log-transformed FH model. The EBLUPs and MSEs in the transformed scale can be back-transformed using the bias correction proposed by Slud and Maiti (2006), which is set as a default, and a crude bias correction (Neves et al., 2013; Rao and Molina, 2015). When the arcsine transformation is used, the EBLUP and the boundaries of the confidence interval are, by default, back-transformed by the inverse transformation as proposed in Casas-Cordero et al. (2016), and thus no method needs to be specified.

`initialvalue(#)` sets the initial value of the optimization algorithm for estimating the variance of the random effect σ_u^2 to #. The default is `initialvalue(0.0)`.

`reps(#)` sets the number of bootstrap repetitions for the confidence intervals to #. The default is `reps(100)`. The confidence intervals are returned if `arcsin` is specified.

`level(#)` sets the confidence level of bootstrap confidence intervals to #. The default is `level(95)`, which corresponds to a 95% confidence level.

`eblup(`*name*`)` stores the EBLUP estimates in the variable *name*. For in-sample domains, the EBLUPs are defined as `eblup()` $= \mathbf{x}_d^\top \widehat{\boldsymbol{\beta}} + \widehat{u}_d$, where $\mathbf{x}_d^\top \widehat{\boldsymbol{\beta}}$ are the estimated fixed effects and $\widehat{u}_d$ is the estimated random effect. The EBLUP can also be expressed as the weighted average of the direct estimate and a synthetic part `eblup()` $= \widehat{\gamma}_d \times \widehat{\theta}_d + (1 - \widehat{\gamma}_d)\, \mathbf{x}_d^\top \widehat{\boldsymbol{\beta}}$. For out-of-sample domains, the EBLUP shrinks to the synthetic part, `eblup()` $= \mathbf{x}_d^\top \widehat{\boldsymbol{\beta}}$.

`mse(`*name*`)` stores the MSE estimates in the variable *name*. The MSE depends on the estimation procedure of σ_u^2 . For `sigmamethod(reml)`, the MSE estimator relies on Prasad and Rao (1990, p. 167); for `sigmamethod(mle)`, the MSE estimator relies on Datta and Lahiri (2000, p. 619); for `sigmamethod(ampl)`, the MSE estimator relies on Li and Lahiri (2010, p. 886); and for `sigmamethod(aryl)`, the MSE estimator relies on Yoshimori and Lahiri (2014). For the log-transformed FH model under the Slud-Maiti bias correction, the MSE is defined as in Slud and Maiti

(2006, p. 248). It is only applicable to in-sample domains. Under the crude bias correction, for in- and out-of-sample domains, mse(eblup_backtransformed) = exp (EBLUP)^2 $\times$ mse(EBLUP) (Neves et al., 2013). In case arcsin is chosen, upper and lower bounds of bootstrap confidence intervals (Casas-Cordero et al. (2016, pp. 394-397), Schmid et al. (2017, pp. 1173-1177)) are returned.

gamma reports summary statistics of the shrinkage factor, $\widehat{\gamma}_d = \widehat{\sigma}_u^2 / (\widehat{\sigma}_u^2 + \sigma_{e_d}^2)$, where $\widehat{\sigma}_u^2$ is the estimated variance of the random effect and $\sigma_{e_d}^2$ is the sampling error variance of each domain provided in variance(*varname*).

nolog suppresses the display of the iteration log of the optimization algorithm.

1.3.3 predict after fayherriot

Syntax

The syntax for predict following fayherriot is

predict [*type*] *newvar* [*if*] [*in*] [, eblup mse reps(#) level(#) ehat estandard uhat gamma cvdirect cvfh]

Options

eblup generates the EBLUPs as defined above; this is the default.

mse generates estimates for the MSE or the boundaries of the confidence interval as defined above.

reps(#) sets the number of bootstrap repetitions for the confidence intervals to #. The default is reps(100).

level(#) sets the confidence level of the bootstrap confidence intervals to #. The default is level(95), which corresponds to a 95% confidence level.

ehat calculates the residuals. The residuals are defined as $\widehat{e}_d = (1 - \widehat{\gamma}_d) \times (\widehat{\theta}_d - \mathbf{x}_d^\top \widehat{\boldsymbol{\beta}})$, where $\widehat{\theta}_d$ corresponds to *depvar*.

estandard calculates the standardized residuals defined as $\widehat{e}_d / \sqrt{\sigma_{e_d}^2}$.

`uhat` calculates the random effects. The random effects are defined as $\widehat{u}_d = \widehat{\gamma}_d \times \left(\widehat{\theta}_d - \mathbf{x}_d^\top \widehat{\boldsymbol{\beta}}\right)$.

`gamma` generates the shrinkage factor as defined above.

`cvdirect` calculates the coefficient of variation (CV) of direct estimates. `cvdirect` $= 100 \times \sqrt{\sigma_{e_d}^2} / \widehat{\theta}_d$, where $\widehat{\theta}_d$ corresponds to *depvar* and $\sigma_{e_d}^2$ is the sampling error variance provided in `variance(`*varname*`)`. In case `logarithm` is specified, `cvdirect` $= 100 \times \sqrt{\sigma_{e_d}^{2'}} / \widehat{\theta}'$ with $\widehat{\theta}' = \exp\left(\widehat{\theta}_{\log}\right)$, and $\sigma_{e_d}^{2'} = \text{var}(\widehat{\theta}_{\log}) \times \left(\widehat{\theta}'\right)^2$. In case `arcsin` is chosen, the CV for the direct estimate cannot be returned because the direct variance in the original scale is unknown within the `fayherriot` command.

`cvfh` calculates the CV based on EBLUPs: `cvfh` $= 100 \times \sqrt{\texttt{mse}} / \texttt{eblup}$. In case `arcsin` is chosen, the CV for the EBLUP cannot be returned because no MSE estimation is provided.

1.3.4 Stored Results

Scalars	
e(N_in)	number of observations used for estimation of e(b) and e(sigma2_u)
e(N_out)	number of out-of-sample observations for which EBLUP is calculated
e(sigma2_u)	estimated sigma2_u
e(r2_a)	adjusted R^2 of unweighted ordinary least squares
e(r2_fh)	adjusted R^2 according to Lahiri and Suntornchost (2015)
e(p_e)	p-value of Shapiro-Wilk test for normality of residuals
e(V_e)	test statistic of Shapiro-Wilk of normality of residuals
e(p_u)	p-value of Shapiro-Wilk test for normality of the random effect
e(V_u)	test statistic of Shapiro-Wilk test of normality of the random effect
Macros	
e(cmd)	fayherriot
e(title)	Fay-Herriot estimation
e(depvar)	name of dependent variable
e(variance)	name of variance variable
e(sigma_method)	sigmamethod() estimation method
e(bias_correction)	bias-correction method for the back-transformation of transformed EBLUPs
e(logarithm)	logarithm true or false
e(arcsin)	arcsine true or false
e(properties)	b V
e(predict)	program to implement predict
e(marginsok)	predictions allowed by margins
e(marginsnotok)	predictions disallowed by margins
Matrices	
e(b)	coefficient vector
e(V)	variance-covariance matrix of coefficients
e(gamma)	summary of values of shrinkage factor gamma
Functions	
e(sample)	marks estimation sample

1.4 Example

We use the FH model to estimate households' material well-being in 2015 in Germany: at the level of federal states (16 divisions), planning regions (96 divisions), and districts (402 divisions). Material well-being is defined as region-specific average equivalent income, that is, household disposable income divided by the Organisation for Economic Co-operation and Development modified scale (Hagenaars et al., 1994).

Following the policies used by several statistical agencies to evaluate the precision of the regional estimates, we rely on the CV, which is the standard error of the estimate divided by the estimate (in percent). For instance, Statistics Canada releases data without warning about low precision if the CV is below 16.5% (Statistics Canada, 2013; Eurostat, 2013).

1.4.1 Data Description and Direct Estimates

We derive the direct estimates from the German Socio-Economic Panel (SOEP), which is a household survey covering about 15,000 households per year (Goebel et al., 2019).

Table 3.2 provides the division-specific numbers of SOEP households. Sample sizes by federal states are large (median: 624), ranging from 114 to 3,159 observations. Sample sizes by planning regions are considerably smaller (median: 132), ranging from 32 to 665 observations. Sample sizes by districts range from 10 to 648 observations (median: 32).[5] Because of small sample sizes, we expect that many direct estimates for planning regions and districts are measured with high imprecision.

Table 1.1: Number of regions and sample sizes.

		Sample-size distribution				
Regional division	Number of regions	Minimum	p10	p25	p50	Maximum
Federal states	16	114	144	444	624	3159
Planning regions	96	32	61	88	132	665
Districts	357	10	14	20	32	648

Note: Data are from SOEP v33.1. Computations are our own.

For each regional level, Table 1.2 provides direct estimates of mean equivalent income and coefficients of variation, our precision indicator.[6] The table suggests considerable regional heterogeneities in material well being. Across federal states, mean equivalent income ranges from €1,362 to €1,863; across planning regions from €1,298 to €2,101; and across districts from €1,023 to €2,976. As expected, coefficients of variation increase as we move to smaller regional levels. In line with the policy of Statistics Canada, not all estimates could be reported for the planning regions and the districts without warning of low precision. In the following we show how this can be achieved using the FH model. In particular, we can a) improve the precision of all estimates and b) receive estimates for the districts without a direct estimator.

[5]For confidentiality issues, we discarded areas with fewer than 10 observations. This left us with 357 out of 402 districts.

[6]We estimated standard errors using the random group estimator to account for the survey sampling design (Rendtel, 1995).

Table 1.2: Summary of mean equivalent household income and coefficients of variation by regional level.

Regional division	Min	p10	p25	p50	p75	p90	Max
(A) Mean equivalized household income							
Federal states	1362	1398	1492	1683	1777	1841	1863
Planning regions	1298	1400	1495	1664	1780	1898	2101
Districts	1023	1311	1463	1641	1847	2049	2976
(B) Coefficient of variation							
Federal states	0.6	0.8	1.4	2.2	3.8	6.4	8.0
Planning regions	1.5	3.4	4.1	5.3	7.2	9.0	18.2
Districts	2.2	5.8	7.6	10.2	13.6	16.7	42.5

Note: Data are from SOEP v33.1. Computations are our own.

1.4.2 Estimation Using fayherriot

For fitting the FH model, we rely on the direct estimates of average equivalent incomes (Table 1.2); their sampling error variances, $\sigma^2_{e_d}$; and region-specific explanatory variables. The set of explanatory variables in this example includes the unemployment rate, the share of population older than 65 years, and per-capita income tax revenue.[7]

1.4.2.1 FH Model for the Planning Regions

In the following, we detail the application of `fayherriot` at the level of planning regions. In this example, all regions are sampled and the model assumptions are fulfilled. The underlying dataset includes 96 observations (one observation per region):

```
. use dataror.dta, clear
. summarize
    Variable |        Obs        Mean    Std. Dev.       Min        Max
-------------+---------------------------------------------------------
      income |         96    1658.387    188.7142   1297.915   2100.683
directvari~e |         96    11448.52    12856.35   612.4922      96107
unemployment |         96    6.259375    2.579212        2.1       12.8
   incometax |         96    399.2719    105.6913      211.6        705
     share65 |         96    56.48438    .8259385       54.9       58.2
-------------+---------------------------------------------------------
           N |         96    162.3854    125.7412         32        665
```

[7]The explanatory variables are obtained from INKAR (Bundesinstitut für Bau-, Stadt- und Raumforschung, 2017), a database of regional indicators derived from high-quality and large-scale national census and register data.

To fit the FH model, we type:

```
. fayherriot income unemployment incometax share65,
> variance(directvariance) gamma nolog
Sigma2_u estimation method:     reml            N in sample       =         96
Transformation of depvar:       none            N out of sample   =          0
EBLUP and MSE bias correction:  none            Sigma2_u          =  4683.7208
                                                Adj R-squared     =     0.5769
                                                FH R-squared      =     0.7808
------------------- Gamma ------------------
  Min       5%      Median      95%       Max
0.0465    0.1464    0.3726    0.7307    0.8844

------------------------------------------------------------------------------
      income |      Coef.   Std. Err.      z    P>|z|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
unemployment |   5.956309   6.664692     0.89   0.371    -7.106248    19.01887
   incometax |   1.278903   .1365014     9.37   0.000     1.011365    1.546441
     share65 |  -38.88107   18.04845    -2.15   0.031    -74.25537   -3.506762
       _cons |   3301.427   1013.564     3.26   0.001     1314.877    5287.976
------------------------------------------------------------------------------
Shapiro-Wilk test for normality:
 Residuals e (standardized)  V =    0.837  p-value = 0.653
 Random effects u            V =    0.392  p-value = 0.981
------------------------------------------------------------------------------
```

The syntax of the command is inline with the familiar Stata regression syntax: `income` contains the direct estimates of mean equivalent income and is regressed on the regional explanatory variables, `unemployment`, `incometax`, and `share65`. `variance()` specifies the variable containing the sampling error variances, `directvariance`. We specify the gamma option to display summary statistics of shrinkage factors $\widehat{\gamma}_d$. `nolog` suppresses the iteration log of the optimization algorithm.

`N in sample` indicates that the full set of 96 planning regions was used in the estimation. `FH R-squared` is an indicator for the goodness of fit of the FH model, proposed by Lahiri and Suntornchost (2015, p. 317, $AdjR_h^2$). Similar to the standard R^2, it expresses the explained variation of `income` in relation to the total variation, while taking into account that some variation in `income` is due to the sampling error. In this example, about 78% of the variation is explained.

The variance of the random effects, $\widehat{\sigma}_u^2 = 4{,}683.72$, is estimated using the REML approach (the default). Together with the sampling error variances $\sigma_{e_d}^2$, it determines the shrinkage factor $\widehat{\gamma}_d$. The shrinkage factor shows how direct estimates and model predictions are weighted when calculating the EBLUP. Large values of $\widehat{\gamma}_d$ mean that a large weight is given to the direct estimate $\widehat{\theta}_d$. In our example, the distribution of $\widehat{\gamma}_d$ ranges from 0.046,5 to 0.884,4 with its median being 0.372,6. So for some regions, the EBLUP relies strongly on the model predictions (small value of $\widehat{\gamma}_d$), and strongly on the direct estimator for others (large value of $\widehat{\gamma}_d$). The Shapiro-Wilk test for normality shows that neither normality of the realized residuals, $\widehat{e}_d$, nor of the random effects, $\widehat{u}_d$, is rejected. Hence, the model assumptions are not violated.

1.4.2.2 Log-transformed FH Model for the Districts

In the district-level analysis, not all regions are sampled, and the normality assumption of the model is violated. Hence, we log-transform equivalent incomes and the variances of the sampling error,

```
. use datadistricts.dta, clear
. gen logincome = log(income)
(45 missing values generated)
. gen directlogvariance = directvariance/income^2
(45 missing values generated)
```

and fit the log-transformed FH model:

```
. fayherriot logincome unemployment incometax share65,
> variance(directlogvariance) nolog logarithm

Sigma2_u estimation method:     mle            N in sample     =        357
Transformation of depvar:       logarithm      N out of sample =         45
EBLUP and MSE bias correction:  sm             Sigma2_u        =     0.0089
                                               Adj R-squared   =     0.2891
                                               FH R-squared    =     0.4745

   logincome      Coef.   Std. Err.      z    P>|z|     [95% Conf. Interval]

unemployment   -.0004102    .003304   -0.12   0.901    -.0068858    .0060655
   incometax    .0007471   .0000904    8.26   0.000     .0005698    .0009243
     share65   -.0063528    .003548   -1.79   0.073    -.0133067    .0006011
       _cons    7.241288   .1051244   68.88   0.000     7.035248    7.447328

Shapiro-Wilk test for normality:
 Residuals e (standardized)  V =    1.614  p-value = 0.128
 Random effects u            V =    0.830  p-value = 0.670
```

By specifying the `logarithm` option, `fayherriot` transforms the estimated EBLUP and MSE back to the original scale. Because we did not specify the bias-correction method, the estimation method is MLE and the bias correction follows Slud and Maiti (2006) (see Figure 1.1). In this default setting, only estimates for the 357 in-sample districts are calculated. `biascorrection(crude)` could be specified to obtain in- and out-of-sample estimates.

1.4.3 Comparison of Direct and FH Estimates

Next we compare the direct with the FH point estimates (EBLUP) and assess their precision. There are two equivalent ways to obtain the EBLUPs and their level of precision (MSE). First, by specifying the `eblup(`*varname*`)` and `mse(`*varname*`)` option (here done for the planning regions):

```
. fayherriot income unemployment incometax share65,
variance(directvariance) nolog eblup(eblupROR) mse(mseROR)
```

The second is using the postestimation `predict` routine directly after the `fayherriot` command:

```
. predict eblupROR, eblup
. predict mseROR, mse
```

An additional feature of `predict` is that it provides the CV for the direct and FH estimates.

```
. predict cvROR_FH, cvfh
. predict cvROR_direct, cvdirect
```

To assess the magnitude of adjustments, Figure 1.2 presents the ratios of EBLUPs and direct estimates against region-specific sample sizes.[8] For federal states, the ratios are all close to 1, suggesting small adjustments of the direct estimator. For planning regions and districts, adjustments are larger, which is an expected result given smaller sample sizes of these domains.

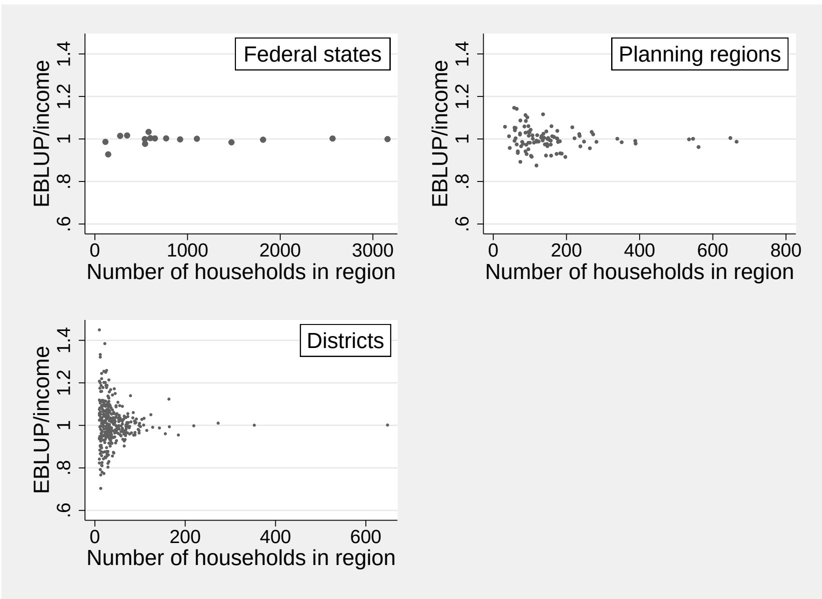


Figure 1.2: Ratio of the EBLUP to the direct (income) estimates plotted against regional sample sizes for all three regional divisions—federal states, planning regions, and districts. Only in-sample domains are plotted. Data are from SOEP v33.1. Computations are our own.

To assess the gain in precision, Figure 1.3 provides box plots of coefficients of variation for the direct and FH estimates. The horizontal line indicates the threshold of 16.5 suggested by Statistics Canada. For the direct estimates, several CVs at the district and planning region level exceed the threshold. For the FH estimates, in contrast, CVs for all regional levels are under the threshold.

[8]For further comparison methods, see Brown et al. (2001).

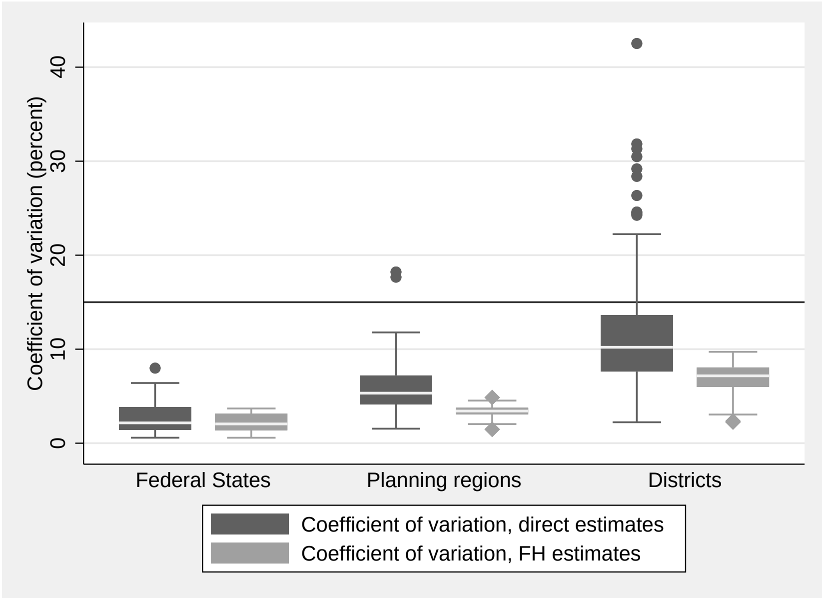


Figure 1.3: Box plots of the distribution of the coefficients of variation for the federal states, the planning regions, and the districts. The horizontal line indicates the precision threshold of 16.5%. Only in-sample domains are plotted. Data are from SOEP v33.1. Computations are our own.

1.5 Conclusion

We implemented the FH model in Stata. It is a small-area estimation technique and aims at improving the precision of direct estimators from a survey by using additional domain-level covariate information. We introduced the `fayherriot` command and provided an application to regional heterogeneities in material well being in Germany. The results showed that the precision of the FH model estimates is markedly higher than that of the direct estimates.

2 Geocoded Tax Data for the German Interwar Period: A Novel Database for Regional Analyses

2.1 Introduction

Due to its history, 20th century Germany has become a laboratory for studies on totalitarianism, war, and forced migration, but also on reconstruction and reunification.[1] This research relies in many cases on regional data. For instance, in their seminal work, Burchardi and Hassan (2013) use region-level variations in expellee numbers to investigate the role of social ties after the German reunification. Regional data for Germany are scarce, however, especially for the first half of the 20th century and concerning crucial economic indicators, such as the gross domestic product (GDP), income, or wealth. As a consequence, researchers studying this period have to work with less or more aggregated data, which potentially weakens our understanding of Germany's past and its repercussions.

To address this gap in the German regional data infrastructure, this article provides a novel regional database. The database includes geocoded tax revenue data for all years from 1926 to 1938 and for five different taxes—payroll, income, corporate, wealth, and turnover tax. The original data were published by the Statistical Office of that time and are available at the regional level of about 900 historical tax districts (Statistisches Reichsamt, 1941). For this article, we geocoded the tax districts for each year based on various sources. The geocoding allows researchers to map, transform, and merge the data with other datasets, which expands the research possibilities of the data. A snapshot of the data is given in Figure 2.1, which shows the regional distribution of per-capita payroll tax revenues in 1929.

In this article, we describe how we compiled the database. We also describe the main features of the German tax system of the 1920s, that is, its state after the fundamental Erzberger reforms (Bach, 2018). We focus on tax exemptions, the tax schedules, and the place and timing of tax collection. These characteristics are

[1] For research on totalitarianism and persecution, see Voigtländer and Voth (2012), Waldinger (2016), Becker and Pascali (2019), Galofré-Vilà et al. (2021). For research on the effects of war, see Brakman et al. (2004), Bosker et al. (2008), Akbulut-Yuksel (2014), Wolf and Caruana-Galizia (2015), Waldinger (2016). For research on forced migration, see Bauer et al. (2013), Chevalier et al. (2018). For research on reconstruction and reunification, see Fuchs-Schündeln and Schündeln (2005), Fuchs-Schündeln (2008), Redding and Sturm (2008), Redding et al. (2011), Burchardi and Hassan (2013).

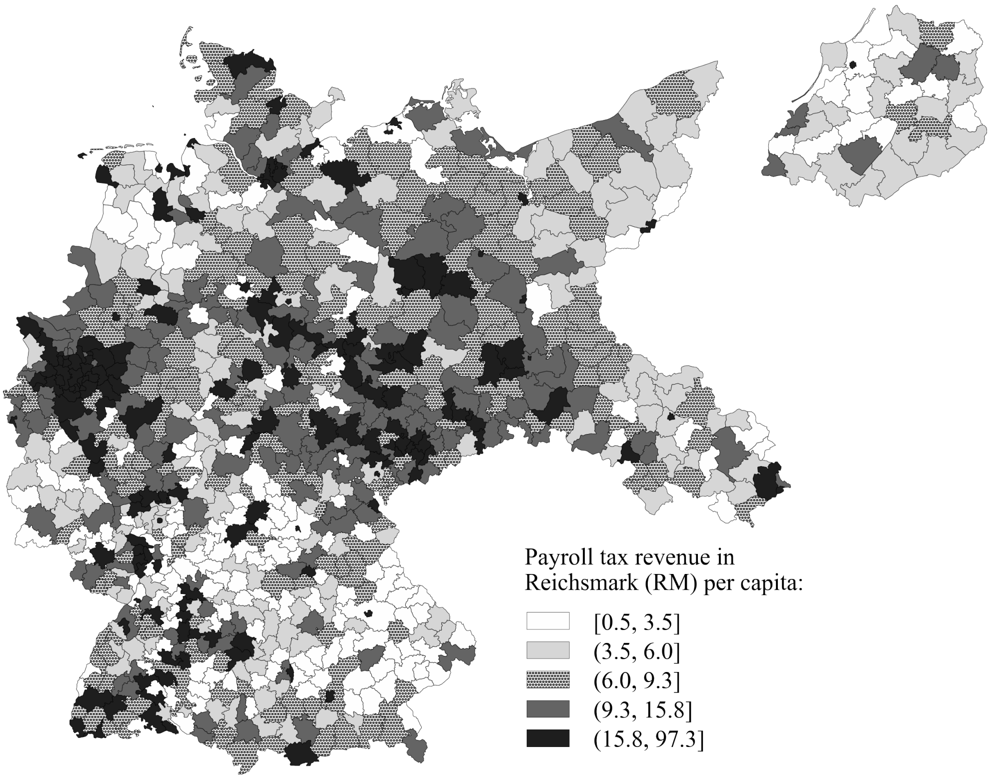


Figure 2.1: Regional distribution of per-capita payroll tax revenues in Germany in 1929.
Note: The map shows, for each tax district, the tax revenues from the payroll tax in Reichsmark (RM) per inhabitant of the tax district in the fiscal year 1929. 1 RM equals approximately 4.34 US dollar in 2020 prices based on a RM-euro conversion rate of 3.8 provided by Deutsche Bundesbank (2021) and the average euro-US dollar exchange rate in 2020 of 1.1422 provided by Eurostat (2021). The payroll tax was a withholding tax and revenues are reported in the tax district where an employer was headquartered. Tax districts are categorized by quintiles of the depicted variable such that each category comprises 20 percent of the tax districts. Source: Statistisches Reichsamt (1941). Computations are our own.

critical to the extent to which tax revenue statistics are informative about gross indicators such as regional turnover or household income.[2] Ideally, regional tax revenues correlate closely with gross indicators across regions, so that tax revenues can be used as a substitute in regional analyses when gross indicators are not available.

Similar to earlier studies, this article finds that tax exemptions were relatively extensive in the German tax system of the 1920s (Jacobi, 1958; Hoffmann and Müller, 1959). For example, in 1926, about half of the tax units of the payroll tax were exempt because their incomes were too low. Extensive tax exemptions reduce the informative value of tax revenue statistics, since the exempt part does not result

[2]Throughout this article, we use the term gross indicator according to the following logic: tax liability = tax rate × (gross indicator − deductions). Depending on the tax, the gross indicator is income from employed labor, total household income, corporate profit, turnover, or net wealth, where net wealth refers to assets less liabilities.

in tax revenues. Unfortunately, the magnitude of the exempt part is not available in historical tax statistics, so we are unable to examine to what extent regional tax revenues provide a distorted picture of regional differences in income, wealth, or turnover levels.

This article finds that tax revenues correlate, for the most part, strongly and linearly with the respective tax bases at the regional level. This also holds for taxes with progressive tax schedules. Lastly, we find that tax revenues correlate closely with regional per-capita gross domestic product (GDP) estimates from Rosés and Wolf (2018, 2019). The result applies to the regional NUTS 2 level, for which the historical GDP estimates are available, making tax revenues an informative indicator of regional economic development.[3]

The results and the database contribute to several strands of literature. Several studies exist that provide historical economic indicators for small regions; primarily GDP.[4] The most detailed regional GDP estimates for the German interwar period are presented in Rosés and Wolf (2018, 2019), who estimate GDP at the NUTS 2 level for various European countries from 1900 to 2010. Our database is much more regionally detailed and contains data for a larger number of years of the interwar period. Moreover, our database contains indicators related to household income and wealth, which go beyond the realm of production statistics and offer new possibilities for analysis.

By providing geocoded tax district borders, this article also contributes to studies that retrieve geocoded borders from historical sources. The most important work for Germany was carried out by the Max Planck Institute for Demographic Research (MPIDR) and the Chair for Geodesy and Geoinformatics University of Rostock (CGG) (2011), who provide historical borders of the standard German administrative units (*Landkreise*). These borders are the basis for our work. We contribute to this literature by providing geocoded borders of another regional division, that of tax districts.

Lastly, our work relates to a body of literature that uses tax data to study income and wealth. At the national level, German income tax data were used in the early long-run income estimates from Hoffmann and Müller (1959) and were also exploited by Kuznets (1955) in his work on growth and income inequality. More recent studies that use historical German tax data include Dell (2005, 2007), Bartels

[3] NUTS 2 stands for the second level of the nomenclature des unités territoriales statistiques, a regional classification of the European Union. More information on NUTS 2 is available at https://ec.europa.eu/eurostat/web/nuts/background, last accessed March 2022.

[4] For France: France Díez-Minguela and Sanchis Llopis (2019). For the Hapsburg Empire: Schulze (2007). For Italy: Felice (2019). For Spain: Alcaide Inchausti (2003); Martínez-Galarraga et al. (2015). For Sweden: Henning et al. (2011); Enflo and Rosés (2015). For the United Kingdom: Geary and Stark (2002); Crafts (2005); Geary and Stark (2015). For Poland/Congress Kingdom of Poland: Bukowski et al. (2019); Koryś and Tymiński (2021). For Portugal: Badia-Miró et al. (2012).

(2019), Albers et al. (2020), and Bartels et al. (2021).[5] At the regional level, very few studies employ historical German tax data, so the data presented in this article provide new research opportunities.[6]

The remainder of the article is organized as follows. Section 2.2 explains how we assembled the geocoded data. Section 2.3 describes the German tax system during the interwar period. Section 2.4 evaluates different tax statistics as a proxy for GDP. Section 2.5 suggests possible extensions of the present work. The last section concludes.

2.2 Geocoded Tax Data for 1926 to 1938

2.2.1 Geocoding of Tax Districts

During the interwar period, the German Statistical Office published tax statistics for different regional entities, such as municipalities, administrative districts, and tax districts. The data on tax revenues were published in a harmonized form in Statistisches Reichsamt (1941) at the level of tax districts (*Finanzamtsbezirke*) and the larger entity of tax states (*Landesfinanzamtsbezirke*).[7] This regional division of the financial administration has not been geocoded previously.[8] The lack of geocoding limits the usability of the large amounts of regional data because without geocoding researchers cannot map the data and match them with other geocoded data sources. We therefore geocoded every tax district and tax state for each year from 1926 to 1938, taking into account annual border changes, thereby increasing the usability of the data.

For the geocoding, we relied on the following sources:

(1) A printed map of the tax districts in 1925 published in Statistisches Reichsamt (1929b).[9]

(2) Geocoded *Landkreise* for the years 1925 to 1939 from MPIDR and CGG (2011) and based on Hubatsch and Klein (1975).

[5] Further recent studies relying on tax data are Piketty (2003), Piketty and Saez (2003), Saez and Zucman (2016), Alstadsæter et al. (2019), Guyton et al. (2021).

[6] Vonyó (2012, 2018) and Becker et al. (2021) are the only studies we are aware of that use regional German tax revenue data of the interwar period.

[7] In 1937, the Nazi government renamed the *Landesfinanzamt Oberfinanzbezirk*, but in the database, we keep the original term throughout (Statistisches Reichsamt, 1941).

[8] The regional division of the financial administration differed from the "standard" administrative division that divided and still divides Germany into *Landkreise*. The latter were geocoded by MPIDR and CGG (2011).

[9] The map is of size 56.5×46.2 cm, resulting in a scale of roughly 1:2,270,000, such that 1 cm in the map corresponds to 22.7 km in reality.

(3) The gazette of the financial administration of the years 1925 to 1939, which reports all changes to the demarcation of tax districts and tax states (Deutsches Reich Reichsfinanzministerium, 1925, 1926a, 1927, 1928, 1929a, 1930, 1931, 1932, 1933, 1934a, 1935, 1936, 1937, 1938, 1939).

(4) Statistisches Reichsamt (1941), which contains a complete panel database of tax revenues for all tax districts and tax states for the years 1926 to 1938 and which also reports changes to the demarcation of tax districts.

(5) The directories of tax districts for the years 1926, 1934, and 1942 (Deutsches Reich Reichsfinanzministerium, 1926b, 1934b, 1942), which describe the demarcation of each tax district.[10]

(6) OpenStreetMap data as of 2019 to locate specific municipalities (OpenStreetMap contributors, 2019).

Geocoding consisted of the following steps (see also Section 2.7.3). First, we scanned map (1) and determined a set of geographical points that are uniquely identifiable in map (1) and a reference map—the map of the *Landkreise* in 1925 (2). Based on these points, geoinformation software (GIS) calculated the geographical location of all pixels of the scanned map, yielding a geocoded image. Next, we traced out all borders of the geocoded image to get a geocoded map in a vector format. The map contains only the tax district borders for 1925. In cases where the tax district borders corresponded to those of *Landkreise*, we used the *Landkreis* borders from the reference map, as we deemed them to be more accurate than map (1).

Based on the geocoded map for 1925, we incorporated annual border changes and dissolutions of tax districts to obtain maps for the subsequent years. These border modifications are reported in sources (3) and (4), while source (5) served for cross-checking purposes.[11] In several cases, dissolutions of tax districts consisted of simply merging two tax districts. In cases where new borders were drawn, we relied on additional information from source (3), which lists the specific municipalities that changed from one tax district to another. We drew the new borders freehand to encompass all of the municipalities listed.[12] If the redrawn borders corresponded to the borders of the underlying *Landkreise*, we used those.

[10] Although the directories were published on a yearly basis, these were the only years available to us.

[11] Most border changes are reported in both sources (3) and (4). A small number of minor border changes are only reported in source (3). We list these changes in Section 2.7.4 and incorporated them into the map if not stated otherwise.

[12] We localized the municipalities with recent maps provided by OpenStreetMap (6). Although the procedure contains a certain margin for error, we believe that the errors are reasonably small and acceptable for the analytical purposes of the maps. Germany's population is decentralized, with municipalities scattered widely across the country, reducing the extent of errors. Moreover, geocoded maps for municipalities are not available for the interwar period meaning that there is no feasible way to improve on our method.

Borders changed throughout the year, but we ensured that the yearly maps correspond to the territorial status at the *end* of the tax administration's fiscal year, which was March 31. For example, the 1926 map corresponds to the territorial status as of March 31, 1927. If a tax district was dissolved before March 31, 1927, it is not included in the 1926 map. This approach corresponds to that of source (4), which reports the tax revenue data and which is why we proceeded analogously.[13]

In the last step, we created maps for the tax states by merging all tax districts of a tax state based on information provided in source (4).

The geocoded maps contain some minor simplifications, which are already present in the printed map (1). First, the various tax districts of larger cities, such as Berlin or Munich, have been merged into a single tax district. Second, small exclaves are omitted in the maps because they mostly consisted of a single municipality or a few square kilometers of land and are presumably insignificant for regional analyses. Last, the Austrian and Czechoslovak territories that were annexed by Germany in 1938 are not included in the maps because information on tax districts in these occupied territories is lacking.

The maps are provided in the shapefile format, with one file for each year, separately for tax districts and tax states. For ease of use, we also provide Python syntaxes that convert the maps and tax revenues into a different administrative division.[14] The syntaxes convert the data according to the spatial overlap between the tax district and the new administrative division. For example, if a new district A covers 80% of the area of historical tax district B, 80% of B's tax revenues are assigned to A. The remaining 20% are assigned to the other districts that cover B's area. Researchers should check, however, whether such a conversion is appropriate for their purposes.

2.2.2 Tax Revenue Data

During the interwar period, the Statistical Office assembled extensive tax statistics that can be grouped into tax declaration (*Steuersoll*) and tax revenue (*Steuerist*) statistics. The *Steuerist* statistics for the years 1926 to 1938 were published in Statistisches Reichsamt (1941), which we digitalized for our database. For each year, tax district, and tax state, the publication contains tax revenues from five different taxes: payroll, income, wealth, corporate, and turnover tax. The sum of all taxes except the turnover tax is also included. All tax revenues are stated in absolute terms and relative to the number of inhabitants. We digitalized the data using optical

[13]Not all publications by the Statistical Office used March 31 as a reference date. Section 2.5 elaborates on this point in more detail.

[14]The syntaxes are publicly available at `https://github.com/chalbmeier/districtconversion`.

Table 2.1: Tax revenues of German tax districts from 1926 to 1938.

	Mean	Minimum	Maximum	N	Missing (%)
(A) Total tax revenue in 1000 Reichsmark (RM):					
Payroll tax (P)	1,526.2	−13.6	371,900.4	10,918	0.00
Income tax (I)	1,723.7	−198.6	412,152.2	10,918	0.00
Corporate tax (C)	830.5	−2,028.6	529,077.5	10,834	0.77
Wealth tax (W)	456.2	−195.2	91,682.6	10,911	0.06
Sum of P, I, C, and W	4,529.9	3.2	1,375,542.5	10,918	0.00
Turnover tax	1,908.5	1.5	446,557.8	10,918	0.00
(B) Tax revenue in Reichsmark (RM) per capita:					
Payroll tax	10.0	−0.3	102.6	10,917	0.01
Income tax	13.5	−3.5	168.0	10,917	0.01
Corporate tax	4.6	−12.9	267.2	10,833	0.78
Wealth tax	3.7	−2.0	40.1	10,911	0.06
Sum of P, I, C, and W	31.6	0.6	475.2	10,917	0.01
Turnover tax	17.1	1.4	160.5	10,917	0.01

Note: The table shows tax revenues for 10,918 tax district-year observations. In 1926, there are 901 tax districts in the data. By 1938, the number drops to 811. These numbers do not correspond to the actual number of tax districts in the German Empire, since for some tax districts only joint data are available. 1 RM equals approximately 4.34 US dollar in prices of 2020 (Deutsche Bundesbank, 2021; Eurostat, 2021). All values are nominal. Source: Statistisches Reichsamt (1941). Computations are our own.

character recognition (OCR) software. Further manual cross-checks ensured that all data points were digitalized without errors.[15]

The final dataset contains 10,918 tax district-year observations, 331 tax state-year observations, and 13 observations for the national level. Each tax district and tax state is identified by unique identifier variable, *id*. The *id* changes over time if the demarcation of the tax district (tax state) changes. That is, an *id* always refers to the same demarcation.[16] The regional level is identified by the *level* variable. A full list of variables is given in Table 2.4 in Section 2.7.1.

Table 2.1 provides tax revenue statistics of the tax districts across all years. Income tax revenues are on average the highest, and wealth tax revenues the lowest. All taxes, except the turnover tax, have some negative values. These are generally rare and most common in the corporate tax statistics. The original source gives

[15]For cross-checking, we took advantage of the fact that the historical tables state the tax revenues in absolute and per-capita terms and also contain the sum over the tax types. In this way, each number was indirectly scanned several times, allowing efficient cross-checking by calculus. In this process, we corrected a few misprints in the original source.

[16]At the national level, the *id* and borders change in 1935 due to the incorporation of the Saarland region, but they do not change in 1938, despite the annexation of Austria and parts of Czechoslovakia. We did not incorporate the annexations of 1938 because we lacked information on the new tax district borders. In addition, there are no tax revenue data in Statistisches Reichsamt (1941) for Austria. However, the source does contain payroll and income tax revenues for the Czechoslovakian tax district Rokitnitz. We kept these revenues in the database for completeness and included them in the national totals as well.

no explanation for negative values, but they are more common during economic recessions, suggesting that they correspond to tax refunds. Missing values are rare and most frequent in the per-capita corporate tax statistics, where 0.78 percent of values are missing.

2.3 The German Tax System of the Interwar Period

In this section, we briefly describe the German tax system in 1926—the first year in our data. In particular, we examine tax exemptions, the tax base, and the tax schedule because they affect how well tax revenues correlate with regional gross indicators, such as gross household income or wealth. A close correlation can be beneficial for many empirical applications because data for many of these gross indicators are not available for the interwar period at a small regional level, so that tax revenues may be a good substitute.

The analysis relies on additional data from the *Steuersoll* (tax declaration) statistics published by the Statistical Office. These statistics contain regional information on the number of tax units, the tax liability, and, depending on the tax, information on the tax base or related measures. These statistics help to assess how tax revenues relate to regional gross indicators, as data on gross indicators are lacking. Apart from that, the use of the *Steuersoll* statistics highlights some reporting differences with the *Steuerist* (tax revenue) statistics of the database, which helps researchers evaluate which statistical source is most appropriate for their purposes.

Some reference figures will provide fuller context for the analysis. In 1925, the year of the most recent census, Germany had 62.4 million inhabitants, and the labor force consisted of 37.0 million people—59.3 percent of the population. The estimated yearly gross national income was 71.2 billion Reichsmark (RM) or 1,141.0 RM per capita, which equals, in today's prices, approximately 4,952.4 US dollars (Ritschl and Spoerer, 1997; Statistisches Reichsamt, 1927).[17]

2.3.1 Payroll Tax

The payroll tax was a withholding tax on income from employed labor and certain rents.[18,19] Every month or week, employers withheld 10 percent of their employees'

[17] 1,141.0 RM equal 4,952.4 US dollars in 2020 prices based on a RM-euro conversion rate of 3.8 provided by Deutsche Bundesbank (2021) and the average euro-US dollar exchange rate in 2020 of 1.1422 provided by Eurostat (2021).

[18] Strictly speaking, the payroll tax was not a stand-alone tax, but part of the income tax code (see Section 2.3.2).

[19] Employees were not the only group who paid the payroll tax. Some individuals known as "berufslose Selbstständige, die von Pension leben" were liable, too.

income and transferred it to the tax authority (Deutsches Reich, 1926c, §§ 69, 70).[20] Each employee benefited from a tax exemption of 1,200 RM per year, and there was an additional exemption for employees with children. Male employees also received an additional tax exemption for being married (Statistisches Reichsamt, 1929a).[21] A result of these tax exemptions was that a large share of employees were exempt from the payroll tax. In 1926, the *Steuersoll* statistics count 12.5 million individuals who paid the tax, 10.4 million individuals who were exempt because their yearly labor incomes were below 1,200 RM, and 0.4 million individuals who were exempt due to family exemptions.[22] These figures exclude individuals who, besides paying the payroll tax, had to declare their income (see Section 2.3.2), so that the true number of payroll taxpayers is underestimated by a small degree (Statistisches Reichsamt, 1929a).[23,24] All in all, the numbers suggest that about 45 percent of individuals with income from employed labor were exempt from payroll tax. The volume of exempt income, however, is unknown.

The considerable size of tax exemptions may limit the usability of payroll tax revenue data for regional analyses. Tax exemptions result in a portion of labor income being untaxed and not reflected in the payroll tax revenues. Due to regional differences in the level of labor income, the share of untaxed income possibly varies by region, which can distort regional comparisons based on tax revenue data. On the other hand, the available data on the number of exempt individuals itself gives an indication of the prevalent income levels within a specific region and may be used to estimate the missing incomes.

We present the number of exempt payroll taxpayers relative to all payroll taxpayers of a tax district in Figure 2.2. Across tax districts, the average share of exempt taxpayers is 51.2 percent, and the standard deviation is 15.6 percentage points. The percentage is lower in many West German regions (the industrial Ruhr region, along

[20]Small firms with three employees or fewer used another system based on tax stamps that had to be purchased in advance (Rinner, 1929; Statistisches Reichsamt, 1929a).

[21]The income tax law states that employees were granted a tax exemption if they had a "wife" ("Ehefrau") (Deutsches Reich, 1926c, § 70). This begs the question of whether working married women were also granted a tax exemption. Unfortunately, we could not find any source clarifying this.

[22]Not every individual with yearly labor incomes below 1,200 RM is excluded from the *Steuersoll* statistics. There are 3.26 million individuals with lower yearly incomes included and classified as non-exempt. These are individuals who paid the payroll tax in some weeks or months of the year, but their yearly labor income did not surpass 1,200 RM due to unemployment or sickness (Statistisches Reichsamt, 1929a).

[23]The exact number of individuals who are excluded because they declared their income is unknown. However, in 1926, there were only 168,036 households who declared income from employed labor. We therefore believe the underestimation to be small (Statistisches Reichsamt, 1931a).

[24]This is an important difference between tax declaration (*Steuersoll*) and tax revenue (*Steuerist*) statistics. In the *Steuersoll* statistics, individuals who paid the payroll tax and who had to declare their income are assigned to the declared income tax statistics. In the *Steuerist* statistics, however, such individuals are assigned to the payroll tax statistics (Statistisches Reichsamt, 1941).

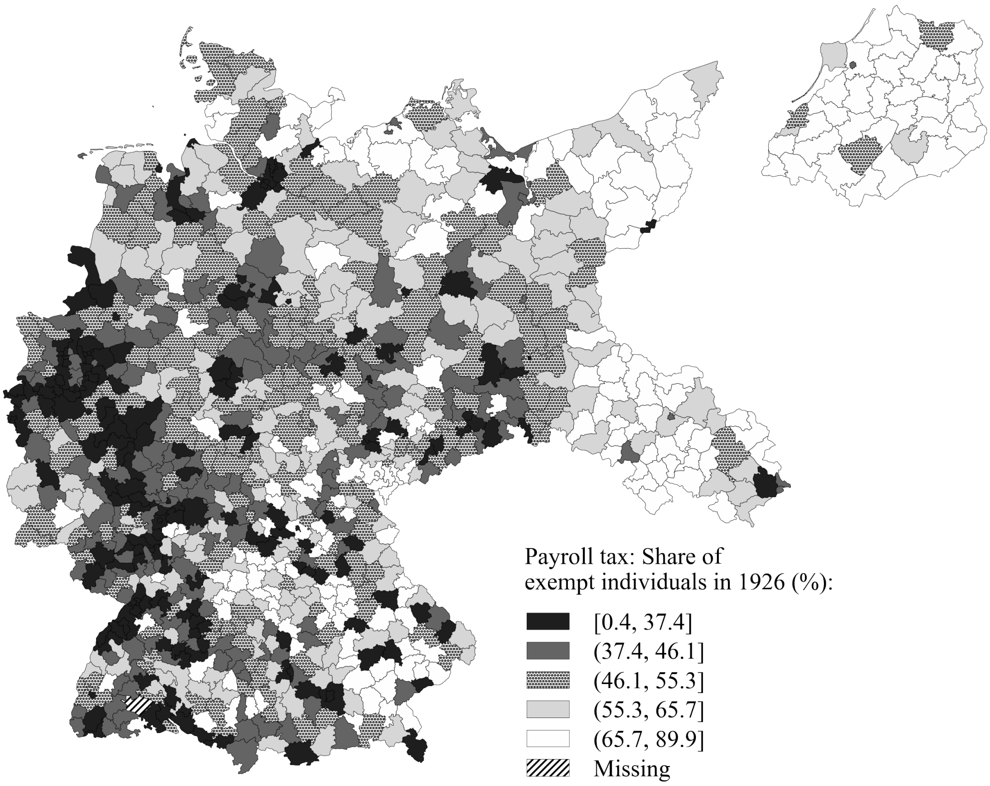
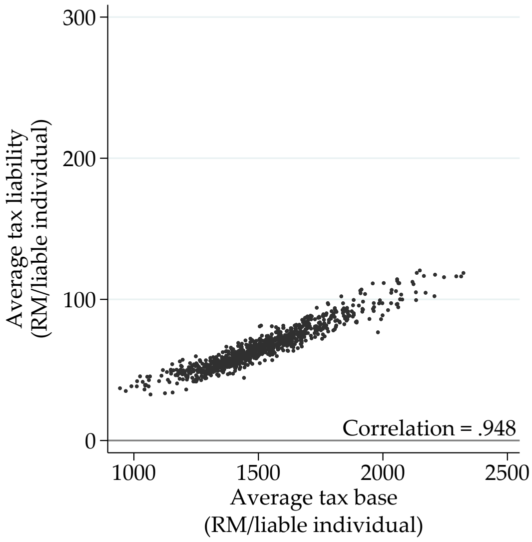


Figure 2.2: Regional distribution of the share of exempt payroll taxpayers in 1926.

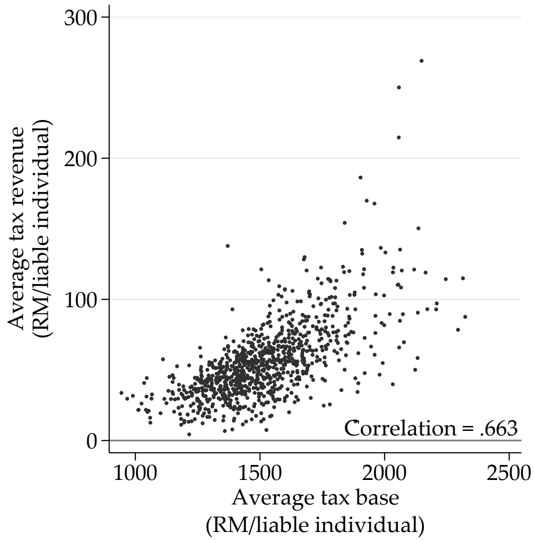
Note: The map shows, for each tax district, the share of exempt individuals relative to all individuals liable for payroll tax except those who had to declare their income. Exempt individuals are all individuals who did not pay the payroll tax due to a yearly lump-sum exemption of 1,200 RM (*Unbesteuerte*). We do not include those 0.4 million individuals who were exempt due to family exemptions (*Steuerbefreite*) in the group of exempt individuals because their labor incomes are recorded in the statistics. Individuals are assigned to the tax district where they reside. Tax districts are categorized by quintiles of the depicted variable, such that each color group comprises 20 percent of the tax districts. Source: Statistisches Reichsamt (1929a). Calculations are our own.

the Rhine River, and around Stuttgart), along the southern border, as well as in large cities like Dresden, Hamburg, and Munich, and in scattered parts of central and eastern Germany (Hanover, Kassel, the Harz Mountains, Lusatia), suggesting that labor incomes were nominally higher in these regions.

Besides tax exemptions, the tax schedule is another important element affecting the relation between tax revenues and regional gross indicators. In the case of the payroll tax, however, the tax schedule is linear and we do not expect a distortion. This is confirmed by the data presented in Figure 2.3. The figure compares regional payroll tax bases, tax liabilities, and tax revenues. Payroll tax bases and tax liabilities are taken from the *Steuersoll* statistics (Statistisches Reichsamt, 1929a), while tax revenues are from the *Steuerist* statistics in our database. The payroll tax bases are defined as the yearly gross labor incomes of non-exempt individuals less a lump sum exemption of 480 RM made for each individual, except for those with labor incomes



(a) Tax base and tax liability



(b) Tax base and tax revenue

Figure 2.3: Indicators of the payroll tax for German tax districts in 1926.

Note: Each dot represents a tax district and shows the payroll tax base, payroll tax liability, and revenue of a tax district averaged over all individuals within the district who paid the payroll tax except for those individuals who had to declare their income. All data are assigned to the tax district where the individual resides, except for revenues, which are assigned to the tax district where the individual's employer is headquartered. The tax base is defined as yearly gross income from employed labor and certain pensions during the calendar year 1926. A lump sum exemption of 480 RM was deducted for each individual, except for individuals with incomes below 1,200 RM, for reporting reasons by Statistisches Reichsamt (1929a). The tax liability is the amount withheld by the employer in 1926, taking into account all exemptions. The tax revenue corresponds to all revenues of a tax district received from the payroll tax during the fiscal year 1926, including payroll taxes from individuals who declared their income. Tax base and tax liabilities are taken from the *Steuersoll* statistics, while tax revenues are taken from the *Steuerist* statistics. Data for 901 tax districts. Source: Statistisches Reichsamt (1929a), Statistisches Reichsamt (1941). Calculations are our own.

below 1,200 RM. The lump sum exemption was made by the Statistical Office for statistical reasons, so that the tax bases shown differ to some degree from the actual tax bases.[25] Tax liabilities correspond to the taxes withheld by the employer. To account for differences in size between tax districts, we average all statistics over the number of payroll taxpayers within the district, excluding those who had to declare their income and are missing the data.

Figure 2.3a shows that there is a clear linear relationship between the average tax base and tax liability, with a strong linear correlation of 0.948. The correlation is not perfect probably because the reported tax bases do not correspond to the actual tax bases to which the ten percent tax rate was applied. Tax revenues themselves correlate less closely with the tax base (Figure 2.3b, correlation = 0.663), which does

[25]Unfortunately, the data sources do not contain either gross labor income or the actual tax base (Statistisches Reichsamt, 1929a).

not necessarily invalidate tax revenues as a good proxy for the tax base. The lower correlation likely stems from various differences in reporting standards of the two statistics. First, tax bases and liabilities refer to the calendar year 1926, while tax revenues refer to the fiscal year 1926.[26] Second, tax bases and liabilities exclude individuals who had to declare their income for income taxation, while tax revenues do not. Third, tax bases and liabilities refer to the tax district where the taxpayer resides, while revenues refer, in the case of the payroll tax, to the tax district where the taxpayer's employer is headquartered (Statistisches Reichsamt, 1941). Especially the latter two points indicate that tax revenues might be the preferred indicator depending on the empirical application.

Aside from differences in reporting standards, tax revenues are different from payroll tax liabilities because tax liabilities contain overpaid taxes that could later be reclaimed by employees. We estimate the effect of tax refunds to be rather small, as in 1926 only 4.6 percent of the total payroll tax liability was refunded (Statistisches Reichsamt, 1929a; Rinner, 1929).[27]

In conclusion, the data indicate that regional payroll tax revenues approximate the payroll tax bases recorded in the *Steuersoll* statistics very well. However, there is a large share of exempt individuals and a possibly large share of exempt labor income. This may introduce a certain distortion when payroll tax revenues are used in regional analyses to approximate regional labor incomes. Researchers may account for such distortion by, for example, controlling for the number of exempt individuals in regression analyses or by combining data on the number of exempt individuals and exemption thresholds to approximate exempt labor income.

2.3.2 Tax on Declared Income

The tax on declared income was a tax on various types of individual and household income and was collected after filing a tax declaration.[28] The tax applied to

[26]The fiscal year, for example 1926, went from April 1, 1926, to March 31, 1927 (Hacker, 2013, p.165).

[27]Employees were entitled to a tax refund if they had no earnings during a certain period of the year, for example, due to unemployment or illness. These employees may have paid too much payroll tax in the months they worked, as the payroll tax was deducted monthly (weekly) based on monthly (weakly) earnings and exemption thresholds, but ultimately annual exemption thresholds applied. In the case of earnings loss, the sum of the monthly exemptions did not necessarily correspond to the annual exemption, so that the employee was entitled to a residual exemption and thus a payroll tax refund. This payroll tax refund only applied if employees had *no* earnings during a certain period of the year. If employees had fluctuating earnings, and therefore paid too much payroll tax, they were not entitled to a refund (Rinner, 1929, pp.102-109).

[28]The law explicitly defined the following types of taxable income (Deutsches Reich, 1926c, § 6): (1) profits from agriculture and forestry; (2) profits from trade and business (*Gewerbe*), which includes income as a liable partner in a corporation; (3) profits from other self-employed work; (4) income from employed labor; (5) capital income; (6) income from rental of immovable property as well as agricultural inventory and mobile business property, as well as legal rights; (7) other recurring

individuals who had an income higher than 8,000 RM per year—including income from employed work, and net of certain deductions—or whose income came from sources that required bookkeeping (Rinner, 1929; Deutsches Reich, 1926c, § 61). The tax applied to these individuals because incomes above 8,000 RM were subject to a progressive tax rate that was higher than the ten percent withholding tax on incomes from employment (section 2.3.1) and capital investments.[29] Moreover, income from other sources, such as self-employment and property rental, ought to be taxed as well.

The income tax schedule featured various progressive elements. Individuals with yearly incomes of 1,300 RM or below were not taxed. Individuals with incomes not higher than 10,000 RM could deduct 720 RM and make additional deductions for children. The tax rate itself was progressive, as well, starting at 10 percent and reaching 40 percent for the highest tax bracket of incomes above 80,000 RM.[30] Income obtained from shares in limited liability companies was taxed at a lower rate as it was also subject to corporate tax (see Section 2.3.3). Spouses had to declare their income jointly by summing up the income of both spouses and treating it as if it had been received by a single individual. In the case of this joint taxation, additional deductions applied (Deutsches Reich, 1926c, §§ 22, 52-57).

The declared income tax was collected throughout the year. Taxpayers were required to pay the tax in form of quarterly payments based on the previous year's tax burden before submitting the tax declaration. Moreover, the ten percent withholding tax on capital income was already deducted at source in the moment of realization. After the end of the fiscal year, liable individuals submitted the tax declaration, and a final payment (or reimbursement) was made based on the actual income during the fiscal year (Deutsches Reich, 1926c, §95, §102) (Deutsches Reich, 1926c, pp.180-181).[31]

A small share of German households were liable for declared income tax. In 1926, 3.8 million households declared their income, of which 2.9 million were deemed

earnings, which include various types of pensions; (8) other income (*sonstige Leistungsgewinne*), including gains from speculative transactions (*Spekulationsgeschäfte*). One-off gains such as gifts, inheritances, and lottery gains, were not subject to income taxation. Depending on the type of income, different costs could be deducted.

[29] In the data, tax revenues of the 10 percent withholding tax on capital income are included in the declared income tax revenues, which is why we do not treat this tax in a separate section.

[30] In 1926, the tax rates and tax brackets of the declared income tax were: 10 percent on the first 8,000 RM per year, 12.5 percent on the next 4,000 RM, 15 percent on the next 4,000 RM, 20 percent on the next 4,000 RM, 25 percent on the next 8,000 RM, 30 percent on the next 18,000 RM, 35 percent on the next 34,000 RM, 40 percent on larger amounts (Deutsches Reich, 1926c, §55).

[31] Note that the fiscal year did not necessarily correspond to the calendar year. For individuals who kept accounts, the fiscal year corresponded to their accounting year, and for farmers, it went from July 1 to June 30 (Deutsches Reich, 1926c, §10). This is important for the *Steuersoll* statistics, which, in the case of the year 1926, comprise information on all tax units whose fiscal year ended in 1926 (Statistisches Reichsamt, 1931a).

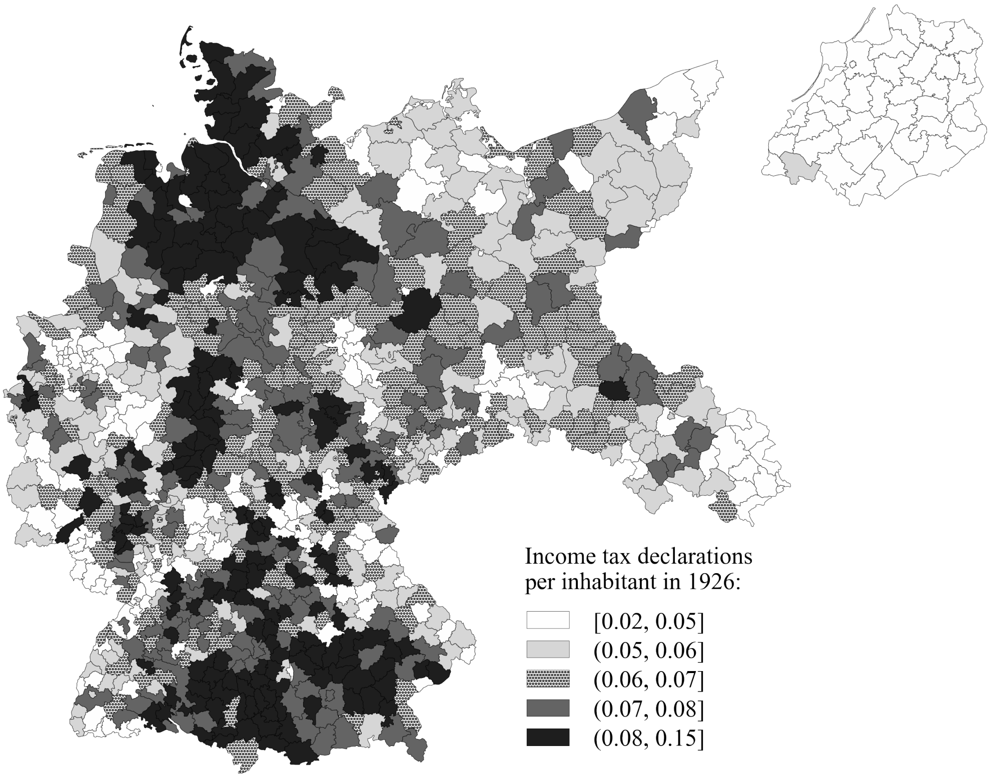


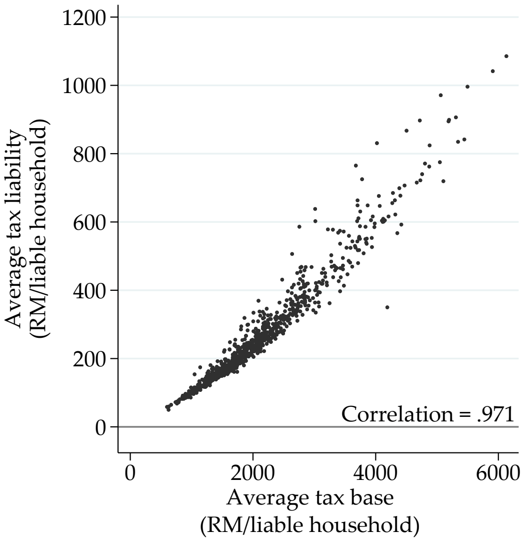
Figure 2.4: The regional distribution of income tax declarations per inhabitant in 1926.
Note: The map shows, for each tax district, the number of income tax declarations divided by the number of inhabitants in the tax district. Income was declared by households. Income declarations are assigned to the tax tax district where the declaring household resided. Tax districts are categorized by quintiles of the depicted variable, such that each color group comprises 20 percent of the tax districts. Source: Statistisches Reichsamt (1931a, 1941). Calculations are our own.

liable for income tax. Many of these were probably farmers and owners of small- to medium-sized businesses, since 63.7 percent of households declared income from trade and business and 36.3 income from agriculture and forestry.[32] The average income per declaration was 3,360.3 RM and a 45.6 percent of declarations did not exceed 1,500 RM of income (Statistisches Reichsamt, 1931a).

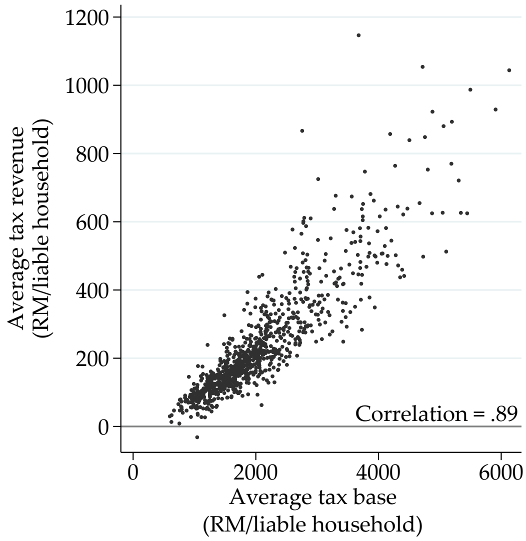
Figure 2.4 presents the regional distribution of income tax declarations per inhabitant.[33] Income tax declarations were particularly common in northern and southern Germany, and in a central region covering today's federal states of Hesse and Thuringia. Notably, income tax declarations have a regional distribution similar

[32]The numbers for the other income types are: other self-employed work: 5.2 percent; declared employed labor income: 4.5 percent; declared capital income: 8.0 percent; property rental: 24.0 percent; other recurring earnings and income: 2.1 percent (Statistisches Reichsamt, 1931a).

[33]A better indicator of the prevalence of income tax declarations within a region is the number of income tax declarations per *household*, as households filed the declarations. We lack the data on the number of households in a tax district, but the data presented should give a good indication as to the prevalence of income tax declarations.



(**a**) Tax base and tax liability



(**b**) Tax base and tax revenue

Figure 2.5: Indicators of the declared income tax for German tax districts in 1926 (part I).

Note: Each dot represents a tax district and shows the income tax base, income tax liability, and income tax revenue of that district averaged over all households liable to declared income tax within a district. The income tax base corresponds to income net of expenses, costs, the tax-exempt part of income, and deductions for family members. Income tax bases (*Einkommen nach Abzug des steuerfreien Einkommensteils und der Ermäßigung für die Familienangehörigen*) and tax liabilities (*festgesetzte Steuer*) are taken from the *Steuersoll* statistics, while tax revenues are taken from the *Steuerist* statistics. All data are assigned to the tax district in which a household resided. Data for 901 tax districts. Source: Statistisches Reichsamt (1931a), Statistisches Reichsamt (1941). Calculations are our own.

to that of wealth tax declarations (see Section 2.3.5), pointing to the fact that the generation of certain types of income, such as income from business or agriculture, requires some level of capital. Moreover, the income and the wealth tax both had a tax threshold, and households that surpassed one threshold may have been more likely to exceed the other.

The effects of the progressive tax schedule are highlighted by additional data presented in Figure 2.5. Panel (a) shows a scatter plot of the declared income tax base and the tax liability—averaged over the number of liable households—at the tax district level. The plot is slightly curved, reflecting the progressivity. The linear correlation between the two variables is very high (correlation = 0.971). Similarly, Panel (b) shows that tax revenues are also highly correlated with the tax base (correlation = 0.890), suggesting that both, tax liabilities and tax revenues, are a good proxy for the tax base at the regional level.[34] Figure 2.16 in the Appendix

[34]Similar to the payroll tax statistics, there are reporting differences between the declared income *Steuersoll* and *Steuerist* statistics. Most importantly, the declared income *Steuersoll* statistics contain the incomes subject to the payroll tax of those employees who had to declare their income. The

provides additional data on declared income, which differs from the tax base in that the tax-exempt part of income and deductions for family members are not subtracted. Declared incomes also correlate highly with tax liabilities and revenues at the regional level, strengthening the notion that income tax revenues are good proxy for incomes to the extent that they were declared.

In summary, all this suggests that declared income tax statistics are certainly important if one wants to approximate regional income levels, because they capture various types of income that go beyond labor income. However, the data also make it clear that the declared income tax statistics cover only a small part of the income distribution and ideally should be used together with the payroll tax statistics, which better capture the incomes of the large majority of workers.[35] Apart from that, the data presented suggest that tax exemptions and the progressive tax schedule have a

corresponding tax liabilities are included as well. In contrast, the *Steuerist* statistics assign the corresponding tax revenues to the payroll tax statistics. Further, the *Steuersoll* statistics include incomes subject to the ten percent withholding tax on capital income and the corresponding liabilities only if taxpayers had to declare their income. The declared income *Steuerist* statistics contain the revenues from this tax in their entirety. In terms of regional allocation, the declared income *Steuersoll* and *Steuerist* statistics both assign the data to the tax district of residence, which should be beneficial for the correlation. An exception is the withholding tax on capital income, which is assigned in the *Steuerist* statistics to the tax district of the source, for example, the headquarter of the borrower (Statistisches Reichsamt, 1931a, 1941; Hacker, 2013). In addition, the declared income tax revenues of a given fiscal year differ slightly from the underlying incomes and *Steuersoll* statistics of the same period due to the way the income tax was collected (Statistisches Reichsamt, 1941). Taxpayers were required to pay the income tax liability during the current fiscal year on a quarterly basis and based on the previous fiscal year's tax liability. A final payment (or reimbursement) was made based on the actual income of the current fiscal year after the taxpayers submitted the tax declaration (Deutsches Reich, 1926c, §§ 95, 102, pp.180-181). Moreover, a taxpayer's fiscal year did not necessarily correspond to the calendar year or to the fiscal year of the tax administration, which is the reference for the tax revenues in our database. For individuals who kept accounts, the fiscal year corresponded to their accounting year, and for farmers, it went from July 1 to June 30. For all other taxpayers, the fiscal year corresponded to the calendar year (Deutsches Reich, 1926c, § 10). Besides this, the withholding tax on capital income, which is included in the declared income tax revenue statistics, was already deducted at source in the moment of realization, which improves the temporal congruence of incomes and tax revenues (Deutsches Reich, 1926c, §§ 83, 86). In the data shown, the declared income *Steuersoll* statistics contain all tax units whose fiscal year ended in 1926. The tax revenue data correspond to all revenues in the tax administration's fiscal year 1926.

[35]One has to keep in mind, however, that it is not straightforward to merge payroll and declared income statistics, as the tax unit of the declared income tax is the household, while that of the payroll tax is the individual. For the national level, merged data are available for the years 1926, 1928, 1932, 1934, and 1936 (Statistisches Reichsamt, 1939b; Bartels, 2019). Regarding the regional level, one has to keep in mind that the *Steuersoll* statistics for the payroll and declared income tax both assign the data to the district where the individual resides. In contrast, the *Steuerist* statistics assign the payroll tax revenues to the tax district in which the individual's employer is headquartered, and the declared income tax revenues to the tax district where the individual resides (Statistisches Reichsamt, 1941).

very modest effect on the regional correlation of declared incomes and tax revenues, which is beneficial for regional approximations of income levels.

2.3.3 Corporate Tax

The corporate tax was a tax for corporations and its tax base was corporate income less costs (profit).[36] Corporate income and costs were defined analogously to individual income in the declared income tax code, although some modifications existed. For instance, corporations were allowed to reduce their tax base by carrying forward past losses, a possibility that in 1929 was also introduced for non-corporate businesses (Deutsches Reich Reichsfinanzministerium, 1929b; Statistisches Reichsamt, 1941). Corporate income distributed to shareholders (dividends) was taxed, as well, meaning that it was taxed twice, by the corporate and the income tax. Universal tax exemptions for profits below a certain threshold, as in the case of the payroll or declared income tax, did not exist for most types of corporations (Desens, 2011; Deutsches Reich, 1926a, §§ 2-17, 26).

The corporate tax rate was for most types of corporations fixed at 20 percent. A reduced progressive tax rate applied for small limited liability companies and commercial cooperatives, easing the burden of double taxation.[37] Further, a reduced rate of 10% applied to certain types of civil law partnerships and commercial cooperatives as well as incomes earned in foreign countries.[38]

Similar to the income tax, corporations had to pay their tax liability in the form of quarterly advance payments based on their last year's liability. A levy on capital income was already deducted at source at the moment of realization. A final payment

[36]The corporate tax law (Deutsches Reich, 1926a, §§ 2-9) and the Statistical Office (Statistisches Reichsamt, 1931a) distinguished between business corporations (*Erwerbsgesellschaften*), civil law corporations (*Körperschaften und Vermögensmassen des bürgerlichen Rechts*), and statutory corporations (*Betriebe und Verwaltungen von Körperschaften des öffentlichen Rechts*). Among the business corporations, common legal entities were joint-stock companies (*Aktiengesellschaft*, *Kommanditgesellschaft auf Aktien*), limited liability companies (*Gesellschaften mit beschränkter Haftung*), and cooperatives (*Genossenschaften*). Partnerships, such as the *offene Handelsgesellschaft* and the *Kommanditgesellschaft*, were not liable for corporate taxation.

[37]The reduced progressive tax rate applied to *Gesellschaften mit beschränkter Haftung* and *Erwerbsgenossenschaften*. "Small" refers to limited liability companies and commercial cooperatives whose share capital (*Stammkapital*), deposited shares (*Einlagen*), or net wealth did not exceed 50,000 RM (Deutsches Reich, 1926a, § 21). In addition to tax rate reductions, shareholders of limited liability companies with a joint profit of 20,000 RM or less could exempt 500 RM of dividends from their personal income record (Desens, 2011).

[38]The reduced tax rate applied to certain types of civil law corporations and commercial cooperatives (*Körperschaften und Vermögensmassen bürgerlichen Rechts*, *Versicherungsvereine*, *Erwerbsegenossenschaften, die einem Revisionsverband angeschlossen sind*), and certain banks (*Privatnotenbanken*, *Hypothekenbanken und Schiffsbeleihungsbanken unter Staatsaufsicht*, *gemischte Hypothekenbanken*) (Deutsches Reich, 1926a, § 21).

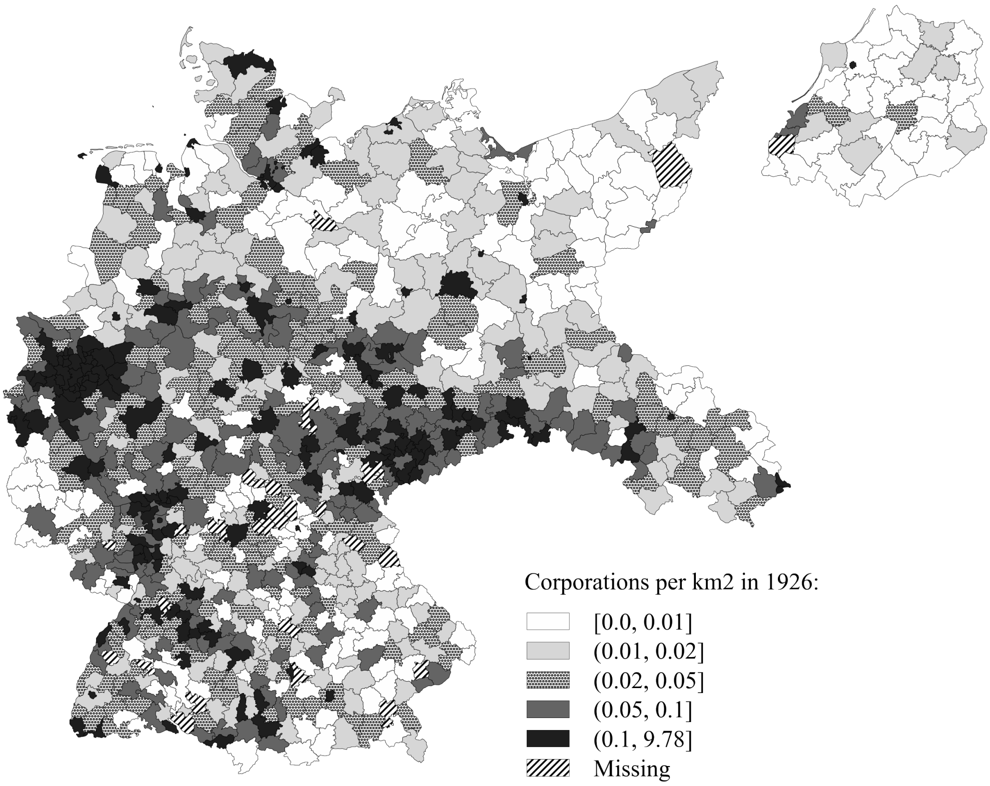
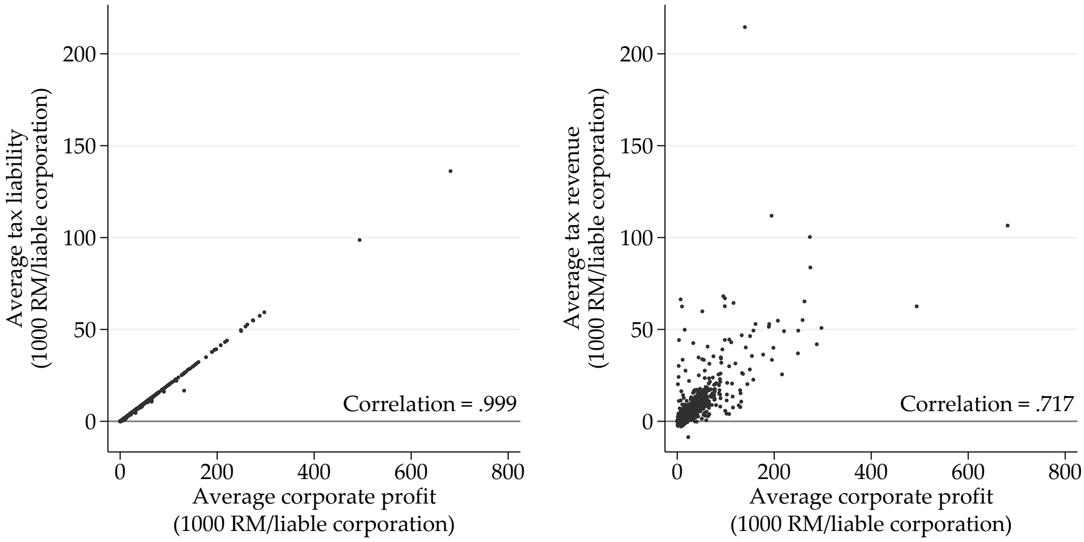


Figure 2.6: Regional distribution of corporations liable for corporate tax per km^2 in 1926.
Note: The map shows, for each tax district, the number of corporations liable for corporate tax in 1926 divided by the area of the district. All data are assigned to the tax district of the corporation's headquarters. Tax districts are categorized by quintiles of the depicted variable, such that each color group comprises 20 percent of the tax districts. Source: Statistisches Reichsamt (1931a). Calculations are our own.

(or reimbursement) after the yearly tax assessment settled the remaining liabilities (Deutsches Reich, 1926a, § 24).[39]

The reach of corporate taxation was, in terms of tax units and tax base, relatively small. In 1926, the Statistical Office registered 36,102 corporations fully liable for corporate taxation, of which 87.01 percent were limited companies or cooperatives, while the remaining 12.99 percent were either civil law of statutory corporations (Statistisches Reichsamt, 1931a). The total corporate income liable for corporate taxation was 1.8 billion RM and lower than gross income liable for the declared income tax (12.0 billion RM) or the payroll tax (20.6 billion RM), suggesting that most income was created by natural persons rather than corporations (Statistisches Reichsamt, 1929a, 1931a).

[39]Similar to the income tax, a corporation's fiscal year did not necessarily correspond to the calendar year and depended on their accounting year. In case corporations made profit from forestry or agriculture, the fiscal year went from July 1 to June 30 (Deutsches Reich, 1926a, § 12).



(a) Corporate profit and tax liability **(b)** Corporate profit and tax revenue

Figure 2.7: Indicators of the corporate tax for German tax districts in 1926.

Note: Each dot represents a tax district and shows the corporate profit, corporate tax liability, and corporate tax revenue of that district averaged over all corporations liable for corporate taxation within a district. All data are assigned to the tax district in which the headquarters of a corporation was located. Profits and liabilities are taken from the *Steuersoll* statistics, while tax revenues are taken from the *Steuerist* statistics. Data for 875 tax districts with non-missing values in figure (a) and 849 tax districts in figure (b). Source: Statistisches Reichsamt (1931a), Statistisches Reichsamt (1941). Calculations are our own.

Corporations were primarily located in regions with high population density (Figure 2.6), such as the western Ruhr region, the mountainous regions in the East, along the Rhine River in the Southwest, and in the large cities. The correlation between population density and corporation density—measured as corporations per square kilometer—is, at the level of tax districts, 0.935, showing that corporations were an urban phenomenon. In addition, Figure 2.15 in the Appendix shows the number of corporations per inhabitant, providing a similar but slightly more fragmented pattern of the spatial distribution of corporations.

The linear tax rate of the corporate tax leads to a nearly perfect correlation between the corporate profit and the tax liability at the tax district level (Figure 2.7a, correlation = 0.999). The slope coefficient between both is 0.199, corresponding closely to the standard tax rate of 20 percent, suggesting that existing tax rate reductions were quantitatively unimportant. Corporate tax revenues, on the other hand, correlate less with profits (Figure 2.7b). The correlation is 0.717 and the slope coefficient is 0.213. One reason for the lower correlation is probably that *Steuersoll* statistics, which provide profits and liabilities, and *Steuerist* (tax revenue) statistics refer to different time periods. The *Steuersoll* statistics include all corporations whose accounting year ended in 1926, and the accounting year could differ by corporation (Statistisches Reichsamt, 1931a). The *Steuerist* statistics include all tax revenues made during the fiscal year 1926, which ended on March 31, 1927. Further, corporations could defer tax payments, which they may have done to a greater extent during the 1926 recession, separating both statistics further (Statistisches Reichsamt, 1941).

In conclusion, we consider corporate tax statistics to be a valuable complement to payroll and declared income tax statistics within the set of available indicators for regional analyses. Although the base was significantly smaller than that of the payroll tax and declared income tax, corporate profits constituted an important part of income earned. A drawback for analytical purposes is that loss carryforward existed, so that it is unclear to what extent tax statistics correspond to actual profits earned in a given year. Nevertheless, we believe that corporate tax statistics can provide valuable regional indicators depending on the application.

2.3.4 Turnover Tax

The turnover tax had the following properties. It was a tax on turnover of goods and services. Certain goods and services were exempt, such as imports and exports.[40] The tax base was the paid price, and the supplier of the good or service was liable for payment. After a few changes in the first half of the 1920s, the tax rate was lowered on April 1, 1926, from 1.00 to 0.75 percent (Deutsches Reich, 1926d, §§ 10, 12). In contrast to today's value-added tax, turnover tax paid on input goods could not be deducted, implying that refined products with multiple production stages split among many companies were taxed more heavily (Bach, 2018). Turnover taxes were collected by the tax district in which a company was headquartered or, in case of sole proprietors, where the proprietor resided (Statistisches Reichsamt, 1931d).

Some figures help to understand the nature of the turnover tax. In 1926, there were 4.8 million tax units liable to turnover tax. Of these, 95.45 percent were sole proprietors, while 4.06 percent were corporations, commercial cooperatives, and

[40] These goods and services were exempt from turnover tax: imports and exports, capital market transactions, trade with precious metals, rental of land, transport of goods and persons, lotteries, insurances and medical services, accommodation of workers and payment in kind, certain withdrawals from businesses. Exempt were also public utility companies and turnovers made for charitable purposes, private schools that depended on public funding, as well as self-employed scholars, artists, writers, commercial agents, and estate agents whose turnover did not exceed 6,000 RM (Deutsches Reich, 1926d, §§ 2, 3). In addition, trades that did not involve a transfer of ownership of the good traded were exempt (Zwischenhandelsprivileg) (Deutsches Reich, 1926d, § 7). Finally, yearly turnovers below 666 RM were exempt, too, as well as consumption of self-produced agricultural goods, such that small agricultural businesses do not figure in the turnover tax statistics (Statistisches Reichsamt, 1931d).

partnerships, and 0.49 percent government authorities and institutions.[41] Sole proprietors generated 42.43 percent of all declared turnover, suggesting that they constituted an important part of the economy. For 1929, the Statistical Office estimated total turnover made in Germany, which allows an assessment of the size of the tax base. Total turnover was estimated at 225.4 billion RM, of which 81.5 percent were declared and 58.2 percent were taxed, indicating that turnover tax revenues were derived from a good portion of total turnover, but not the whole (Statistisches Reichsamt, 1932, p.7).[42]

Some regions had more turnover tax units than others, providing insights into the degree of entrepreneurship and self-employment in a region (Figure 2.8).[43] Relative to the population, the number of tax units was large in the South (Bavaria, parts of Baden and Swabia), and Northwest (Lower Saxony), indicating that these regions relied more heavily on a small-scale, fragmented business structure. These regions were also relatively sparsely populated, in contrast with populous industrial regions, where larger companies possibly predominated. However, not all regions with low population density had a high numbers of turnover tax units. For example, the east (Brandenburg and Pomerania) and far east (East Prussia) had significantly lower numbers than the southern and northwestern regions, a fact that Statistisches Reichsamt (1931d, p.18) attributes to the presence of medium to large agricultural businesses at the expense of smaller ones.

As is the case with other taxes, the relationship between declared turnover, tax liability, and tax revenue indicates how well one can approximate the other at the regional level (Figure 2.9). Regional data on turnover tax bases are lacking, which is not critical in this case, as the tax base should perfectly correlate with the tax liability due to the linear tax rate. Regarding the declared turnover, we find a slightly regressive relation with tax liabilities, which may stem from tax exemptions for imports and exports as well as for trades without a transfer of ownership. Such exempt turnover was generated predominantly in the commerce and logistics sector and was probably located in large cities with above-average turnover (Statistisches Reichsamt, 1931d). Despite these exemptions, the correlation between average declared turnover and average tax liability is still very high (correlation = 0.955). Turnover tax revenues provided in our database also correlate highly with declared

[41] The source, Statistisches Reichsamt (1931d), uses the following German terms: sole proprietors: *Einzelpersonen*; corporations, commercial cooperatives, and partnerships: *Aktiengesellschaften, Kommanditgesellschaften a. A., Berggewerkschaften, Gesellschaften mit beschränkter Haftung, Genossenschaften, Kommanditgesellschaften, offene Handelsgesellschaften, übrige Gesellschaften und Körperschaften*; government authorities and institutions: *Behörden und Ämter*.

[42] The numbers for 1929 on declared turnover and taxed turnover exclude, for statistical reasons, tax units with a turnover below 5,000 RM. Hence, the tax base was in reality slightly larger than the 58.2 percent stated.

[43] The number of turnover tax units is not a perfect proxy for the number of businesses, as businesses with very low turnover (below 666 RM) were exempt from the turnover tax and do not figure into the statistics.

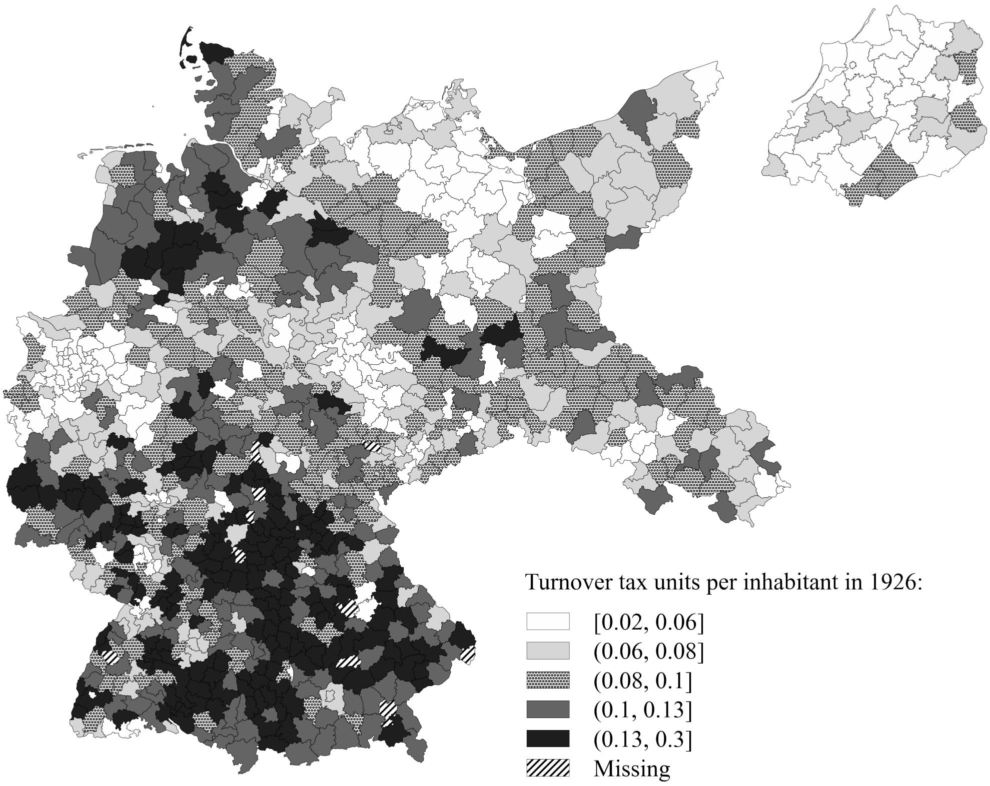
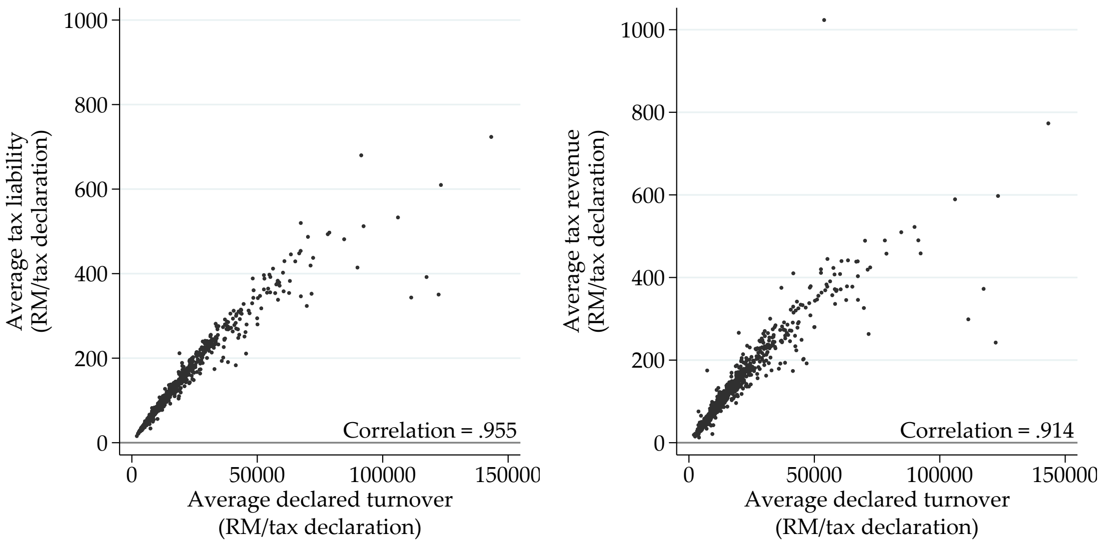


Figure 2.8: Regional distribution of turnover tax units per inhabitant in 1926.
Note: The map shows, for each tax district, the number of turnover tax units in 1926 divided by the number of inhabitants of the district. All tax units are assigned to the tax district in which a company's headquarters was located or, in case of sole proprietors, where the proprietor's residence was located. Tax districts are categorized by quintiles of the depicted variable such that each color group comprises 20 percent of the tax districts. Source: Statistisches Reichsamt (1931d, 1941). Calculations are our own.

turnover (correlation = 0.914), showing that they are an equally good proxy when there are no declared turnover data.

In summary, the results indicate that turnover tax statistics are another relevant source of historical regional indicators, because they relate, unlike the other taxes, directly to production. This might be why these statistics are used in the literature as a proxy for local production, although rather rarely (Vonyó, 2012, 2018). Economic researchers may often be interested in value added. In this regard, one concern is that the turnover tax was not a tax on value added and, naturally, differences between turnover and value added exist. On the other hand, we test in Section 2.4 how strongly turnover tax statistics correlate with regional GDP. We find high correlations, which supports the notion that turnover tax statistics provide good regional indicators of production in many empirical applications.



(a) Declared turnover and tax liability **(b)** Declared turnover and tax revenue

Figure 2.9: Indicators of the turnover tax for German tax districts in 1926.

Note: Each dot represents a tax district and shows the turnover, turnover tax liability, and turnover tax revenue of that district averaged over all turnover tax declarations from natural and legal persons within a district. Turnover comprises all taxed and untaxed turnover except for tax-free turnover from capital market transactions, charitable organizations, business with turnover below 666 RM, self-produced agricultural goods, as well as tax-free turnover of the state-owned postal and railway service. All data are assigned to the tax district in which a company's headquarters was located or, in case of sole proprietors, where the proprietor's residence was located. Turnover and liabilities are taken from the *Steuersoll* statistics, while tax revenues are taken from the *Steuerist* statistics. Data for 891 tax districts with non-missing data. Source: Statistisches Reichsamt (1931d), Statistisches Reichsamt (1941). Calculations are our own.

2.3.5 Wealth Tax

The wealth tax was a tax on net wealth of natural persons and most legal persons such as corporations (Deutsches Reich, 1926e, § 2).[44] In principle, all assets were taxed, and the law separated the assets into four categories: (1) agricultural and forestry assets, (2) business assets, (3) real estate and land, and (4) other assets (Deutsches Reich, 1926b, § 2). Assets held in foreign countries were taxed as well. Some assets, including pension and insurance entitlements, were exempt. Debts were subtracted from the value of assets (Statistisches Reichsamt, 1931b). The value of assets was assessed by the tax authorities on an irregular basis, for example, in

[44] This includes legal persons liable to corporate tax (business corporations, civil law corporations, foundations, and statutory credit institutions) as well as partnerships (*offene Handelsgesellschaften*, *Kommanditgesellschaften*), which were not liable to corporate tax. The shares held in these partnerships were not taxed, however (Deutsches Reich, 1926b, §§ 26, 46). Certain legal persons including charitable institutions and churches were exempt (Deutsches Reich, 1926e, § 4).

1925, and some assets were assessed again in 1927.[45] Assessment standards differed between the asset categories, but the guiding principle was that the asset value should either correspond to the current market value or the the typical profit an asset could generate over 25 years.[46] Assets of married persons were added together and taxed jointly (Deutsches Reich, 1926e, § 10).

The tax schedule contained several progressive elements and exemptions. All natural persons with net wealth of 5,000 RM or less were not taxed and are not included in the *Steuersoll* statistics (Statistisches Reichsamt, 1931b). Some other persons were exempt because their income and net wealth fell below certain thresholds. These persons are included in the statistics, though.[47] The lowest tax rate was 0.1 percent for persons with net wealth below 10,000 RM and it increased progressively up to 0.5 percent for persons with net wealth above 50,000 RM.[48]

[45]In 1927, business and other assets were assessed again, while agricultural and forestry assets, real estate, and land were not assessed (Deutsches Reich, 1927a,b).

[46]In general, the value of an asset should either reflect the typical profit it could generate over 25 years (*Ertragswert*) or the market value one could obtain under normal circumstances (*Gemeinwert*) (Deutsches Reich, 1921, § 138, § 152). Assets were evaluated and taxed according to one of the two principles. Agricultural and forestry assets were taxed according to the *Ertragswert*, and a typical profit was defined in reference to profits of similar farms in the same region (Deutsches Reich, 1926b, § 16). Similarly, real estate that was rented out or built according to local custom was valued according to the *Ertragswert*, and otherwise according to the *Gemeinwert*. The typical profit corresponded to earned rental income of the last three years—or rental income that could have been earned—less one fifth as allowance for maintenance. Undeveloped land was valued with the *Gemeinwert* (Deutsches Reich, 1921, § 152), and so were business assets (Deutsches Reich, 1926b, § 31). Within the group of other assets, shares were valued at the market value they were traded for on a fixed date (Deutsches Reich, 1926b, §§ 40, 41). Importantly, shares were liable at only half their value (Deutsches Reich, 1926b, §§ 43). For further assets, specific evaluation rules existed. For example, debt claims were valued at face value (Deutsches Reich, 1921, § 143).

[47]A natural person was exempt from the wealth tax if her net wealth did not exceed 10,000 RM and the yearly income did not exceed 3,000 RM. The threshold for income was higher for households with more than one child. Further, if a person was older than 60 years or incapacitated for work, the wealth tax was not levied if net wealth did not exceed 20,000 RM and yearly income 5,000 RM; or if net wealth did not exceed 30,000 RM and yearly income 4,000 RM. Again, the income thresholds varied with the number of children (Statistisches Reichsamt, 1931b; Deutsches Reich, 1926e, § 8).

[48]The tax brackets and tax rates were (Deutsches Reich, 1926e, § 7):

- 0.1% on net wealth ≤ 10,000 RM,
- 0.2% on net wealth > 10,000 and ≤ 20,000 RM,
- 0.3% on net wealth > 20,000 and ≤ 30,000 RM,
- 0.4% on net wealth > 30,000 and ≤ 50,000 RM,
- 0.5% on net wealth > 50,000 RM.

Starting in 1927, higher rates for net wealth above 250,000 RM applied (Statistisches Reichsamt, 1931b; Deutsches Reich, 1926e, §§ 7, 24). These were:

- 0.55% on net wealth > 250,000 and ≤ 500,000 RM,
- 0.6% on net wealth > 500,000 and ≤ 1,000,000 RM,
- 0.65% on net wealth > 1,000,000 and ≤ 2,500,000 RM,
- 0.7% on net wealth > 2,500,000 and ≤ 5,000,000 RM,
- 0.75% on net wealth > 5,000,000 RM.

As with the other taxes, liable persons had to pay their tax liability in quarterly advance payments based on last year's liability. Differences from the actual tax liability, which was determined by the tax authority after filing the tax declaration, were settled with the next quarterly payment. (Deutsches Reich, 1926e, § 15). Natural persons were liable at their place of residence, and legal entities at their headquarters (Deutsches Reich, 1921, §§ 51, 52). Wealth tax statistics are assigned accordingly.

Like the income tax declaration, a wealth tax declaration was not common due to the lump-sum exemption for net wealth of 5,000 RM or less. In 1927—there are no data for 1926—2.43 million natural and 0.11 million legal persons filed a wealth tax declaration. The declared net wealth was 64.6 billion and 34.4 billion RM, respectively. The actual tax base was smaller, as certain exemptions still had to be subtracted, the magnitude of which is unknown to us (Statistisches Reichsamt, 1931b).[49] Declared net wealth of natural persons constituted about one third of total net wealth of natural persons in Germany, which indicates that a substantial share of net wealth was exempt.[50] The yearly tax liability was 0.4 billion RM, resulting in an effective tax rate of 0.41 percent (Statistisches Reichsamt, 1931b).

The regional distribution of wealth tax declarations provides further insights into the nature of the wealth tax. In Figure 2.10, the share of wealth tax declarations made by natural persons per inhabitant is shown. The share is, in almost every

The higher rates did not apply to "assets subject to income taxation by states and municipalities" ("Vermögen, das der Ertragsbesteuerung durch Länder und Gemeinden unterliegt" (Deutsches Reich, 1926e, § 7). Hue de Grais et al. (1926) loosely specify these assets as land, real estate, and business assets. Terhalle (1926, p.324) states that the higher rates were primarily meant to tax mobile capital. Besides this, note that we report the tax brackets stated in Hue de Grais et al. (1926, p.242), because the tax brackets stated in our copy of Deutsches Reich (1926e, §7) seem to be inconsistent. The inconsistent brackets were 0.65% on net wealth > 1,000,000 and ≤ 2,500,000 RM; 0.7% on net wealth > 2,000,000 and ≤ 3,000,000 RM; 0.75% on net wealth > 5,000,000 RM.

[49]Non-subtracted exemptions are those granted for combinations of low income and net wealth (exemptions according to *§ 8 Abs. V.St.G. Nr. 1 und 2* and *§ 7 Abs. 3 V.St.G. und Erlass des R.d.F.III v 1180 vom 12.4.1927).* There are, as far as we know, no data on the magnitude of these exemptions, but on the reduction in tax liabilities caused by them. These reductions amount to 24,472,521 RM in 1927, which, when divided by the lowest possible tax rate of 0.1 percent, corresponds to a tax base of approximately 24 billion RM (Statistisches Reichsamt, 1931b, p.40). This is a conservative estimate, as some of the exempt net wealth would probably have been taxed at a higher tax rate. It is also important to note that no lump-sum exemption of 5,000 RM was granted to liable individuals, so that this does not have to be taken into account in the relation between the declared net wealth and the tax base (Deutsches Reich, 1926e, §§ 6-8), (Statistisches Reichsamt, 1931b, p.44). Apart from this, the declared net wealth corresponds to the value of assets less debt, liabilities, and other minor deductions: liabilities due to future rent payments (*Rentenschulden*), wages and interest payments received within three months prior to the wealth assessment (*Dreimonatsabzug*), and half-year profit from agricultural and forestry businesses due to different assessment dates for working capital and other assets (Statistisches Reichsamt, 1931b, p.27).

[50]Total net wealth of natural persons was estimated by Albers et al. (2020) at about 180 billion RM.

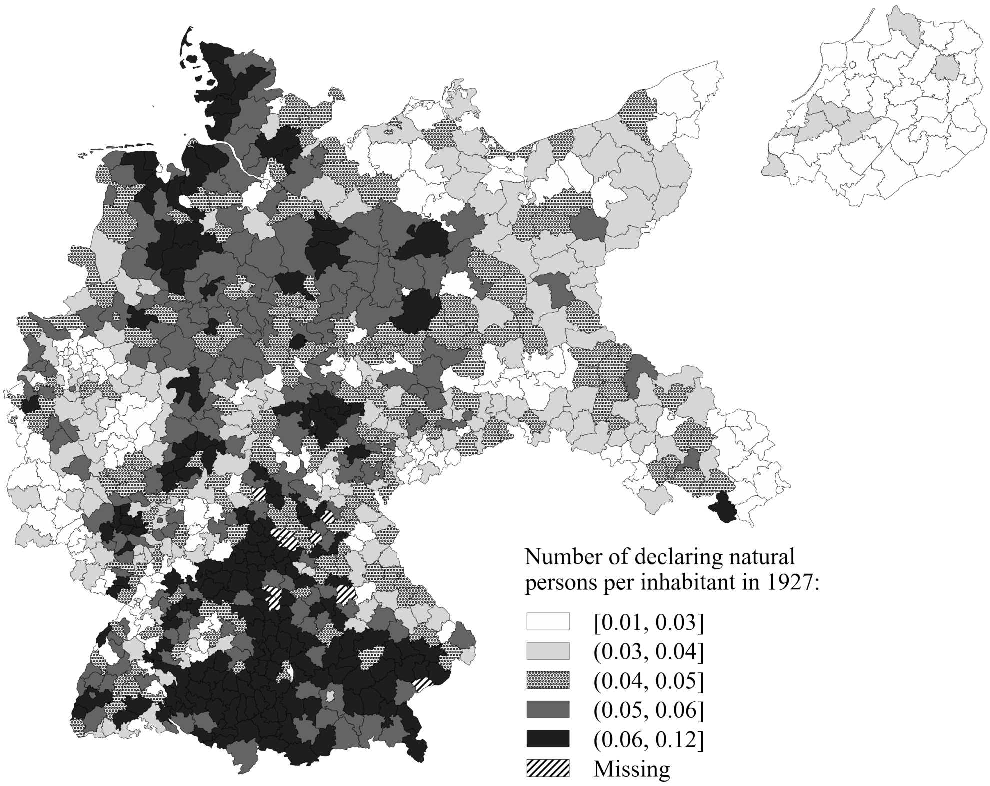
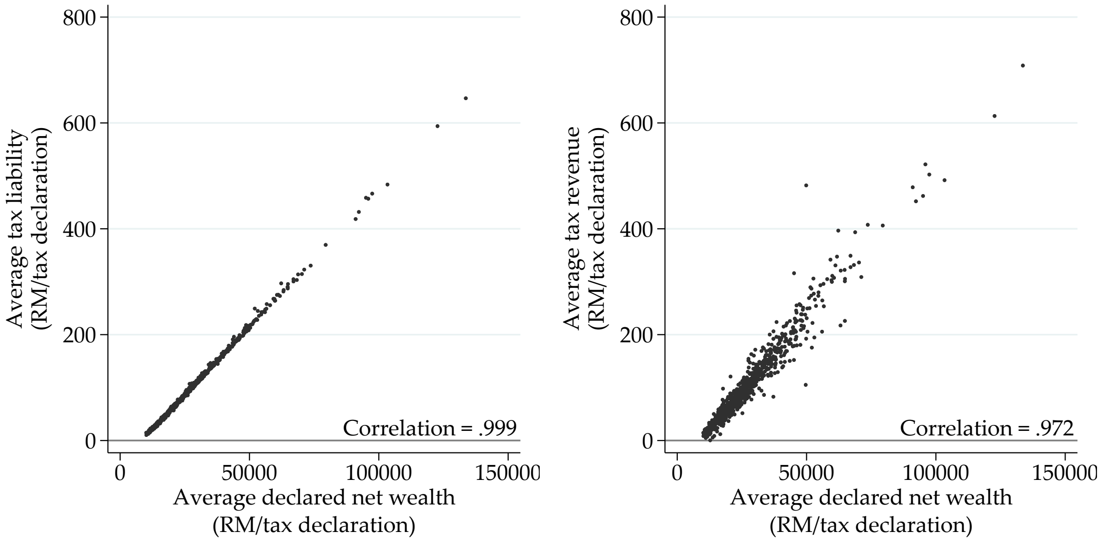


Figure 2.10: Regional distribution of natural persons who declared their wealth per inhabitant in 1927.

Note: The map shows, for each tax district, the number of natural persons who declared their wealth in 1927 divided by the number of inhabitants of the tax district. All tax units are assigned to the tax district in which their place of residence was located. Tax districts are categorized by quintiles of the depicted variable such that each color group comprises 20 percent of the tax districts. Source: Statistisches Reichsamt (1931b, 1941). Calculations are our own.

region, below 12 percent and higher by comparison in northern and southern regions. Presumably, these regions had a higher level of entrepreneurship and a small-scale agricultural structure that included some business and agricultural wealth, as also suggested by the turnover tax statistics. One should not conclude, however, that these regions are on average richer, as regional wealth distributions might be highly skewed.

Additional regional data on the declared net wealth and the tax liability show the effects of the progressive tax schedule. In Figure 2.11a, the average declared net wealth in each tax district is plotted against the average tax liability. The relationship between the two is nearly perfectly linear (correlation = 0.999), indicating that the progressive tax schedule—and those tax exemptions still included the declared net wealth variable—had little effect on the linearity between both statistics. Similarly, declared net wealth also correlates highly with tax revenues (correlation = 0.972,



(**a**) Declared net wealth and tax liability (**b**) Declared net wealth and tax revenue

Figure 2.11: Indicators of the wealth tax for German tax districts in 1927, natural and legal persons. Note: Each dot represents a tax district and shows the declared net wealth, wealth tax liability, and wealth tax revenue of that district averaged over all wealth tax declarations from natural and legal persons within a district. Net wealth corresponds to the value of assets less debt, liabilities, and other minor deductions (liabilities due to future rent payments (*Rentenschulden*), wages and interest payments received within three months prior to the wealth assessment (*Dreimonatsabzug*), and half-year profit from agricultural and forestry businesses due to different assessment dates for working capital and other assets.) All data are assigned to the tax district in which the residence of a person was located, or, in case of legal persons, in which the headquarters was located. Net wealth and liabilities are taken from the *Steuersoll* statistics, while tax revenues are taken from the *Steuerist* statistics. Data for 899 tax districts with non-missing data. Source: Statistisches Reichsamt (1931b), Statistisches Reichsamt (1941). Calculations are our own.

Figure 2.11b), suggesting that tax revenues are a possible proxy variable for declared net wealth in regional analyses.[51]

In conclusion, the data suggest that wealth tax revenues are appropriate to approximate declared net wealth. Because of tax exemptions, however, declared net wealth does not correspond to total net wealth, and it remains uncertain to what extent the lack of exempt net wealth is important for applications of the tax revenue data. Regional data on historical wealth levels are, to our knowledge, non-existent, which, on the one hand, bars us from testing the effect of tax exemptions, but, on the other hand, emphasizes the importance of having at least tax revenue data.

[51] Similar to the other taxes, we assume that differences in the reference period between the *Steuersoll* and *Steuerist* statistics explain most of why tax liabilities differ slightly from tax revenues. Further, some tax units might have not been able to pay their taxes, causing an additional deviation between the two statistics.

2.4 Tax Data as a Proxy for GDP

In this section, we compare the tax revenue data with regional estimates of historical GDP to examine whether tax revenues are a good indicator for regional economic development. We choose GDP because it is the classic measure for economic growth and development. Available historical GDP estimates are, however, less detailed in terms of spatial and temporal granularity than tax revenue data. Hence, the detailed data in our database can be a valuable addition to many regional analyses.[52]

2.4.1 Comparison of Tax Data and GDP Data Across Regions

We first compare tax revenues and GDP across regions. For the comparison, we use the correlation between GDP and tax revenues at the regional level. We also use the R^2 statistic from a regression of GDP on tax revenues to see how much of the regional variation in GDP is explained. We calculate these indicators separately for each tax. The analysis also considers the tax statistics from the tax declaration statistics because they differ from tax revenues in certain aspects, as shown in Section 2.3. More specifically, we use the same tax declaration statistics as in Section 2.3, that means, the number of tax units, the payroll tax base, the declared income, corporate profit, declared turnover, and declared net wealth. Since regions differ in size, we divide all data by the population of a region so that the correlations are unaffected by the size differences.

GDP and population estimates are drawn from the sixth version of the Rosés and Wolf (2019) database. We select data for 1925 and 1938—the years corresponding most closely to our tax data. These years are compared to tax data for 1926 and 1938, as we lack tax data for 1925. Since the NUTS 2 regional division of the Rosés and Wolf database is broader than the tax district division, we add up the tax data so that they correspond to the NUTS 2 regions. In cases where a tax district overlaps with two or more NUTS 2 regions, we split the tax data of the tax district between the NUTS 2 regions. The split is proportional to the spatial overlap between the tax district and a NUTS 2 region.[53] The final sample consists of all 36 German NUTS 2 regions, with the exception of the Saar region in 1925, which we excluded because no tax data are available.[54]

[52]For interwar Germany (and Europe), the most detailed regional GDP estimates are provided by Rosés and Wolf (2018, 2019). The data are provided at the relatively broad NUTS 2 level and include, for the interwar period, the years 1925 and 1938.

[53]For instance, if 60 percent of the area of a tax district is covered by a specific NUTS 2 region, we assign 60 percent of its tax data to that NUTS 2 region. The remaining data are assigned to the other NUTS 2 regions that cover the tax district.

[54]The Saar region was not part of Germany in 1925. We also exclude the Polish NUTS 2 regions that used to be part of Germany, as no GDP estimates are available for them.

The results are presented in Table 2.2. They indicate that many per-capita tax statistics are a good proxy for per-capita GDP. The correlations are mostly very high, with the maximum in 1925/1926 being 0.872 and in 1938 0.849. Moreover, the R^2 statistics reach values up to 0.761. The estimates for both the 1925/1926 and 1938 period are similar, suggesting that tax revenues are a good proxy in both periods. With regards to tax revenues and tax declaration statistics, the results indicate that data from the two statistical sources correlate equally well with GDP. One exception is the per-capita number of tax units, which in some cases has a lower correlation. All in all, both statistics could serve as indicators for regional development.

Payroll tax statistics are among those that correlate most strongly with GDP. The correlation for the payroll tax base is 0.861, and for payroll tax revenues it is 0.870 in 1925/1926 and 0.833 in 1938. Notably, the payroll tax bases and tax revenues correlate equally well, suggesting that differences in the regional allocation of the two tax statistics described in Section 2.3.1 do not have significant effects on the correlation.[55]

Declared income and corporate tax statistics have a slightly weaker correlation than the other statistics considered, with magnitudes of around 0.7 to 0.8, which is still relatively high. Declared income tax statistics do not include labor income of many employers and, thus, do not contain the full labor share of value added. Similarly, corporate tax statistics represent only the capital share of GDP. They also do not consider production carried out by sole proprietors and partnerships, which were not liable for corporate taxation. These aspects probably explain the lower correlations.

Turnover tax statistics are a better proxy for GDP than declared income and corporate tax statistics. Correlations range from 0.804 to 0.849, depending on the year and statistic. The number of turnover tax units has a negative correlation with GDP, which supports findings from the previous section that many turnover tax units are probably small-scale businesses with supposedly low value added.

Wealth tax statistics referring to legal persons (companies) are a good proxy for GDP as well, probably reflecting the natural relationship between GDP and capital. For example, in 1925/1926, the correlation between GDP and the declared net wealth of legal persons is 0.813. Wealth tax statistics for natural persons correlate less closely with GDP, probably due to the fact that part of their wealth is not utilized in production or utilized less efficiently than corporate wealth. As regards tax revenues, data are only available jointly for natural and legal persons. The corresponding

[55] Payroll tax revenues are allocated to the tax district where the headquarters of the employer is located, while the tax base is allocated to the place of residence of the employees. For more finely grained regional divisions than NUTS 2, this difference in allocation may be important. Payroll tax revenues are possibly a good proxy for GDP if employees live close to the production site. On the other hand, the payroll tax base is a good proxy if the employer does not have branches spread out among regions and if the headquarters is the single production site.

Table 2.2: Correlations between per-capita GDP and per-capita tax indicators for NUTS 2 regions.

	Correlation		Explained variation in GDP (R^2)	
	1925/1926	1938	1925/1926	1938
Payroll tax:				
- Number of tax units	0.744	.	0.553	.
- Tax base	0.861	.	0.742	.
- Tax revenue	0.870	0.833	0.757	0.695
Declared income tax:				
- Number of tax units	-0.069	.	0.005	.
- Declared gross income	0.763	.	0.582	.
- Tax revenue	0.786	0.740	0.618	0.548
Corporate tax:				
- Number of tax units	0.660	.	0.435	.
- Profit	0.765	.	0.586	.
- Tax revenue	0.728	0.799	0.530	0.638
Turnover tax:				
- Number of tax units	-0.521	.	0.271	.
- Declared turnover	0.804	.	0.646	.
- Tax revenue	0.824	0.849	0.679	0.720
Wealth tax, natural persons:				
- Number of tax units	-0.384	.	0.148	.
- Declared net wealth	0.370	.	0.137	.
Wealth tax, legal persons:				
- Number of tax units	0.872	.	0.761	.
- Declared net wealth	0.813	.	0.662	.
Wealth tax, natural and legal persons:				
- Tax revenue	0.744	0.820	0.554	0.672
Observations	35	36	35	36

Note: Column 1 shows the correlation between GDP per capita in 1925 and different per-capita tax statistics in 1926 on the level of NUTS 2 regions. The number of tax units of the wealth tax and declared net wealth refer to 1927. Column 2 shows the correlation between GDP per capita and different per-capita tax statistics in 1938. Column 3 and 4 show the R^2 of a linear regression of GDP per capita regressed on the respective per-capita tax statistic. All data are standardized with the population of a region in 1926 or 1938. Tax statistics are defined as in Section 2.3. All tax statistics except tax revenues are drawn from the tax declaration (*Steuersoll*) statistics. Source: Statistisches Reichsamt (1929a, 1931a,b,d, 1941), Rosés and Wolf (2019). Calculations are our own.

correlations are 0.744 in 1925/1926 and 0.820 in 1938, which are relatively high values as well.

In summary, we find that most tax statistics correlate relatively closely with GDP per capita, suggesting that tax data can serve as a substitute for economic development if GDP data are lacking. The results hold for the NUTS 2 level. At smaller regional levels, we expect tax statistics to correlate slightly worse with GDP as the spatial mismatch between the place of production (location of the plant) and the place of tax reporting (location of headquarters or the place of residence) is likely to increase. Moreover, specific industries or companies get a larger relative weight within a region. Hence, idiosyncratic production functions and labor-capital shares become more important for the relation between GDP and tax data at smaller regional levels. Nevertheless, we believe that the variety and detail of tax data offer significant potential for empirical applications.

2.4.2 Comparison of Tax Revenue Growth and GDP Growth

Besides the level of GDP, its growth is of equal importance. So can GDP growth be approximated by growth in tax revenues?

At the national level, it becomes evident that tax revenues grow and decrease more strongly than GDP from one year to the next [Figure 2.12; GDP data from Ritschl and Spoerer (1997)]. Moreover, tax revenue growth rates are marked by exceptional spikes, which probably stem from tax reforms that reduce the correlation between tax revenue and GDP growth.[56] Although correlations are still relatively high for the payroll, income, and corporate tax, there does not seem to be a clear argument for using tax revenue growth as a substitute for GDP growth at the national level; also, because national GDP estimates are readily available for each year in Ritschl and Spoerer (1997).

For regions, the data by Rosés and Wolf (2019) allow us to assess GDP growth over the total period from 1925 to 1938. For each NUTS 2 region, we calculated the growth in GDP and in tax revenues. Next, we determined the correlation between the two growth rates across regions.[57] The results in Table 2.3 show that, for this

[56]For example, in 1935, a new income tax law came into force. As a consequence, the so-called crisis tax (*Krisensteuer*), charges for unemployment assistance, and a tax for unmarried persons (*Ehestandshilfe*) were incorporated into income taxation, leading to an exceptional increase of specifically payroll tax revenues. Also, corporate taxation underwent a series of changes such as the introduction of the tax loss carry-forward in 1928/1929 as well as increases in the tax rate from 20 to 25 percent in 1936, and to 30 percent in 1937. In 1938, the corporate tax rate was raised again to 35 percent for corporations with profits larger than 100,000 RM. The turnover tax was raised from 0.75 to 0.85 percent in 1930, and to 2.00 percent in 1932, probably explaining the anti-cyclical growth of turnover tax revenues in that period. In terms of wealth taxation, important changes include the increase of the tax exemption limit from 5,000 to 20,000 RM in 1931, and a reduction of assessed asset values (*Einheitswerte*) by 20 percent between 1932 and 1936 (Statistisches Reichsamt, 1941). Wealth tax exemption limits were changed again in 1936 to 10,000 RM for individuals and varying limits for spouses depending on the number of children. For a more detailed description of tax reforms, we refer to Statistisches Reichsamt (1941).

[57]For the comparison, we deflated the tax revenues with a gross national income deflator provided by Ritschl and Spoerer (1997), while the GDP data by Rosés and Wolf (2019) are already deflated. As in Section 2.4.1, we used the 1926 tax revenue data because no data were available for 1925.

Figure 2.12: Yearly growth rates of nominal GDP and tax revenues from different taxes in Germany for the years 1927 to 1938.

Note: The correlations between the growth rates of GDP and those of tax revenues are for the payroll tax: 0.861; declared income tax: 0.940; corporate tax: 0.813; turnover tax: −0.008; wealth tax: 0.669. Source: Statistisches Reichsamt (1941); Ritschl and Spoerer (1997). Calculations are our own.

longer period and at the regional level, tax revenue growth does not correlate closely with GDP growth. This is true regardless of whether growth of absolute or per-capita values is considered. Among all taxes, payroll tax revenues have the highest positive correlations (0.203 and 0.190). The other taxes correlate negatively with GDP growth, indicating that none of the tax revenue growth rates are suitable indicators for long-term regional development.

In conclusion, the results suggest that tax revenue growth rates are a mediocre proxy for GDP growth, especially at the regional level and over the long term. The correlations between growth rates are probably relatively low due to tax reforms and changes over time in underlying fundamentals, such as market elasticities and tax incidences. For shorter periods and taking into account tax reforms, tax revenue growth may still be a valuable proxy for GDP growth.

Table 2.3: Correlations between GDP growth and tax revenue growth from 1925/1926 to 1938 for NUTS 2 regions.

	Correlation with percentage growth in GDP	
	(1) Growth in absolute values	(2) Growthin per-capita values
Percentage growth in tax revenues from:		
- Payroll tax	0.203	0.190
- Income tax	-0.391	-0.432
- Corporate tax	-0.160	-0.160
- Turnover tax	-0.311	-0.435
- Wealth tax	-0.130	-0.180
Observations	35	35

Note: Column (1) shows the correlation between the percentage growth of absolute real GDP from 1925 to 1938 and the percentage growth of absolute real tax revenues from 1926 and 1938 at the level of NUTS 2 regions. Column (2) shows the correlation between the two measures in per-capita terms. Tax statistics are defined as in Section 2.3. Data: Statistisches Reichsamt (1941), Rosés and Wolf (2019). Calculations are our own.

2.5 Qualifications and Extensions

This article has strengths and limitations. Starting with the strengths, we think that the article's most important contribution is to provide a novel database of historical and geocoded data. In particular, the geocoding is of great value because it helps researchers to perform spatial data operations (mapping, conversion of data to other regional divisions, matching with other data sources), which increases the applicability of the historical data by a large extent. We hope that the simple use of the data facilitates interesting empirical work on a wide range of topics. Topics such as the economic development of regions, the persistence of historical institutions, and the analysis of taxation come to our mind. Other research disciplines such as political science, geography, and history may also find value in the data.

Besides this, we hope to have given a sufficiently thorough account of the interwar tax system. Our analysis indicates that tax exemptions were relatively extensive. We could not show, however, whether tax exemptions have significant implications for the application of tax revenue data in regional analyses. Our concern is that tax exemptions may devalue tax revenues as a regional proxy for common economic indicators, such as household income. The validity depends, in our view, on the linearity of the relationship between tax revenues and the approximated indicator. Unfortunately, we lack the data, for example, on regional household incomes, to test how linear the relationship is. Nevertheless, results in Section 2.4 indicate that tax revenues correlate reasonably well with GDP, suggesting that they can serve as an indicator for regional development. Moreover, some researchers may be interested in tax revenues themselves rather than in their ability to approximate other economic indicators.

In hindsight, we also see that the geocoded tax districts could have been stored in a different format. The current format is one shapefile per year, with the status of borders fixed at a specific date. This is a very common and accessible format. However, it does not take into account that borders changed throughout the year, so that valuable information is not stored and lost. Other formats are available where borders have validity dates, such as spatial databases (Peuquet, 2005). The use of such databases would improve the temporal precision of the historical information stored and would allow researchers to retrieve the status of borders at arbitrary dates, which may be beneficial for certain applications.

There are two immediate extensions to our work. First, additional regional tax data can be merged to our geocoded tax districts and tax states. The main source for such data are the *Steuersoll* statistics published by the Statistical Office. Among these statistics are those used in Section 2.3, but more statistics are available, including the number of taxpayers and the tax base for different years, the type of declared wealth, and the turnover by industry.[58] Although *Steuersoll* statistics are not available for every year of the interwar period, they can offer new research potential, in particular when they are combined with the geocoded tax districts.

Second, our work may serve as a starting point for the estimation of regional levels of income and wealth. The estimation would require an approximation of income and wealth of the untaxed households and possibly consider specific parameters of the tax system, such as exemption levels. Although the estimation would probably have to rely on certain simplifying assumptions, regional income and wealth estimates would undoubtedly provide better opportunities for analyzing growth and give new insights into Germany's (long-term) regional development.

2.6 Conclusion

The present paper provides a novel database of geocoded tax data for small German regions in the interwar period (1926–1938). The data are retrieved from various historical sources and comprise yearly tax revenues for five different taxes: the payroll, declared income, corporate, wealth, and turnover tax. All data are geocoded and available at the regional level of about 900 historical tax districts and larger tax states, allowing for detailed regional analyses. We show that the tax revenues provided correlate closely with historical GDP estimates at the NUTS 2 region level. Hence, the data serve as a measure of economic development for a period for

[58]For instance, Statistisches Reichsamt (1931c), Statistisches Reichsamt (1937), and Statistisches Reichsamt (1939a) contain regional information on the number of tax units, and Statistisches Reichsamt (1931b) and Statistisches Reichsamt (1931d) contain data on wealth and turnover.

which there are relatively few regional economic indicators. Thereby they can help researchers in their empirical work on the German interwar period and its long-term repercussions.

All in all, the paper shows that geocoding of historical data brings important benefits, despite requiring a certain time and effort. Geocoding facilitates the conversion of historical data into modern administrative regional divisions and allows them to be linked with other data sources. These spatial data operations enhance the applicability of the historical data for economic research, which increasingly relies on linking data and analyzing long-term developments and historical events (Abramitzky, 2015; Schröder et al., 2020).

Against this background, the paper paves the way for several interesting research directions. One idea is to use tax data as regional measure of economic development in analyses of relevant events of the interwar period. This may, for example, convey important information about the regional impact of the Great Depression and possible consequences for political voting behavior. Next, it would be interesting to use historical tax data to estimate regional income and wealth levels, which would allow comparing the development of regions over several decades. This may help us better understand the regional development of German regions and, for example, its relationship to large-scale public interventions during the postwar reconstruction period. A related idea is to link historical tax data to individual-level information to examine whether variations in regional development provide unequal opportunities for individual outcomes over the life-course. Other ways of linking recent data with the historical tax data presented in this paper could also stimulate new research. And many analyses may benefit from additional regional tax data published by the Statistical Office that can be linked to the geocoded tax districts provided in this paper.[59]

[59]For instance, Statistisches Reichsamt (1931b), Statistisches Reichsamt (1931c), Statistisches Reichsamt (1931d), Statistisches Reichsamt (1937), and Statistisches Reichsamt (1939a) contain further regional tax data.

2.7 Appendix

2.7.1 Description of Variables

Table 2.4 gives an overview of the variables in the database. All monetary variables are in Reichsmark (RM) in nominal terms.[60] The variable *id* is a unique identifier of each tax district. The *id* changes over time whenever the borders of a tax district change. For every change, a 9 was appended at the end of an *id*. If two or more districts were merged, the *id*s of all districts were appended, separated by 9s.[61] The second identifier variable, *initialid*, corresponds to the *id* of 1926 and remains unchanged over all years. The *id* variable of the first year, 1926, corresponds, with a few exceptions, to the page number and position of a tax district in the tables of the original source and allows us to trace the data back to the source.[62] The variable *stateid* identifies the tax states and also shows which tax state a tax district belongs to. The variable is coded according to the principles of the *id* variable.

An excerpt from the original data source is shown in Figure 2.13.

[60] National GDP deflators for the period are available, for example, in Ritschl and Spoerer (1997).

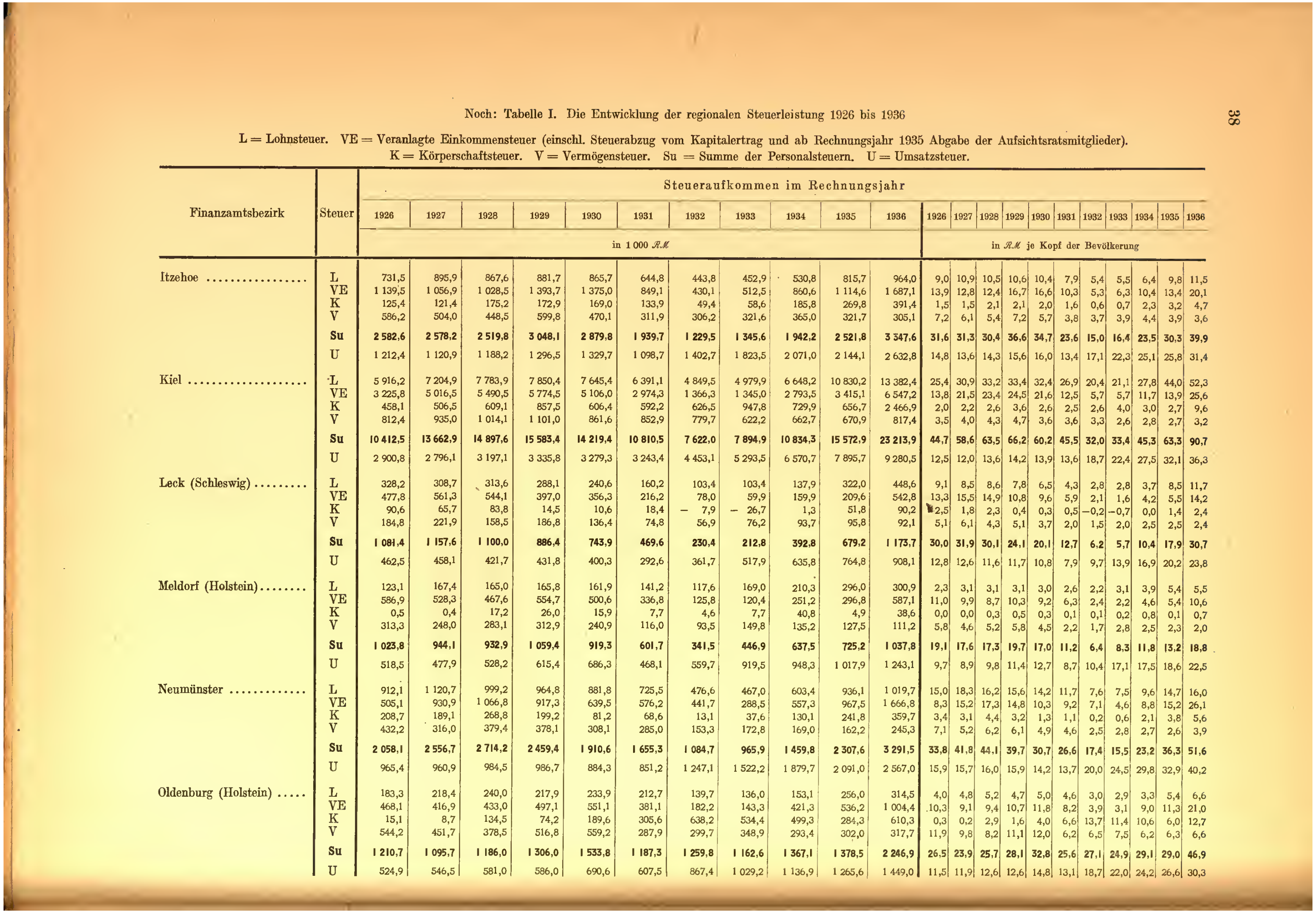
[61] For instance, after the merge of two tax districts with *id*s 1202 and 1172, the new tax district gets the *id* 120291172.

[62] For instance, the *id* 1266 implies that the data of the given tax district are found in Statistisches Reichsamt (1941) on page 126 at the sixth position in the table. The exceptions are tax districts that changed between 1925 and 1926 and are consequently marked with a 9, since the initial map in our work referred to 1925 (see Section 2.7.3).

Table 2.4: List of variables of tax revenue database.

Variable[a]	Description	Spatial allocation of tax revenues
id	Unique identifier	
initialid	Unique identifier in the first year, 1926	
district	Name of tax district	
year	Fiscal year in which taxes were collected [b]	
p_revenue	Payroll tax revenue in 1,000 RM	To tax district where the headquarters of employer was located
p_pcrevenue	Payroll tax in RM per capita	
i_revenue	Declared income tax revenue in 1,000 RM[c]	To tax district where the residence of household was located
i_pcrevenue	Declared income tax revenue in RM per capita[c]	
c_revenue	Corporate tax revenue in 1,000 RM	To tax district where the headquarters of corporation was located
c_pcrevenue	Corporate tax revenue in RM per capita	
w_revenue	Wealth tax revenue in 1,000 RM	To tax district where the residence of household or headquarters of legal person was located
w_pcrevenue	Wealth tax revenue in RM per capita	
su_revenue	Sum of p_revenue, i_revenue, c_revenue, and w_revenue in 1,000 RM	
su_pcrevenue	Sum of p_revenue, i_revenue, c_revenue, and w_revenue in RM per capita	
t_revenue	Turnover tax revenue in 1,000 RM	To tax district where headquarters of company or residence of sole proprietor was located
t_pcrevenue	Turnover tax revenue in RM per capita	
population	Number of inhabitants[d]	
level	Administrative level: [0] national, [1] tax states, [2] tax districts	
stateid	Unique identifier of tax state a tax district belongs to	
state	Tax state a tax district belongs to	
page	Page in Statistisches Reichsamt (1941) the data are retrieved from	
geometry	Polygon object containing district borders	

[a] In the shapefiles, some variable names are shortened due to limitations of the file format.
[b] The fiscal year, for example 1926, went from April 1, 1926 to March 31, 1927 (Hacker, 2013, p.165).
[c] Includes the 10 percent capital withholding tax and, starting in 1935, it includes a levy for supervisory board members (the latter became deductible from the income tax in 1935, Statistisches Reichsamt (1941)).
[d] For the years 1926 to 1936, the original source does not state the number of inhabitants and we calculated it as follows: $1000 \times su\_revenue/su\_pcrevenue$. The underlying number of inhabitants is a yearly estimate made by Statistisches Reichsamt (1941) based on census data from 1925 and 1933.



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Noch: Tabelle I. Die Entwicklung der regionalen Steuerleistung 1926 bis 1936

L = Lohnsteuer. VE = Veranlagte Einkommensteuer (einschl. Steuerabzug vom Kapitalertrag und ab Rechnungsjahr 1935 Abgabe der Aufsichtsratsmitglieder). K = Körperschaftsteuer. V = Vermögensteuer. Su = Summe der Personalsteuern. U = Umsatzsteuer.

Finanzamtsbezirk	Steuer	Steueraufkommen im Rechnungsjahr 1926	1927	1928	1929	1930	1931	1932	1933	1934	1935	1936
		in 1 000 ℛℳ										
Itzehoe	L	731,5	895,9	867,6	881,7	865,7	644,8	443,8	452,9	530,8	815,7	964,0
	VE	1 139,5	1 056,9	1 028,5	1 393,7	1 375,0	849,1	430,1	512,5	860,6	1 114,6	1 687,1
	K	125,4	121,4	175,2	172,9	169,0	133,9	49,4	58,6	185,8	269,8	391,4
	V	586,2	504,0	448,5	599,8	470,1	311,9	306,2	321,6	365,0	321,7	305,1
	Su	**2 582,6**	**2 578,2**	**2 519,8**	**3 048,1**	**2 879,8**	**1 939,7**	**1 229,5**	**1 345,6**	**1 942,2**	**2 521,8**	**3 347,6**
	U	1 212,4	1 120,9	1 188,2	1 296,5	1 329,7	1 098,7	1 402,7	1 823,5	2 071,0	2 144,1	2 632,8
Kiel	L	5 916,2	7 204,9	7 783,9	7 850,4	7 645,4	6 391,1	4 849,5	4 979,9	6 648,2	10 830,2	13 382,4
	VE	3 225,8	5 016,5	5 490,5	5 774,5	5 106,0	2 974,3	1 366,3	1 345,0	2 793,5	3 415,1	6 547,2
	K	458,1	506,5	609,1	857,5	606,4	592,2	626,5	947,8	729,9	656,7	2 466,9
	V	812,4	935,0	1 014,1	1 101,0	861,6	852,9	779,7	622,2	662,7	670,9	817,4
	Su	**10 412,5**	**13 662,9**	**14 897,6**	**15 583,4**	**14 219,4**	**10 810,5**	**7 622,0**	**7 894,9**	**10 834,3**	**15 572,9**	**23 213,9**
	U	2 900,8	2 796,1	3 197,1	3 335,8	3 279,3	3 243,4	4 453,1	5 293,5	6 570,7	7 895,7	9 280,5
Leck (Schleswig)	L	328,2	308,7	313,6	288,1	240,6	160,2	103,4	103,4	137,9	322,0	448,6
	VE	477,8	561,3	544,1	397,0	356,3	216,2	78,0	59,9	159,9	209,6	542,8
	K	90,6	65,7	83,8	14,5	10,6	18,4	− 7,9	− 26,7	1,3	51,8	90,2
	V	184,8	221,9	158,5	186,8	136,4	74,8	56,9	76,2	93,7	95,8	92,1
	Su	**1 081,4**	**1 157,6**	**1 100,0**	**886,4**	**743,9**	**469,6**	**230,4**	**212,8**	**392,8**	**679,2**	**1 173,7**
	U	462,5	458,1	421,7	431,8	400,3	292,6	361,7	517,9	635,8	764,8	908,1
Meldorf (Holstein)	L	123,1	167,4	165,0	165,8	161,9	141,2	117,6	169,0	210,3	296,0	300,9
	VE	586,9	528,3	467,6	554,7	500,6	336,8	125,8	120,4	251,2	296,8	587,1
	K	0,5	0,4	17,2	26,0	15,9	7,7	4,6	7,7	40,8	4,9	38,6
	V	313,3	248,0	283,1	312,9	240,9	116,0	93,5	149,8	135,2	127,5	111,2
	Su	**1 023,8**	**944,1**	**932,9**	**1 059,4**	**919,3**	**601,7**	**341,5**	**446,9**	**637,5**	**725,2**	**1 037,8**
	U	518,5	477,9	528,2	615,4	686,3	468,1	559,7	919,5	948,3	1 017,9	1 243,1
Neumünster	L	912,1	1 120,7	999,2	964,8	881,8	725,5	476,6	467,0	603,4	936,1	1 019,7
	VE	505,1	930,9	1 066,8	917,3	639,5	576,2	441,7	288,5	557,3	967,5	1 666,8
	K	208,7	189,1	268,8	199,2	81,2	68,6	13,1	37,6	130,1	241,8	359,7
	V	432,2	316,0	379,4	378,1	308,1	285,0	153,3	172,8	169,0	162,2	245,3
	Su	**2 058,1**	**2 556,7**	**2 714,2**	**2 459,4**	**1 910,6**	**1 655,3**	**1 084,7**	**965,9**	**1 459,8**	**2 307,6**	**3 291,5**
	U	965,4	960,9	984,5	986,7	884,3	851,2	1 247,1	1 522,2	1 879,7	2 091,0	2 567,0
Oldenburg (Holstein)	L	183,3	218,4	240,0	217,9	233,9	212,7	139,7	136,0	153,1	256,0	314,5
	VE	468,1	416,9	433,0	497,1	551,1	381,1	182,2	143,3	421,3	536,2	1 004,4
	K	15,1	8,7	134,5	74,2	189,6	305,6	638,2	534,4	499,3	284,3	610,3
	V	544,2	451,7	378,5	516,8	559,2	287,9	299,7	348,9	293,4	302,0	317,7
	Su	**1 210,7**	**1 095,7**	**1 186,0**	**1 306,0**	**1 533,8**	**1 187,3**	**1 259,8**	**1 162,6**	**1 367,1**	**1 378,5**	**2 246,9**
	U	524,9	546,5	581,0	586,0	690,6	607,5	867,4	1 029,2	1 136,9	1 265,6	1 449,0

Finanzamtsbezirk	Steuer	1926	1927	1928	1929	1930	1931	1932	1933	1934	1935	1936
		in ℛℳ je Kopf der Bevölkerung										
Itzehoe	L	9,0	10,9	10,5	10,6	10,4	7,9	5,4	5,5	6,4	9,8	11,5
	VE	13,9	12,8	12,4	16,7	16,6	10,3	5,3	6,3	10,4	13,4	20,1
	K	1,5	1,5	2,1	2,1	2,0	1,6	0,6	0,7	2,3	3,2	4,7
	V	7,2	6,1	5,4	7,2	5,7	3,8	3,7	3,9	4,4	3,9	3,6
	Su	**31,6**	**31,3**	**30,4**	**36,6**	**34,7**	**23,6**	**15,0**	**16,4**	**23,5**	**30,3**	**39,9**
	U	14,8	13,6	14,3	15,6	16,0	13,4	17,1	22,3	25,1	25,8	31,4
Kiel	L	25,4	30,9	33,2	33,4	32,4	26,9	20,4	21,1	27,8	44,0	52,3
	VE	13,8	21,5	23,4	24,5	21,6	12,5	5,7	5,7	11,7	13,9	25,6
	K	2,0	2,2	2,6	3,6	2,6	2,5	2,6	4,0	3,0	2,7	9,6
	V	3,5	4,0	4,3	4,7	3,6	3,6	3,3	2,6	2,8	2,7	3,2
	Su	**44,7**	**58,6**	**63,5**	**66,2**	**60,2**	**45,5**	**32,0**	**33,4**	**45,3**	**63,3**	**90,7**
	U	12,5	12,0	13,6	14,2	13,9	13,6	18,7	22,4	27,5	32,1	36,3
Leck (Schleswig)	L	9,1	8,5	8,6	7,8	6,5	4,3	2,8	2,8	3,7	8,5	11,7
	VE	13,3	15,5	14,9	10,8	9,6	5,9	2,1	1,6	4,2	5,5	14,2
	K	2,5	1,8	2,3	0,4	0,3	0,5	−0,2	−0,7	0,0	1,4	2,4
	V	5,1	6,1	4,3	5,1	3,7	2,0	1,5	2,0	2,5	2,5	2,4
	Su	**30,0**	**31,9**	**30,1**	**24,1**	**20,1**	**12,7**	**6,2**	**5,7**	**10,4**	**17,9**	**30,7**
	U	12,8	12,6	11,6	11,7	10,8	7,9	9,7	13,9	16,9	20,2	23,8
Meldorf (Holstein)	L	2,3	3,1	3,1	3,1	3,0	2,6	2,2	3,1	3,9	5,4	5,5
	VE	11,0	9,9	8,7	10,3	9,2	6,3	2,4	2,2	4,6	5,4	10,6
	K	0,0	0,0	0,3	0,5	0,3	0,1	0,1	0,2	0,8	0,1	0,7
	V	5,8	4,6	5,2	5,8	4,5	2,2	1,7	2,8	2,5	2,3	2,0
	Su	**19,1**	**17,6**	**17,3**	**19,7**	**17,0**	**11,2**	**6,4**	**8,3**	**11,8**	**13,2**	**18,8**
	U	9,7	8,9	9,8	11,4	12,7	8,7	10,4	17,1	17,5	18,6	22,5
Neumünster	L	15,0	18,3	16,2	15,6	14,2	11,7	7,6	7,5	9,6	14,7	16,0
	VE	8,3	15,2	17,3	14,8	10,3	9,2	7,1	4,6	8,8	15,2	26,1
	K	3,4	3,1	4,4	3,2	1,3	1,1	0,2	0,6	2,1	3,8	5,6
	V	7,1	5,2	6,2	6,1	4,9	4,6	2,5	2,8	2,7	2,6	3,9
	Su	**33,8**	**41,8**	**44,1**	**39,7**	**30,7**	**26,6**	**17,4**	**15,5**	**23,2**	**36,3**	**51,6**
	U	15,9	15,7	16,0	15,9	14,2	13,7	20,0	24,5	29,8	32,9	40,2
Oldenburg (Holstein)	L	4,0	4,8	5,2	4,7	5,0	4,6	3,0	2,9	3,3	5,4	6,6
	VE	10,3	9,1	9,4	10,7	11,8	8,2	3,9	3,1	9,0	11,3	21,0
	K	0,3	0,2	2,9	1,6	4,0	6,6	13,7	11,4	10,6	6,0	12,7
	V	11,9	9,8	8,2	11,1	12,0	6,2	6,5	7,5	6,2	6,3	6,6
	Su	**26,5**	**23,9**	**25,7**	**28,1**	**32,8**	**25,6**	**27,1**	**24,9**	**29,1**	**29,0**	**46,9**
	U	11,5	11,9	12,6	12,6	14,8	13,1	18,7	22,0	24,2	26,6	30,3

Figure 2.13: Excerpt from the original source of the tax revenue data.
Source: Statistisches Reichsamt (1941).

2.7.2 Additional Figures

We present additional data in the following figures. Figure 2.14 shows the population density in Germany in 1926 to give a general idea of the regional demography. The most densely populated regions were the basins of the Rhine, Ruhr, Neckar, and Main rivers in the West and Southwest, the mountainous regions in the East, including Silesia, and the large cities such as Hamburg, Munich and Berlin. Varying degrees of population density go along with differences in economic structure and are ultimately reflected in the tax statistics.

Figure 2.15 shows the number of corporations liable to corporate tax relative to the number of inhabitants. The figure complements Figure 2.6 in Section 2.3, which shows the number of corporations relative to the area of a tax district. Figure 2.15 presents a rather uneven pattern, which probably stems from the relative rare occurrence of corporations in general, and specific local conditions that result in one region having more corporations than another.

The last figure contains data from the declared income tax statistics. The figure compares, on the left side, the declared incomes with tax liabilities at the level of tax districts. On the right side, declared incomes are compared to tax revenues. The figure complements Figure 2.5 in Section 2.3, which made a similar comparison, but using the tax base of the declared income tax instead of declared incomes. The main takeaway from this figure is that it closely resembles Figure 2.5, implying that the tax exemptions that need to be subtracted from declared incomes to obtain the tax bases have little impact on the regional correlations shown.

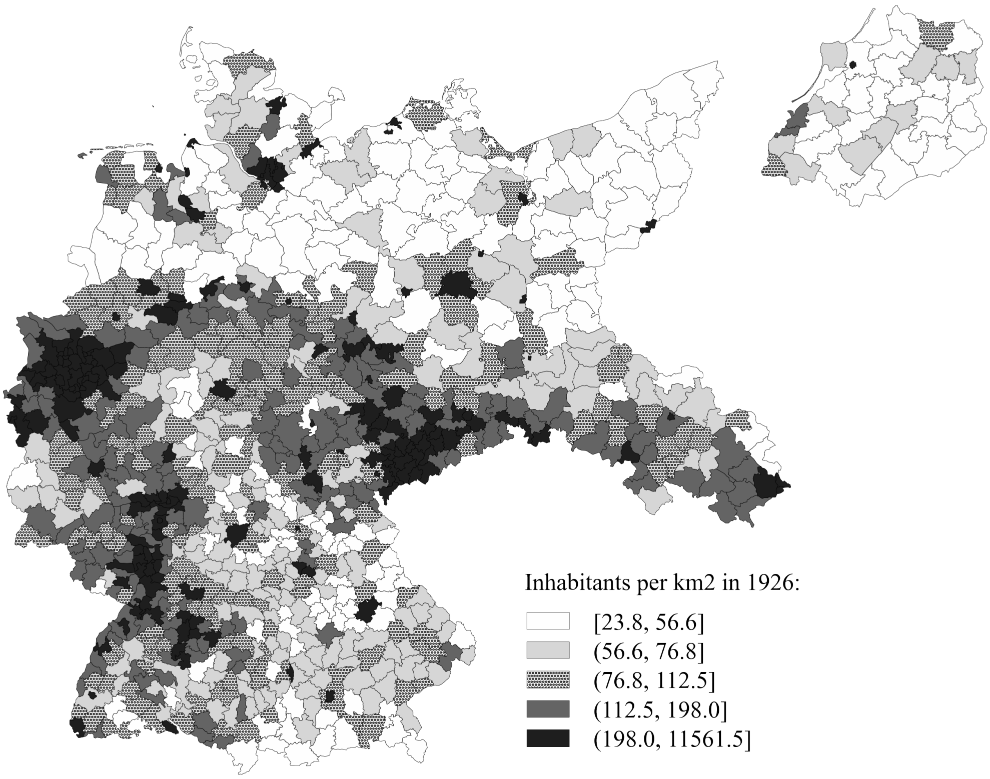


Figure 2.14: Map of population density in 1926.

Note: The map shows, for each tax district, the number of inhabitants per square kilometer in 1926. Tax districts are categorized by quintiles of the depicted variable, such that each color group comprises 20 percent of the tax districts. Source: Statistisches Reichsamt (1941). Calculations are our own.

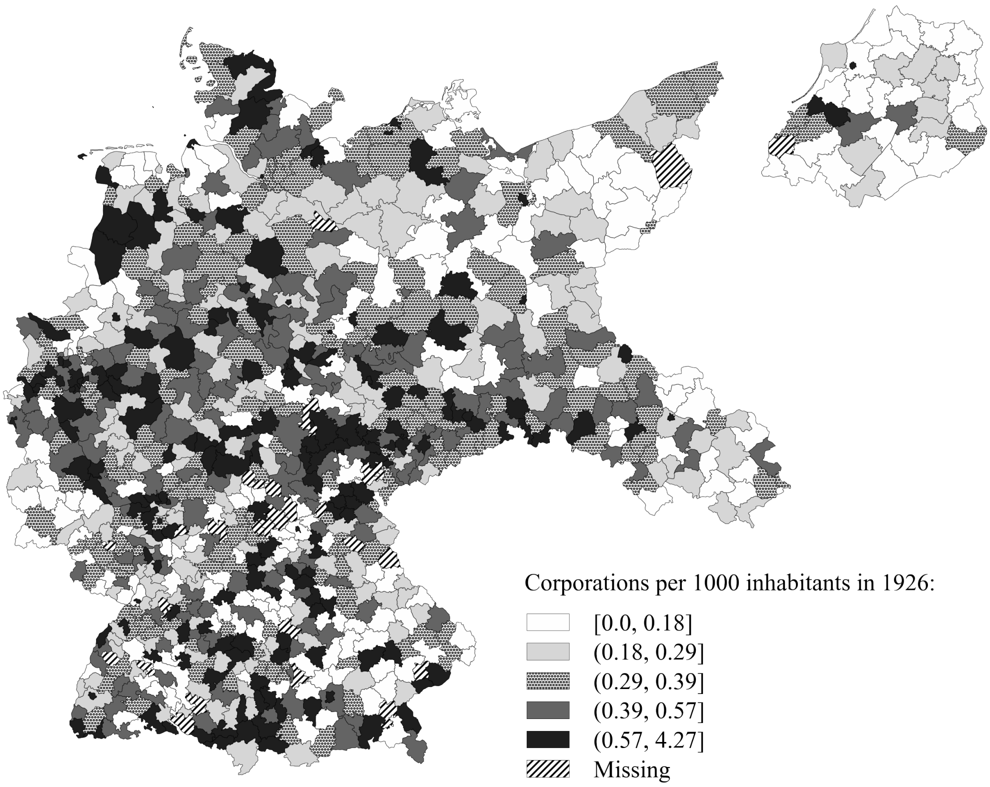
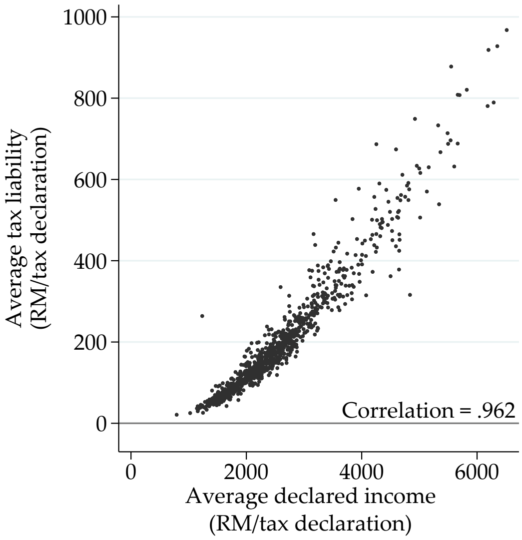
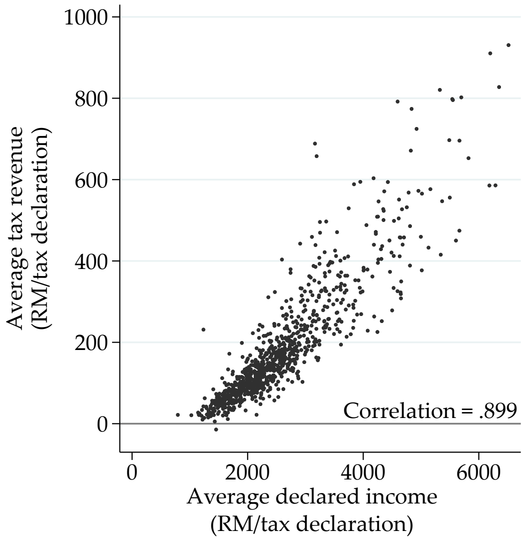


Figure 2.15: Regional distribution of corporations liable to the corporate tax per inhabitant in 1926.
Note: The map shows, for each tax district, the number of corporations liable for corporate tax in 1926 relative to the number of inhabitants in 1000. All data are assigned to the tax district of the headquarters of a corporation. Tax districts are categorized by quintiles of the depicted variable, such that each color group comprises 20 percent of the tax districts. Source: Statistisches Reichsamt (1931a, 1941). Calculations are our own.



(a) Declared income and tax liability



(b) Declared income and tax revenue

Figure 2.16: Indicators of the declared income tax for German tax districts in 1926 (part II).

Note: Each dot represents a tax district and shows the declared gross income, income tax liability, and income tax revenue of that district averaged over all income tax declarations within a district. Declared gross income corresponds to income net of expenses and costs. Tax exemptions are not subtracted. Declared gross income (*Einkommen*) and tax liabilities (*festgesetzte Steuer*) are taken from the *Steuersoll* statistics, while tax revenues are taken from the *Steuerist* statistics. All data are assigned to the tax district in which a household resided. Data for 901 tax districts. Source: Statistisches Reichsamt (1931a), Statistisches Reichsamt (1941). Calculations are our own.

2.7.3 Geocoding of Tax Districts

In the following, we describe in more detail the steps involved in geocoding the historical tax districts. The geocoding started with a historical map, which was published in Statistisches Reichsamt (1929b) and shows the tax districts of Germany in 1925. We converted the scan into a geocoded image, that is, an image in which each pixel is located in a geographic coordinate system. For the conversion, we determined a set of reference points that were uniquely identifiable in the scan and a geocoded reference map. As reference map, we used a map of German administrative districts provided by MPIDR and CGG (2011), so that the reference points mostly corresponded to prominent borderlines, coastal strips, or islands. The points were fed into an algorithm provided by the GRASS GIS software, which calculated for each pixel of the scan the geographic position, resulting in the geocoded image (Figure 2.17) (GRASS Development Team, 2017).

In the next step, we converted the geocoded image into a geocoded vector file (shapefile). The final shapefile should contain only the borders of the tax districts, and these should be vector objects rather than pixels. To achieve this, we manually traced the borders shown in the superimposed geocoded image in the QGIS software (QGIS.org, 2021). Many tax district borders coincided with those of administrative districts, since both administrative divisions are similar. In these cases, we copied the administrative district borders from the shapefile provided by MPIDR and CGG (2011) rather than tracing them (Figure 2.18). This procedure seemed more accurate and also ensured greater consistency with the maps of MPIDR and CGG (2011).

Having obtained a shapefile for 1925, we continued by successively creating shapefiles for the following years. We relied on information on border changes reported in sources (3) to (5) and referred to in Section 2.2. Specifically, source (3) stated in great detail which municipalities had changed from one tax district to the other at a given point in the year. In addition, source (5), of which we show an excerpt in Figure 2.19, contained descriptions of all tax district demarcations and served for cross-checking purposes. To incorporate border changes, we located the affected municipalities in OpenStreetMap maps and assigned them to the tax districts in accordance with the sources (OpenStreetMap contributors, 2019). During this procedure, we also superimposed the maps of administrative districts for reference, in case the administrative districts changed in conjunction with the tax districts. As stated in Section 2.2, in some cases, we had to draw new borders freehand around the municipalities that changed from one tax district to the other. In other cases, we relied on the borders of the administrative districts to obtain the new tax district borders.

For the incorporation of border changes, we created, for each year, a "map of changes" rather than a finished map. The map of changes included only those tax districts that were altered from one year to the other. Afterwards, we replaced all tax districts in the previous year's map with the altered tax districts using Python code to obtain the finished map. This was a very useful procedure, because in some cases we initially overlooked a change or incorporated the wrong one.[63] When we discovered the error, we could simply correct the specific map of changes and the Python code would automatically incorporate the correction into all maps for the following years.

Finally, we used the yearly tax district maps to create maps for tax states by grouping all the tax districts that belonged to a tax state using information from Statistisches Reichsamt (1941).

[63] As reported in Section 2.7.4, our sources sometimes contained inconsistent information.

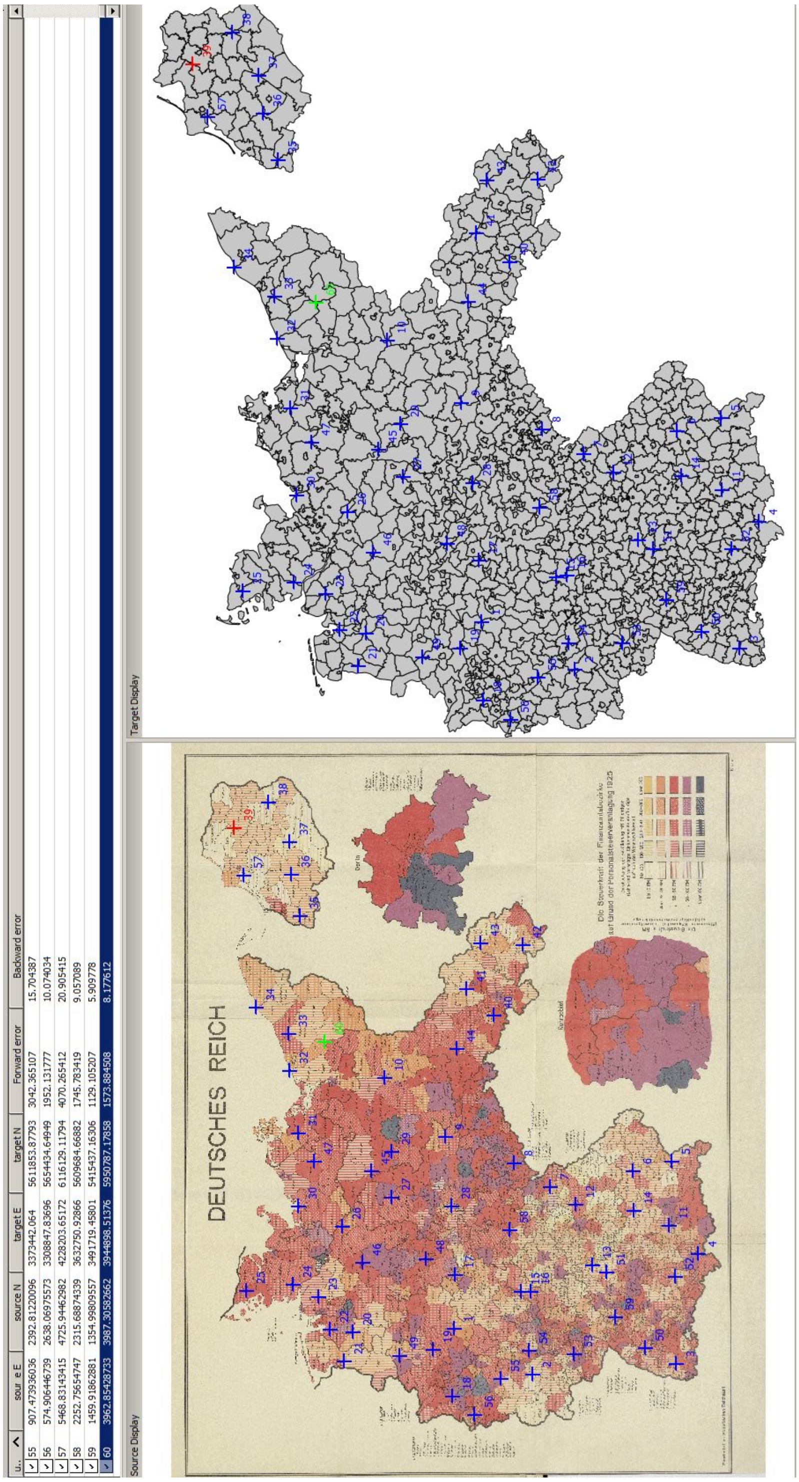
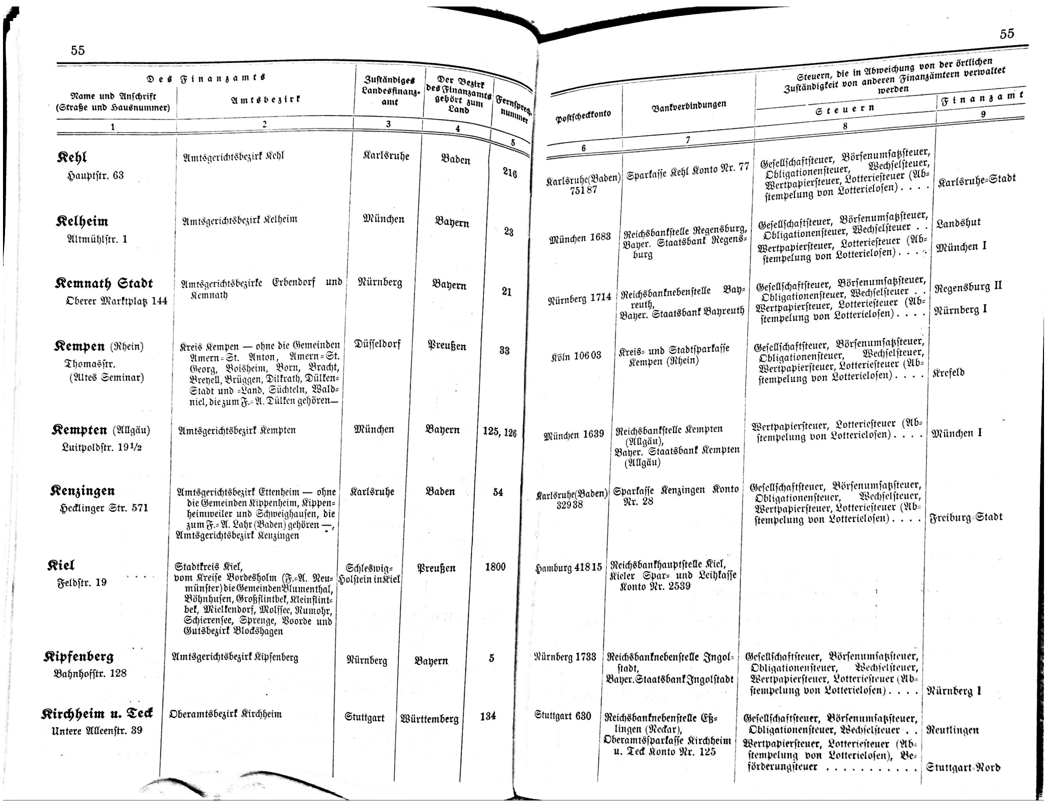


Figure 2.17: Determination of reference points to obtain a geocoded image.

Note: The left side of the figure shows a scan of a tax district map in 1925. The right side shows geocoded administrative districts (*Landkreise*) for the same year. Blue crosses indicate reference points that were uniquely identifiable in both maps and that served to geocode the scanned map on the left. Source: Statistisches Reichsamt (1929b), MPIDR and CGG (2011), GRASS GIS. Calculations are our own.

Figure 2.18: Creation of a vector file with the borders of tax districts (detail).

Note: The green lines are newly created tax district borders based on the underlying georeferenced image. The blue lines correspond to geocoded borders of administrative districts (Landkreise). Source: Statistisches Reichsamt (1929b), MPIDR and CGG (2011), QGIS. Calculations are our own.



55 | 55

Des Finanzamts: Name und Anschrift (Straße und Hausnummer)	Des Finanzamts: Amtsbezirk	Zuständiges Landesfinanzamt	Der Bezirk des Finanzamts gehört zum Land	Fernsprechnummer	Postscheckkonto	Bankverbindungen	Steuern, die in Abweichung von der örtlichen Zuständigkeit von anderen Finanzämtern verwaltet werden: Steuern	Steuern, die in Abweichung von der örtlichen Zuständigkeit von anderen Finanzämtern verwaltet werden: Finanzamt
1	2	3	4	5	6	7	8	9
Kehl Hauptstr. 63	Amtsgerichtsbezirk Kehl	Karlsruhe	Baden	216	Karlsruhe (Baden) 75187	Sparkasse Kehl Konto Nr. 77	Gesellschaftsteuer, Börsenumsatzsteuer, Obligationensteuer, Wechselsteuer, Wertpapiersteuer, Lotteriesteuer (Abstempelung von Lotterielosen)	Karlsruhe-Stadt
Kelheim Altmühlstr. 1	Amtsgerichtsbezirk Kelheim	München	Bayern	23	München 1683	Reichsbankstelle Regensburg, Bayer. Staatsbank Regensburg	Gesellschaftsteuer, Börsenumsatzsteuer, Obligationensteuer, Wechselsteuer . . Wertpapiersteuer, Lotteriesteuer (Abstempelung von Lotterielosen)	Landshut München I
Kemnath Stadt Oberer Marktplatz 144	Amtsgerichtsbezirke Erbendorf und Kemnath	Nürnberg	Bayern	21	Nürnberg 1714	Reichsbanknebenstelle Bayreuth, Bayer. Staatsbank Bayreuth	Gesellschaftsteuer, Börsenumsatzsteuer, Obligationensteuer, Wechselsteuer . . Wertpapiersteuer, Lotteriesteuer (Abstempelung von Lotterielosen)	Regensburg II Nürnberg I
Kempen (Rhein) Thomasstr. (Altes Seminar)	Kreis Kempen — ohne die Gemeinden Amern-St. Anton, Amern-St. Georg, Boisheim, Born, Bracht, Breyell, Brüggen, Dilkrath, Dülken-Stadt und -Land, Süchteln, Waldniel, die zum F.-A. Dülken gehören —	Düsseldorf	Preußen	33	Köln 10603	Kreis- und Stadtsparkasse Kempen (Rhein)	Gesellschaftsteuer, Börsenumsatzsteuer, Obligationensteuer, Wechselsteuer, Wertpapiersteuer, Lotteriesteuer (Abstempelung von Lotterielosen)	Krefeld
Kempten (Allgäu) Luitpoldstr. 19½	Amtsgerichtsbezirk Kempten	München	Bayern	125, 126	München 1639	Reichsbankstelle Kempten (Allgäu), Bayer. Staatsbank Kempten (Allgäu)	Wertpapiersteuer, Lotteriesteuer (Abstempelung von Lotterielosen)	München I
Kenzingen Hecklinger Str. 571	Amtsgerichtsbezirk Ettenheim — ohne die Gemeinden Kippenheim, Kippenheimweiler und Schweighausen, die zum F.-A. Lahr (Baden) gehören —, Amtsgerichtsbezirk Kenzingen	Karlsruhe	Baden	54	Karlsruhe (Baden) 32938	Sparkasse Kenzingen Konto Nr. 28	Gesellschaftsteuer, Börsenumsatzsteuer, Obligationensteuer, Wechselsteuer, Wertpapiersteuer, Lotteriesteuer (Abstempelung von Lotterielosen)	Freiburg-Stadt
Kiel Feldstr. 19	Stadtkreis Kiel, vom Kreise Bordesholm (F.-A. Neumünster) die Gemeinden Blumenthal, Böhnhusen, Großflintbek, Kleinflintbek, Mielkendorf, Molfsee, Rumohr, Schierensee, Sprenge, Voorde und Gutsbezirk Blockshagen	Schleswig-Holstein in Kiel	Preußen	1800	Hamburg 41815	Reichsbankhauptstelle Kiel, Kieler Spar- und Leihkasse Konto Nr. 2539		
Kipfenberg Bahnhofstr. 128	Amtsgerichtsbezirk Kipfenberg	Nürnberg	Bayern	5	Nürnberg 1733	Reichsbanknebenstelle Ingolstadt, Bayer. Staatsbank Ingolstadt	Gesellschaftsteuer, Börsenumsatzsteuer, Obligationensteuer, Wechselsteuer, Wertpapiersteuer, Lotteriesteuer (Abstempelung von Lotterielosen)	Nürnberg I
Kirchheim u. Teck Untere Alleenstr. 39	Oberamtsbezirk Kirchheim	Stuttgart	Württemberg	134	Stuttgart 630	Reichsbanknebenstelle Eßlingen (Neckar), Oberamtssparkasse Kirchheim u. Teck Konto Nr. 125	Gesellschaftsteuer, Börsenumsatzsteuer, Obligationensteuer, Wechselsteuer . . Wertpapiersteuer, Lotteriesteuer (Abstempelung von Lotterielosen), Beförderungsteuer	Reutlingen Stuttgart-Nord

Figure 2.19: Excerpt from the tax district directory 1926.
Source: Deutsches Reich Reichsfinanzministerium (1926b)

2.7.4 Discrepancies Between the Historical Sources

In the following, we report discrepancies regarding border changes reported in the source of the tax revenue data (Statistisches Reichsamt, 1941) and those reported in the financial gazettes (source 3) or the tax district directories (source 5). In most cases, these discrepancies consist of sources 3 and 5 reporting changes that are not specified in Statistisches Reichsamt (1941). We incorporated these border changes into the geocoded maps if not stated otherwise.

1927:

- The municipality Wilhelmsburg changed from tax district Harburg-Land to Harburg-Stadt, which was renamed Harburg-Wilhelmsburg (Deutsches Reich Reichsfinanzministerium, 1927).

1928:

- Municipalities Weddewarden, Speckenbüttel, and Leherheide changed from tax district Wesermünde-Land to Wesermünde-Stadt as implied by Deutsches Reich Reichsfinanzministerium (1934b) and the population numbers implied by Statistisches Reichsamt (1941).

1929:

- Dissolution of tax district Volkach: The municipalities Dipach, Neusetz, Oberpleichfeld, Prosselsheim, Seligenstadt, and Püssensheim changed from tax district Kitzingen to Würzburg (Deutsches Reich Reichsfinanzministerium, 1929a).
- The municipalities Steißlingen, Volkershausen, Beuren an der Aach, and Wiechs changed from tax district Stockach to Singen.
- The municipalities Marienwerder and Leinhausen changed from tax district Neinburg and Hannover-Land to Hannover-Stadt (Deutsches Reich Reichsfinanzministerium, 1929a).
- The municipality Weilimdorf changed from tax district Leonberg to tax district Stuttgart. This change is not reported in the gazettes, but is implied by Deutsches Reich Reichsfinanzministerium (1934b) and underlying changes of the administrative districts (Landkreise) of that time.
- Deutsches Reich Reichsfinanzministerium (1929a) lists some municipalities (and Gutsbezirke) that were incorporated into tax district Köngisberg-Land. However, several of these municipalities do not exist anymore. As a result, it was unclear how Königsberg-Land was affected. We left it unchanged.

1930:

- Redistribution of several municipalities between the tax districts Kemnath, Waldsassen, and Weiden (Deutsches Reich Reichsfinanzministerium, 1930).
- The municipalities Felkendorf, Limmersdorf, and Rimlas changed from tax district Kulmbach to Bayreuth (Deutsches Reich Reichsfinanzministerium, 1930).
- In 1929, the administrative borders of the Ruhr regions were redefined. These changes affected the borders of almost all tax districts in that region to a minor or major degree in 1930 (Deutsches Reich Reichsfinanzministerium, 1929a, 1930). Changes of the following districts are not reported in Statistisches Reichsamt (1941): Buer, Dinslaken, Dülken, Geldern, Gelsenkirchen, Gladbeck, Grevenbroich, Hagen, Hamm, Hattingen, Herne, Iserlohn, Kempen, Moers, Mühlheim, Ohligs, Opladen, Remscheid, Rheydt, Schwelm, Wesel, Wuppertal-Barmen.
- In 1930, the borders of the tax district Ohligs were changed and the district was renamed Solingen-West. Statistisches Reichsamt (1941) does not report any tax data separately for Ohligs since 1930, but includes it in the data for Solingen.

<u>1932:</u>

- The municipalities Beuren and Großen-Buseck changed from tax district Grünberg (Hessen) to Gießen (Deutsches Reich Reichsfinanzministerium, 1932).
- The municipalities Wiltsch (tax district Frankenstein) and Neu Wilmsdorf (Habelschwerdt) changed to tax district Glatz (Deutsches Reich Reichsfinanzministerium, 1932).
- The municipalities Kosemitz and Zülzendorf changed from tax district Strehlen to Frankenstein (Deutsches Reich Reichsfinanzministerium, 1932).
- The municipalities Algersdorf, Berzdorf, Deutsch Neudorf, Dobrischau, Haltauf, Korschwitz, Kraßwitz, Kummelwitz, Kunern, Münchhof, Neobschütz, Neu Karlsdorf, Pleßguth, Schildberg, Schönjohnsdorf, and Waldneudorf changed from tax district Münsterberg (Schlesien) to Strehlen (Deutsches Reich Reichsfinanzministerium, 1932).
- From the tax district Hirschberg (Riesengebirge), the municipalities Ketschdorf and Seitendorf changed to tax district Bolkenhain, and the municipalities Rothenzechau and Röhrsdorf changed to Landeshut (Schlesien) (Deutsches Reich Reichsfinanzministerium, 1932).
- The municipalities Haasel, Prausnitz, Laasnig, and Hänchen changed from tax district Jauer to Goldberg (Schlesien), and the municipality Siegendorf changed from Goldberg (Schlesien) to Liegnitz (Deutsches Reich Reichsfinanzministerium, 1932).

- The municipality Borganie changed from tax district Neumarkt to Schweidnitz (Deutsches Reich Reichsfinanzministerium, 1932).
- The municipalities Keldenich and Wesseling changed from Landkreis Bonn to Landkreis Köln and thus from tax district Bonn to Köln (Deutsches Reich Reichsfinanzministerium, 1932).
- The scanned map (Statistisches Reichsamt, 1929b) suggests that the municipalities Beuron and Irndorf belong to the tax district Spaichingen. When Spaichingen was dissolved in 1932, there is no hint in our sources which tax district obtained these two municipalities. We therefore assigned Beuron to the tax district Sigmaringen and Irndorf to Tuttlingen, which were the corresponding administrative districts (Landkreise) of that time.

1933:

- The municipalities Naumburg a. Bober, Alt Kleppen, Groß Dobritsch, Groß Reichenau, Klein Dobritsch, Kosel, Kottwitz, Kunzendorf, Neu Kleppen, Neuwaldau, Paganz, Peterswaldau, Popowitz, Poydritz, Reichenbach, Schöneich, Theuren, Tschirkau, and Zedelsdorf changed from tax district Grünberg (Schles.) to Freystadt (Niederschles.) (Deutsches Reich Reichsfinanzministerium, 1933).
- We reduced the size of tax district Biedenkopf at the expense of tax district Frankenberg in 1933, although Statistisches Reichsamt (1941) reports that tax district Biedenkopf changed in 1932. We chose 1933 because Statistisches Reichsamt (1941) also reports to the contrary that Frankenberg changed in 1933 and that the implied population grew between 1932 and 1933. The population of Biedenkopf shrank in 1932 and 1933. The financial gazettes report no change.

1934:

- The Gutsbezirk Staatsforstrevier Einsiedel changed from tax district Zschopau to Chemnitz-Land (Deutsches Reich Reichsfinanzministerium, 1934a).
- Tax district Rostock-Stadt was enlarged at the expense of Rostock-Land as implied by changes to the Landkreise of that time and Deutsches Reich Reichsfinanzministerium (1942)

1935:

- The island Helgoland changed from tax district Elmshorn to Cuxhaven in 1935 (Deutsches Reich Reichsfinanzministerium, 1935) after it had changed in 1934 from tax district Altona to Elmshorn.
- The municipality Großwaltersdorf changed from tax district Freiberg (Sachsen) to Flöha (Deutsches Reich Reichsfinanzministerium, 1935).

- The municipality Hohentanne changed from tax district Nossen to Freiberg (Sachsen) (Deutsches Reich Reichsfinanzministerium, 1935).
- The municipalities Fretzdorf and Gadow changed from tax district Neuruppin to Kyritz (Deutsches Reich Reichsfinanzministerium, 1935).
- The municipality Roßlau was incorporated into tax district Dessau as implied by administrative border changes (Landkreise) of that time and Deutsches Reich Reichsfinanzministerium (1942).

1936:

- The reshaping of the tax districts Erkelenz, Geilenkirchen, and Wassenberg: The border changes reported in (Deutsches Reich Reichsfinanzministerium, 1936) refer to "Ämter", and it is not exactly clear which municipalities changed from one district to another.[64] In the map, we changed the municipalities Breberen and Schierwaldenrath, which belonged to the Amt Gangelt in 1969, and the municipalities Waldenrath and Aphoven, which belonged to the Amt Waldenrath in 1969, from Wassenberg to Geilenkirchen (Landesregierung Nordrhein-Westfalen, 1969, p.393). The enlargement of Geilenkirchen is in line with a population growth of Geilenkirchen implicitly stated in Statistisches Reichsamt (1941). In addition, we changed the municipality Hückelhoven from tax district Erkelenz to Wassenberg, a change that is reported in (Deutsches Reich Reichsfinanzministerium, 1936).
- The municipalities Gevenich, Göimbach, and Körrenzig changed from tax district Erkelenz to Jülich according to changes of the corresponding Landkreise of that time and as implied by Deutsches Reich Reichsfinanzministerium (1942).
- The municipality Leipe changed from tax district Jauer to Bolkenhain (Deutsches Reich Reichsfinanzministerium, 1936).

1937:

- The municipality Elmschenhagen changed from tax district Plön to Kiel (Deutsches Reich Reichsfinanzministerium, 1937).
- The municipality Reupzig changed from tax district Dessau to Köthen (Deutsches Reich Reichsfinanzministerium, 1937).
- Zwickau-Stadt administered the taxes of corporations of Zwickau-Land starting in November 1936, including the payroll tax, deducted capital income tax, and the deductible levy for supervisory board members (Steuerabzug von Aufsichtsratsvergütungen) (Deutsches Reich Reichsfinanzministerium, 1936,

[64] "Ämter" were subdivisions of *Landkreise* and consisted of one or more municipalities (Deutsches Reich, 1872, § 21).

p.137). The tax data do not show any particular increases, but the population shifts significantly from Zwickau-Stadt to Zwickau-Land. Because of these these inconsistencies, we have merged the tax data and districts for both in 1937 and 1938.

Some border changes were only reported in the Statistisches Reichsamt (1941), but not in the financial gazettes, or both sources were inconsistent: 1927:

- The enlargement of Königsberg-Stadt at the expense of Königsberg-Land: We did not include this change in the map due to missing information on which municipalities changed.

1928:

- Enlargement of Harburg-Wilhelmsburg at the expense of Harburg-Land: The financial gazettes only report a change in 1927, which we incorporated. The population data underlying the tax data suggests, however, that there were border changes in 1927 and 1928. Due to missing information, we did not change the map in 1928.
- Statistisches Reichsamt (1941) reports border changes for the tax district Breslau-Stadt in 1928, while Deutsches Reich Reichsfinanzministerium (1928) states that those municipalities added to administrative entity Breslau-Stadt remain in their former tax districts Breslau-Land, Neumarkt, and Oels. We have left the tax districts of the map unchanged.
- Statistisches Reichsamt (1941) reports larger territorial changes for tax district (Bad) Schwalbach. Because the underlying administrative districts (Landkreise) changed in 1928 and no contradicting notes were reported in Deutsches Reich Reichsfinanzministerium (1934b), we adapted the tax districts accordingly in the map.

1932:

- Statistisches Reichsamt (1941) reports larger territorial changes for tax districts Rummelsburg and Neustettin. Because the underlying administrative districts (Landkreise) changed in 1932 and no contradicting notes were reported in Deutsches Reich Reichsfinanzministerium (1934b), we adapted the tax districts accordingly in the map (affecting also the tax district Köslin).
- Statistisches Reichsamt (1941) reports larger territorial changes for Aschendorf and Lingen. Because the underlying administrative entities changed in 1932 and no contradicting notes were reported in Deutsches Reich Reichsfinanzministerium (1934b), we adapted the tax districts accordingly in the map.

- Statistisches Reichsamt (1941) reports larger territorial changes for tax districts Aachen-Land und Monschau and Düren. Because the underlying administrative districts (Landkreise) changed in 1932 and no contradicting notes were reported in Deutsches Reich Reichsfinanzministerium (1934b), we adapted the tax districts accordingly in the map.
- Statistisches Reichsamt (1941) reports larger territorial changes for tax districts Ahrweiler and Mayen. Because the underlying administrative districts (Landkreise) changed in 1932 and no contradicting notes were reported in Deutsches Reich Reichsfinanzministerium (1934b), we adapted the tax districts accordingly in the map.
- The tax districts Schrobenhausen, Bad Schwalbach, and Diez were dissolved in 1932 according to Deutsches Reich Reichsfinanzministerium (1932), but were reestablished in 1933 (Deutsches Reich Reichsfinanzministerium, 1933). Statistisches Reichsamt (1941) reports no changes such that we left the districts unchanged in the map. Similarly, Waldbröl and Wassenberg were declared to be dissolved on March 4, 1933, but the dissolution was eliminated on May 26, 1933 (Deutsches Reich Reichsfinanzministerium, 1933).

1933:

- Statistisches Reichsamt (1941) reports that parts of the dissolved tax district Hünfeld were added to Hersfeld. Deutsches Reich Reichsfinanzministerium (1932) reports that Hünfeld was completely merged with Fulda, which is how we proceeded in the map.
- Statistisches Reichsamt (1941) reports that tax district Meißen received part of the dissolved tax district Radebeul. Deutsches Reich Reichsfinanzministerium (1933) reports that Radebeul was added entirely to Dresden. We changed the map according to the latter.
- The municipalities Raudten, Alt-Raudten, Brodelwitz, Gaffron, Mlitsch, Ober Dammer, Queißen, Töschwitz und Zedlitz were shifted from tax district Steinau (Oder) to Lüben on December 1, 1932 (Deutsches Reich Reichsfinanzministerium, 1932), but Statistisches Reichsamt (1941) reports it for 1933. We therefore implemented this in 1933.

1937:

- Statistisches Reichsamt (1941) reports larger territorial changes for tax districts Rostock-Stadt and Rostock-Land in 1937, but no such changes are reported by Deutsches Reich Reichsfinanzministerium (1937) or Deutsches Reich Reichsfinanzministerium (1938). In addition, the underlying administrative districts (Landkreise) remained unchanged and the population of Rostock-Land did not change significantly in 1937, such that we left the tax districts unaltered.

- Statistisches Reichsamt (1941) reports larger territorial changes for tax districts Ratzeburg and Lübeck, which very likely corresponded to exclaves of Lübeck that were incorporated into Ratzeburg. Because we generally did not map exclaves, we left both districts unchanged.

3 The Long-Term Effects of Destruction During the Second World War on Private Wealth in Germany

3.1 Introduction

In human history, social conflicts and natural disasters have often driven down economic inequalities. The Second World War (WWII) was certainly an extreme catastrophe. Not only did it destroy countless human lives as well as human and economic capital; it is also argued to have been a "great leveler" (Scheidel, 2017), an event that markedly reduced the high income and wealth concentration at the beginning of the twentieth century (Roine and Waldenström, 2015; World Inequality Database, 2017).

Compared to the pre-war decade, the period after the Second World War saw a significant decline in the concentration of income and wealth in many countries. In 1930, the top 1% of the population held 43% of total wealth in the United States, 42% in Germany, 50% in France, and 57% in the United Kingdom.[1] In the year after the end of the war, these shares were only 30%, 25%, 31%, and 46%, respectively. Results are comparable for many other countries' top 1% income shares.[2] As documented in Scheidel (2017) and Ransom (2019), the equalizing effect of the Second World War can be attributed to a number of causes, including physical destruction, expropriation and confiscation, resettlement, and interruptions in international trade and capital flows.

While the studies cited above provide historical time series on income and wealth concentration, we take a different and complementary perspective. Using heterogeneous variation in the destruction of housing stock in Germany, we quantify the extent to which WWII building destruction continues to reverberate in private wealth today. One focus here is on the implications for real estate wealth, which is the main wealth component for many households, but which has hardly been studied so far. Germany serves as our laboratory. Here, during the war, Allied bombs destroyed about 20 percent of the country's entire housing stock. The extent of the

[1] Figures for the United States, France, and United Kingdom from the World Income Database (Alvaredo et al., 2017). Figures for Germany from Albers et al. (2020).

[2] See Atkinson and Piketty (2007), Atkinson and Piketty (2010), Roine and Waldenström (2015), and Bartels (2019) for a more comprehensive picture.

destruction varied greatly, even across narrow regional entities. We use this regional variation to identify the effects on today's wealth distribution.

The data requirements for such an analysis are high: In addition to individual and regional control variables, it requires valid and regionally linkable data on the extent of bombing and on private wealth today. For the historical data, we have digitalized levels of destruction for municipalities larger than 3,000 inhabitants based on Gassdorf and Langhans-Ratzeburg (1950). To our knowledge, this is the most detailed digitalized database on WWII destruction in Germany. To these data, we added historical data on regional-level economic performance. We then linked the historical data with present-day data from the German Socio-Economic Panel (SOEP). SOEP not only provides wealth portfolios of the population today but also includes respondents' and their parents' birthplaces. Hence, it allows present-day wealth holdings to be linked with past regional destruction.

We expect that the destruction of housing during the Second World War will have a negative effect on wealth today. The mechanism directly connecting the two is the fact that WWII bombs destroyed the real estate of property owners as well as their heirs. There are also other potential mechanisms. One potential mechanism is education. Akbulut-Yuksel (2014) shows that bombing prevented children from going to school, resulting in fewer years of education. As higher education, ceteris paribus, implies higher lifetime earnings, and income is highly correlated with wealth, this mechanism should support our expectation. Another mechanism supporting our expectation is the adverse effect of combat activity on health (Li and Koulovatianos, 2020). Besides these mechanisms, which operate by way of human capital, macro-level mechanisms with unclear implications are conceivable. For example, Brakman et al. (2004) shows that wartime destruction caused cities to deviate from a random growth path. If real estate prices are related to city growth, this will also have long-term implications for wealth. As another example, Vonyó (2012, 2018) finds that the destruction of the housing stock leads to a spatial mismatch between labor and capital, resulting in lower productivity—and thus lower returns to capital—in urban areas after war.

We implement two empirical designs. First, we model households' wealth today as a function of the regional level of destruction. Second, we use the distance between a municipality and London as an instrumental variable, hereby following Vonyó (2012) and Akbulut-Yuksel (2014). In both designs, we control for a rich set of pre-war regional and city-level control variables. The dependent variable, wealth today, is described by net-of-debt wealth, net value of primary residence, and being the owner of primary residence. This allows us to gain comparatively detailed insights into the effects of wartime destruction on household wealth today. In a complementary mediation analysis, we study potential channels such as health, education, and work experience, through which the wartime destruction could have affected wealth accumulation.

Our empirical analysis focuses on two age cohorts. The first consists of persons born between 1931 and 1945. They were still children or adolescents during the war, and during the air attacks, their parents very likely lived in the cities where the children were born. For the first cohort, we estimate whether the level of regional destruction of the children's birthplaces matters for wealth holdings at the age of, on average, 65 years. The second cohort comprises persons whose mother or father was born between 1931 and 1945. Here we study whether the destruction of the parents' place of birth affects wealth at the age of about 40 years.

Our results suggest that wartime destruction has a significant negative effect on current wealth. Controlling for pre-war regional and city-level characteristics, estimates from our preferred model suggest a negative effect size of about −1,015 euros for individual net wealth per additional percentage point of destruction. Effect sizes are of similar magnitude for both birth cohorts. Analyzing the wealth portfolio in more detail, wartime destruction is most detrimental to real estate wealth. The mediation analysis points to education as an important mediator that, in the first-generation sample, contributes approximately one quarter to the total effect of destruction on net wealth.

The remainder of the paper is as follows. Section 3.2 summarizes the historical context of the Allied air war and gives an overview of related literature. Data and methods are presented in sections 3.3 and 3.4. Sections 3.5 and 3.6 discuss the results. Section 3.8 concludes.

3.2 Literature and Historical Context

3.2.1 Literature Review

There is an extensive literature on the socioeconomic consequences of war. In addition to the immediate impacts, which range from death and physical and psychological injury to the destruction of physical capital, the literature considers a wide range of other consequences. These include, above all, the consequences for the growth of income and wealth,[3] labor markets,[4] taxation and government spending,[5]

[3] See, for example, Ichino and Winter-Ebmer (2004), Lee (2005), Burchardi and Hassan (2013), Jürges (2013), Akbulut-Yuksel (2014), Piketty and Zucman (2014), Schiman et al. (2019), and Li and Koulovatianos (2020).

[4] See, for example, Neelsen and Stratmann (2011), Jürges (2013), Braun and Omar Mahmoud (2014), Lee (2014), and Schiman et al. (2019).

[5] See, for example, Chevalier et al. (2018).

health,[6] education,[7] population growth,[8] economic growth and productivity,[9] city size,[10] and consumption.[11]

Within this literature, one strand provides long-term time series assessing the distribution of certain outcomes before, during, and after periods of war. One of the most prominent works on the impacts on wealth and income is that of Piketty and Zucman (2014), who shows that income and wealth inequalities in many countries dropped sharply during and after the two world wars. For example, the top 1% income share dropped in Britain, Denmark, France, Germany, Japan, Sweden, and the United States between 1910 and 1950 from about 20% to 10% (Piketty and Zucman, 2014, pp.316-317). The top 1% wealth share dropped in the same period in the United States from about 45% to 30% and in various European countries from 60% to 40% (Piketty and Zucman, 2014, p.349). He also shows that inequalities trended upward again in the decades after the Second World War, particularly after 1980, resulting in a u-shaped inequality curve over the course of the last century. Piketty and Zucman (2014) and Scheidel (2017) argue that the reduction in inequality was caused to a large extent by wartime destruction and violent conflict. In this vein, Piketty writes that the "concentration of circumstances (wartime destruction, progressive tax policies made possible by the shocks of 1914–1945, and exceptional growth during the three decades following the end of World War II [...] created a historically unprecedented situation, which lasted for nearly a century." (Piketty and Zucman, 2014, p.356).

Another strand of literature shifts the focus from descriptions of long-term time series to the identification of causal effects of wars. In Table 3.1, we classify these studies along three dimensions: (1) type of treatment, (2) the outcome, and (3), the time gap between cause and effect, that is to say, short- versus long-term effects.

With respect to treatment type, the literature assesses four different war-related shocks: (1) bombing, (2) other combat activity, (3) war-induced migration flows, and (4) hunger and food shortages. Most of the studies use regional or temporal variation in treatment intensity for the identification of effects. Similar to our study, Akbulut-Yuksel (2014) uses regional variation in destruction intensity during WWII in her study on long-term effects on education and health, finding significant negative long-term effects of destruction. Other examples include Davis and Weinstein (2002)

[6]See, for example, Akbulut-Yuksel (2014), Kesternich et al. (2014), Lee (2014), Kesternich et al. (2015), van Ewijk and Lindeboom (2017), and Li and Koulovatianos (2020).

[7]For example, Ichino and Winter-Ebmer (2004), Neelsen and Stratmann (2011), Jürges (2013), Akbulut-Yuksel (2014), Lee (2014), Miguel and Roland (2011), Waldinger (2016), and Schiman et al. (2019).

[8]See, for example, Davis and Weinstein (2002).

[9]See, for example, Vonyó (2012, 2018), Davis and Weinstein (2008), and Braun and Kvasnicka (2012).

[10]See, for example, Davis and Weinstein (2002), Brakman et al. (2004) Bosker et al. (2007), and Bosker et al. (2008).

[11]See, for example, Kesternich et al. (2015).

and Brakman et al. (2004), who both use regional variations in WWII destruction levels—in Japan and Germany, respectively—to assess the effects on long-term city growth. For Japan, the authors find that destroyed cities grow faster after the war and return to their pre-war size within 20 years, while findings for Germany suggest that cities do not completely return to their pre-war size.

Table 3.1: Literature on the causal effects of wars.

Outcome	Treatment	Short-term effects	Long-term effects
Wealth	1) Bombing	-	This paper
	2) Combat activity	Lee (2005)	Kesternich et al. (2014), Li and Koulovatianos (2020)
	3) Migration	-	-
	4) Hunger	-	-
Economic activity (e.g. output, income, or consumption)	1) Bombing	Vonyó (2012, 2018)	Miguel and Roland (2011), Akbulut-Yuksel (2014), Wolf and Caruana-Galizia (2015)[12]
	2) Combat activity	-	Ichino and Winter-Ebmer (2004), Lee (2014)
	3) Migration	Braun and Kvasnicka (2012), Braun and Omar Mahmoud (2014), Braun and Dwenger (2017)	Burchardi and Hassan (2013)
	4) Hunger	-	Neelsen and Stratmann (2011), Jürges (2013), Kesternich et al. (2015)
Education	1) Bombing	Waldinger (2016)	Miguel and Roland (2011), Akbulut-Yuksel (2014), Waldinger (2016)
	2) Combat activity	-	Ichino and Winter-Ebmer (2004), Kesternich et al. (2014), Lee (2014)
	3) Migration	Waldinger (2016)	Waldinger (2016), Becker et al. (2020)

Continued on next page

[12]The treatment in Wolf and Caruana-Galizia (2015) is regional variation of homeownership rates instrumented by WWII bombing.

Outcome	Treatment	Short-term effects	Long-term effects
Education	4) Hunger	-	Neelsen and Stratmann (2011), Jürges (2013)
Health	1) Bombing	-	Akbulut-Yuksel (2014)
	2) Combat activity	-	Kesternich et al. (2014), Lee (2014), Li and Koulovatianos (2020)
	3) Migration	-	-
	4) Hunger	-	Jürges (2013), Kesternich et al. (2015), van Ewijk and Lindeboom (2017)
Population	1) Bombing	Davis and Weinstein (2002), Brakman et al. (2004), Bosker et al. (2007), Bosker et al. (2008), Davis and Weinstein (2008)	Davis and Weinstein (2002), Brakman et al. (2004), Bosker et al. (2007), Bosker et al. (2008), Davis and Weinstein (2008), Miguel and Roland (2011)
	2) Combat activity	-	-
	3) Migration	Schumann (2014), Braun and Dwenger (2017)	Schumann (2014)
	4) Hunger	-	-
Other	1) Bombing	-	-
	2) Combat activity	-	Kesternich et al. (2014) (life satisfaction, and marital status)
	3) Migration	Braun and Dwenger (2017) (marital status, political voting), Chevalier et al. (2018) (political voting)	Chevalier et al. (2018) (political voting)
	4) Hunger	-	-

In terms of outcomes, studies focus on a number of different aspects, which can be grouped broadly into six themes: (1) wealth, (2) economic activity, including income and consumption, (3) education, (4) health, (5) population growth, and (6) other outcomes. Some papers also analyze multiple outcomes and are listed more than once in the table. Notably, and despite the large descriptive literature on this topic, only a few papers analyze the causal effect of war on household wealth and portfolio composition. Exceptions include Lee (2005), Kesternich et al. (2014), and Li and Koulovatianos (2020). Lee (2005) shows that physical injuries and exposure to combat during the US Civil War had strong negative effects on subsequent savings, as did illnesses while in military service. In particular, veterans who served in a company that underwent more dangerous military missions had less personal wealth five years after the war. Kesternich et al. (2014) looks at the long-term effects of WWII combat activity on civilians who lived in European combat regions. With outcomes being measured in the first decade of the twentieth century, they find significant negative effects for health outcomes and education but no effects for financial wealth.[13] They justify the lack of effect on today's wealth by the fact that wealth is mainly determined by savings and asset prices after the war. Finally, Li and Koulovatianos (2020) analyze the effects of combat exposure during the Second Sino-Japanese War (1937–1945) and the Chinese Civil War (1946–1950) on health and wealth. They find a negative effect on wealth among persons who were children or exposed to war in utero, and show that a deterioration in health due to combat exposure is the main driver of the result.

As regards time gaps, some papers study the short-term effects of war. With respect to bombing, Vonyó (2012) analyzes the immediate postwar period in Germany and shows that the destruction of the housing stock led to a spatial mismatch of capital and labor, resulting in lower economic productivity. Waldinger (2016) shows that the destruction of university departments in Germany had negative effects on scientific output in the short term, but not in the long term. Similarly, many of the papers on bombing and city growth deal with short-term effects and their persistence (Davis and Weinstein (2002), Brakman et al. (2004), Bosker et al. (2007), Bosker et al. (2008), Davis and Weinstein (2008)).

Other papers show that war affects socio-economic outcomes several decades later. Similar to our paper, many of these papers rely on recent survey data on respondents who were treated in childhood or in utero and surveyed at an advanced age. For example, Kesternich et al. (2015) show that WWII-related hunger episodes during childhood have an effect on food consumption at the age of 50 to 80. Similarly, in the aforementioned study by Akbulut-Yuksel (2014), the time gap between treatment and effect is 40 years, while Li and Koulovatianos (2020) show that war has a significant negative effect on wealth stocks 60 or more years after treatment.

3.2.2 The Allied Bombing Campaign on German Territory

The identification strategy in this paper is based on regional differences in the extent of property destruction. The Allied bombing campaign in the Second World War was a massive military operation that inflicted heavy damage on German cities, infrastructure, and industrial centers, destroying about 20 percent of industrial capital and residential housing stock in Germany(Albers, 1989). Initiated in September

[13] Note that the paper does not consider destruction, and that the treatment is defined rather broadly as either living in a country that fought in WWII or living in a region in which combat took place.

1939, the air raids on German territory varied greatly in intensity over the years, and the majority of damage was inflicted between summer 1942 and spring 1945, when the US Army Air Forces entered the war in support of the British Royal Air Force.

Bombing targets were not chosen at random, but the various goals and scope of the bombings introduced an important element of randomness in terms of the populations and regions that were hit. The Allied air attacks followed three main goals: (1) to damage specific production sites of crucial industries such as the ball bearing, oil, and aircraft industry, (2) to weaken the morale of the German population through area bombing of residential areas, (3) to support and clear the way for Allied ground troops on their way toward Berlin (The United States Strategic Bombing Survey, 1945; Hampe, 1963a). Especially the first two goals caused heavy damage to the broader population. Area bombing, which was implemented for the first time in spring 1942, consisted in sending formations of hundreds of planes to cause broad and heavy damage on populated areas within time spans of a few hours. The invasion of Germany after the liberation of France also brought with it heavy destruction, especially in West German cities along the border to the Netherlands, Belgium, and France, as Allied air forces used their unrivaled superiority to ensure safe passage for ground troops. Moreover, air attacks lacked precision and often hit unintended targets.[14]

Most of the planes carrying out the attacks flew from England, and to a minor degree from Italy and France after their liberation. There were few aerial attacks on Germany from the East, as the Soviet Red Army used their aircraft mainly in support of ground troops and did not strategically attack German cities or industrial centers (Hampe, 1963a). The result was that northwestern regions of Germany suffered more damage than eastern and southern regions, a pattern which we exploit for the instrumental variable estimation strategy explained in more detail in Section 3.4.2 (Hampe, 1963b).

For the protection of civilians, the Nazi German policy response was mainly to increase anti-air defense capabilities, provide air-raid shelters, and relocate civilians to rural sites. However, during the course of the war, Allied forces increasingly gained control of German air space and achieved technological superiority, rendering many defensive systems ineffective. Moreover, in the final years of the war, German policy prioritized the protection of strategic industries over the safety of civilians, ultimately leading to an estimated 370,000 to 390,000 German civilians killed by airstrikes (Groehler, 1990, p.320). In terms of wealth, estimates suggest that the air

[14] The United States Strategic Bombing Survey (1945) states “only about 20% of the bombs aimed at precision targets fell within [the] target area”, the target area being “a circle having a radius of 1000 feet [305 meter] around the aiming point of attack.”

attacks in Germany destroyed about 20 percent of national wealth relative to 1939 levels.[15]

After the war, the new German government was confronted with a severe housing and employment shortage, aggravated by the inflow of millions of German refugees from former Eastern territories.[16] Policy makers responded with a variety of measures, two of which are noteworthy with respect to the study of household wealth, as they potentially affect the observed long-term effects of the destruction. First, the German government introduced a one-off levy on wealth: the *Lastenausgleich*, which transferred money from those who had suffered no or little damage to those who had lost property. Although the levy was 50 percent on assets held in 1948, its redistributive impact should not be overestimated. On the one hand, beneficiaries of the levy were only compensated for part of their losses, and replacement rates decreased with the value of the damage.[17] On the one hand, from the perspective of those who had to pay the levy, the burden was not high, as the levy could be paid in annuities over 30 years, facilitated by the substantial economic growth of the 1950s and 1960s. A second set of policies aimed at the provision of subsidized housing and the construction of new, mostly rental homes. The effect was that between 1950 to 1961, the number of dwellings in West Germany and Berlin increased from 10.1 million to 16.1 million, while at the same time, the homeownership rate declined from 39.1 to 34.1 percent (Statistisches Bundesamt, 1955, 1964).[18] Although such policies very likely affected prices on real estate markets as well as investment decisions, their effect on post-war household wealth is unclear.

3.3 Data

Our study relies on two data sources. The first source is the historical destruction data for German municipalities provided by Gassdorf and Langhans-Ratzeburg (1950) (GLR), enriched with regional control variables capturing the pre- and post-war phase. The second source is a representative household panel dataset for Germany, the Socio-Economic Panel (SOEP). Since 2002, SOEP has been providing information on wealth, our main outcome variable, together with a broad set of

[15]Estimates summarized in Hampe (1963b, p.243) state that the total percentage of national wealth lost as a result of the war was 45, of which 12 percentage points are due to the loss of territories east of the Oder-Neiße line and 20 to 22 percentage points to air attacks.

[16]By 1953, the number of refugees in West Germany had reached 8.3 million people from former Eastern territories and 2 million from the Soviet Occupation Zone (Albers, 1989).

[17]Replacement rates decreased from 100 percent for damages up to 6,200 Reichsmark (approximately 19,000 euros in 2020) to a minimum rate of 3.5 percent for damages larger than two million Reichsmark (approximately six million euros).The minimum rate was increased to 6.5 percent in 1967. (Albers, 1989; Deutsche Bundesbank, 2019).

[18]The increase also includes 0.3 million dwellings in the Saarland region that were built in 1957 when the Saarland became part of Germany again.

control variables. For the analyses, all data sources are georeferenced and the historical data is linked to present-day wealth holdings using SOEP respondents' place of birth.

3.3.1 Historical Data

3.3.1.1 Levels of City Destruction

Gassdorf and Langhans-Ratzeburg (1950) assembled a comprehensive dataset on the destruction of German cities and municipalities.[19] It covers all 1,898 West German municipalities with more than 3,000 inhabitants [20] and provides the share of destroyed dwellings in 1945 relative to the total number of dwellings in 1939 for 1,739 of these municipalities. Unfortunately, GLR does not provide destruction information for municipalities in East Germany.

The GLR database builds on harmonized administrative sources, including federal statistical offices, ministries, and local administrations. Administrative agencies collected these data in the post-war period to allocate refugees and reconstruction funds. In GLR, a dwelling is classified as destroyed if it was more than 50 percent damaged.[21] According to this definition and taking the weighted average using municipalities' population sizes in 1939, about 29 percent of the buildings were destroyed.

The extent of destruction varies with city size and across regions. Figure 3.1 provides a scatter plot of municipality-level destruction levels and population sizes. At any level of city size, destruction levels exhibit large variation, while the destruction level, on average, increases with city size. While we will control for this relationship in the subsequent analyses, our identification strategy builds on the differences in destruction levels across regions. Figure 3.2 provides a heat map of regional destruction. To create this Figure, we georeferenced the GLR data using the so-called Geonames database.[22] The map shows that cities cluster in the central western area of Germany (what today is the state of North Rhine-Westphalia) and in the southwest, along the Rhine river. It is also in these regions where most of the destroyed cities are located. However, cities that were largely (50 percent or more) destroyed can be found in all German regions.

We assess the quality of the GLR data in two respects. First, we compare the average level of destruction according to GLR with two alternative highly aggregated

[19] We use the terms "city" and "municipality" interchangeably.

[20] The original GLR data contain 1,901 entries because three cities are reported twice.

[21] An exception are cities from the state of Bavaria. Here, only completely destroyed dwellings were classified as destroyed. For robustness, we conduct our main analysis excluding Bavarian cities.

[22] See http://www.geonames.org. Last accessed in October 2020. To each municipality we assigned the geo-coordinates of its center. Some of the municipalities were disbanded after 1939 and merged with neighboring cities, but they continue to exist as districts under their old name. In these cases, we assigned the geo-coordinates of the district center.

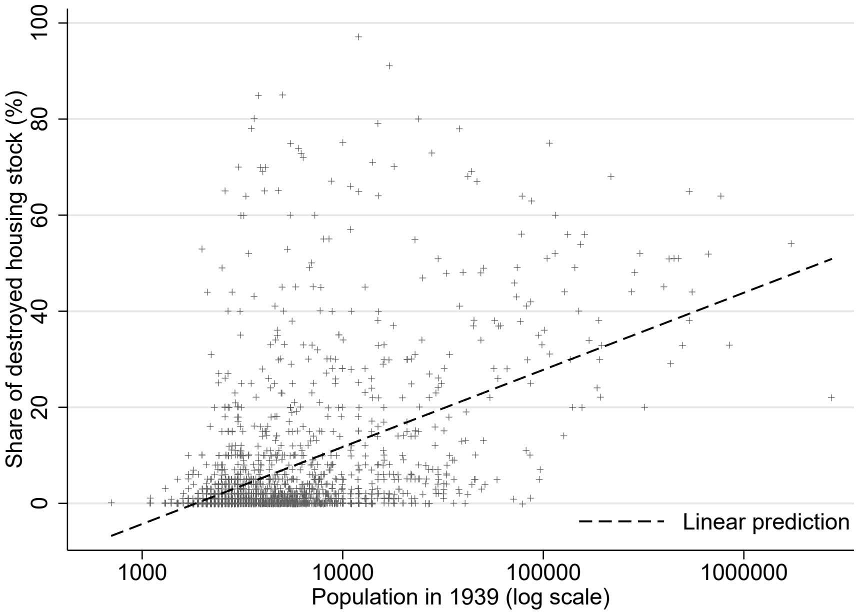


Figure 3.1: Destruction and population size

Note: The slope coefficient of the fitted line is 6.969, meaning that average destruction increases by approximately 0.07 percentage points for a one-percent increase in city size. Source: Gassdorf and Langhans-Ratzeburg (1950); own calculations.

data sources. The United States Strategic Bombing Survey (1945) and Albers (1989) estimate that 20 percent of dwelling units were destroyed or heavily damaged, thus indicating a slightly lower level of destruction than the GLR data. This is not surprising given that the GLR data do not contain very small municipalities, which were, on average, destroyed less than larger urban agglomerations. Second, we compare the GLR data with the destruction data for the largest 199 West German cities provided by Kästner (1949).[23] For these 199 cities, average destruction levels based on Kästner (1949) and GLR are very close, and the city-level destruction values form both data sources correlate highly (0.84).[24] Thus, both cross-validations suggest that the GLR data provide valid information on the levels of destruction of German municipalities.

[23]This is the data source used by Brakman et al. (2004), Vonyó (2012), Burchardi and Hassan (2013), Akbulut-Yuksel (2014), and Braun and Omar Mahmoud (2014) among others.

[24]Differences are largest for some cities in the state of North Rhine-Westphalia, for which the average destruction in Kästner (1949) is 6.5 percentage points higher. A footnote in Kästner (1949, p.368) states that their figures for North Rhine-Westphalia contain not only "completely destroyed" but also "heavily destroyed" dwellings, pointing to the possibility that the authors had to use a different definition of destruction for North Rhine-Westphalia than for the other states and a different one than used in the GLR data.

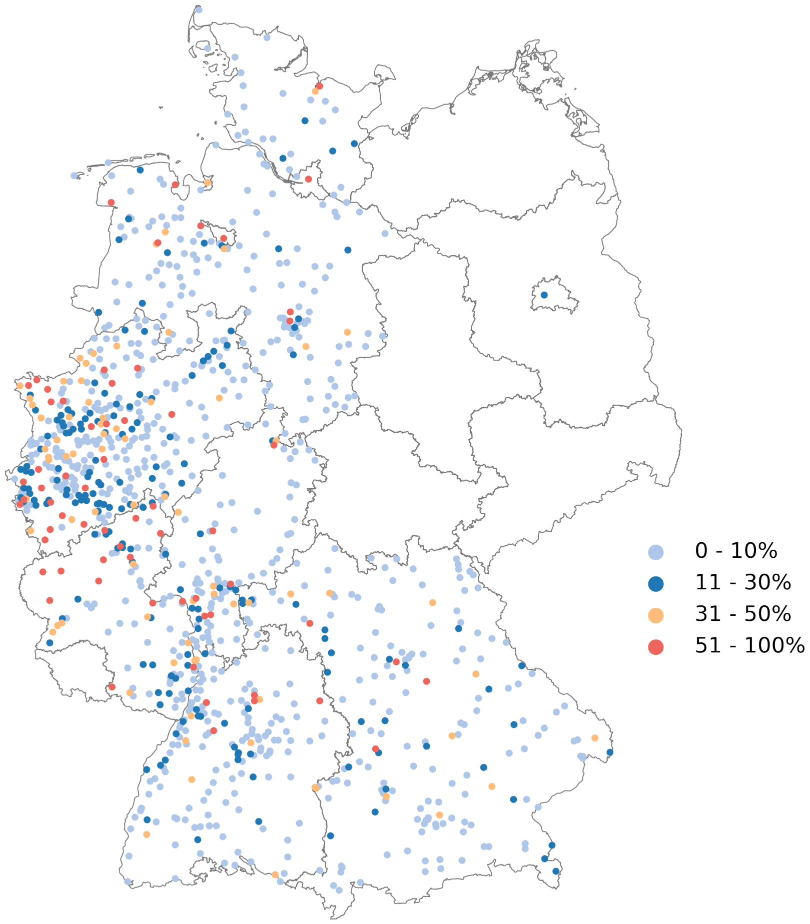


Figure 3.2: Share of destroyed housing stock in 1945 for cities with more than 3000 inhabitants. N = 1,739.
Source: Gassdorf and Langhans-Ratzeburg (1950); own calculations.

3.3.1.2 Additional Control Variables

In the analysis below, we include two types of regional variables to control for pre-war conditions:

(1) *Economic performance*. To capture differences in regional economic performance, we use data from Brockmann et al. (2022) at the level of tax districts on per-capita tax revenues from income, payroll, wealth, and corporate taxes in the year 1938. In total, we use data for 516 tax districts covering West Germany and Berlin.

(2) *Population density*. To capture structural differences between rural and urban regions, we use population densities at the level of 571 administrative districts—so-called Stadt- and Landkreise—in 1939 from Statistisches Re-

ichsamt (1944). Population densities are defined as inhabitants per square kilometer.

3.3.2 SOEP Data

The German Socio-Economic Panel (SOEP) is among the largest and longest-running representative panel surveys worldwide and is recognized for maintaining the highest standards of data quality and research ethics (Goebel et al., 2019). In 2019, the survey covered about 30,000 adults in 20,000 households. Since 1984, SOEP has provided both a broad set of self-reported "objective" variables, such as income, age, and gender, as well as many "subjective" indicators such as satisfaction with life and worries. Most importantly for our purposes, SOEP provides detailed household information on income portfolios and biographical data, including respondents' own and their parents' place of birth.

To cope with panel attrition, several refresher samples have been added to the SOEP to maintain the sample size. Further, to ensure the cross-sectional representativeness in the presence of influx to the underlying target population, several boost samples have been added.

3.3.2.1 Linking Historical and SOEP Data

We link the SOEP data with regional historical data using respondents' place of birth.

We match the municipality-level destruction data to SOEP respondents with the closest distance. For each person, we calculate the distance between her city of birth and all cities with destruction information and assign the destruction rate of the closest city to each person. We drop individuals for whom the closest distance exceeds five kilometers to ensure a close match between city of birth and actual destruction treatment.

We match the historical economic performance indicators using the georeferenced tax district borders by Brockmann et al. (2022) and the geocoded place of birth information from SOEP. Every SOEP respondent is matched to the tax district in which she was born. Analogously, we match SOEP respondents with the historical population densities using the georeferenced district borders provided by MPIDR and CGG (2011).

For the construction of what we refer to as the *first-generation sample* (see Section 3.3.2.3), we consider SOEP respondents who were born after 1930 and before 1946 and match them with the regional information about their own place of birth. Additionally, we investigate whether parents' exposure to WWII bombing had an effect on their children's wealth "today," motivated by the idea that the effects of destruction last across generations. To this end, to construct what we refer to as the *second-generation sample* (see Section 3.3.2.3), we match SOEP respondents to the destruction data using their fathers' and mothers' cities of birth. Figure 3.3 depicts the temporal sequence of birth, bombing exposure, and wealth surveying for all samples.

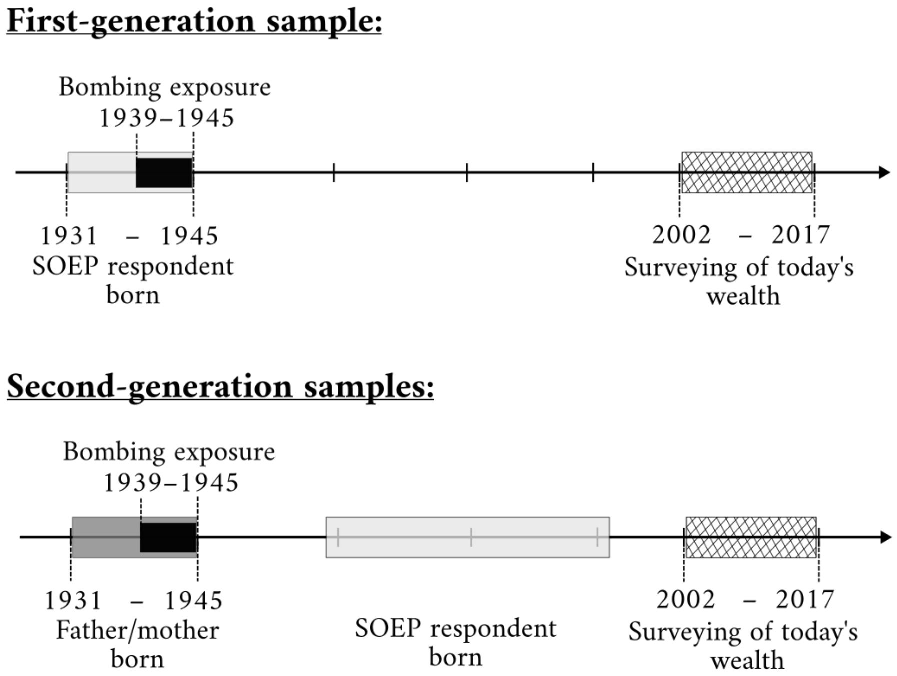


Figure 3.3: Timeline of events

3.3.2.2 Focal Variables

Our analyses build on two core pieces of information from the SOEP: wealth and place of birth.

SOEP has been surveying respondents on their wealth portfolios every five years since 2002 using the questionnaire module "my personal balance sheet." A unique feature of the SOEP study is that each adult respondent in a household provides her/his portfolio. This includes net (of debt) wealth, the net-of-debt value of the residential real estate that the respondent occupies[25] and also whether (or not) the respondent owns the building she occupies. This allows for direct linkage of an individual's real estate today with her birth place in the past. To cope with item-non response, SOEP provides each portfolio component in imputed form.[26]

Because SOEP is a panel and the wealth module was implemented four times between 2002 and 2017, for most respondents, portfolio information is available

[25]The value is only known if the respondent owns the residential property. For non-owners, we use a value of zero.
[26]For each wealth item, five imputations are provided.

at several time points. In our analysis, we use the earliest possible year for each respondent to limit the effects of old-age dissaving. Old-age dissaving is a potential threat to our analysis as it likely attenuates wealth differences between the wealthy and the poor, and also between those whose real estate was destroyed and those whose real estate was left intact.[27] We censor net wealth and the net-of-debt value of residential real estate at the 0.1th and 99.9th percentile to reduce biases from outliers.

As regards respondents' birth, SOEP provides the year and also geocoded place of birth (the latter since 2012). The parents' cities of birth were first collected in 2018, either indirectly, by asking SOEP respondents about their parents, or directly from the parents, provided they participated in SOEP. Unfortunately, respondents' own place of birth is not available for two SOEP subsamples—L2 and L3—which consequently do not enter the subsequent analysis. L2 and L3 constitute about 16.7 percent of all SOEP individuals in 2012 and comprise mainly young families with low income. The exclusion of the two samples should be innocuous for our results, as persons born 1945 and earlier constitute only around 0.5 percent of samples L2 and L3. Apart from this, the parental place of birth is not available for a smaller share of SOEP respondents who conducted paper- or web-based interviews as the questions about the parental place of birth were only used in computer-assisted personal interviews (CAPI).[28]

3.3.2.3 Construction of Working Samples

Our analyses rely on different SOEP working samples.

The *first-generation sample* includes SOEP respondents who were born after 1930 and before 1946, and thus were directly exposed to the bombings in childhood. As information in the GLR data is restricted to West German cities and Berlin, we exclude respondents from East Germany. In addition, we exclude respondents who *lived* part of their lives in the German Democratic Republic (GDR) as they experienced an additional wealth shock with the creation of a socialist system.

We apply analogous sample selection criteria to the two *second-generation samples* as we did for the *first-generation sample*. For the sample of fathers, we select individuals whose fathers were born between 1931 and 1945. The father's place of birth has to be in West Germany or Berlin, within five kilometers of a city with information on WWII destruction, and the respondent (not the father) may not have lived in the GDR.[29] In the sample of mothers, we apply analogous criteria.

[27] Similarly, selective deaths within the population also potentially affect our analysis. If poorer individual pass away at younger ages, they are less likely to be surveyed. Based on this assumption, selective death also leads to an underestimation of potential effects.

[28] CAPI interviews constituted about 76 percent of all interviews in 2018 Further, parental information is available for non-CAPI interviewees if parents themselves participated in SOEP.

[29] The data do not allow us to determine whether parents lived in the GDR. However, as the focus is on parents born in West Germany before 1946, almost all of the children were born before 1990,

Table 3.2: Dropped number of observations due to sample selection criteria and final sample sizes.

	First gen.	Second gen. fathers	Second gen. mothers
Initial sample (N)	53415	53,415	53,415
Sample selection criterium:		Dropped (N):	
+ Born 1931-1945	-46277	−40,117	−40,701
+ Did not live in GDR	-1791	−2,954	−2,787
+ With geocoded birth place	-2968	−8,539	−8,204
+ Birth place in West Germany or Berlin	-491	−474	−438
+ Birth place within 5km to city from GLR data	-311	−280	−265
+ Non-missing destruction and population	-42	−29	−34
Final sample (N)	1535	1022	986

Note: The initial sample consists of all individuals who participated in at least one of the four survey waves that included the wealth questionnaire (2002, 2007, 2012, or 2017). Source: SOEP v35; own calculations.

The effect of these sample selection criteria for sample sizes is presented in Table 3.2. The table shows that from the initially 53,415 individuals in the SOEP for whom wealth information is available, 1,535 remain in the final first-generation sample. Most individuals, 86.6 percent, are dropped because they were not born between 1931 and 1945. Another 5.6 percent are dropped because the place of birth is missing. In the second-generation samples, sample selection criteria have similar effects, but relatively larger numbers of individuals are dropped due to a lack of data on parents' birthplaces.

when the GDR still existed. If children did not live in the GDR, we consider it to be unlikely that their parents lived for an extended period in the GDR.

3.3.2.4 Descriptive Characteristics of the Working Samples

Table 3.3 provides descriptive statistics for the *first*- and *second-generation samples*. Column 1 shows that the individuals in the first-generation sample are, on average, 67.12 years old, hold 193,323 euros of net wealth, and 64.56 percent are homeowners. They were born in cities that had, on average, 401,820 inhabitants in 1939 and that were 21.98 percent destroyed. Columns (3) and (5) show that individuals in the second-generation samples are, by construction, substantially younger than the first-generation sample with an average age of 39.3 years (father sample) and 42.6 years (mother sample). Consistent with the lifecycle hypothesis, their average net wealth is, at around 40,000 euros, lower than that of the first-generation sample. Parents of these second-generation individuals were born in cities that had, on average, about 300,000 inhabitants and that were 23.2 (fathers) and 22.0 (mothers) percent destroyed. Further, the table compares these samples to persons from the same birth cohort who (or whose parents) were also born in West Germany or Berlin, but who had to be excluded from analysis because their municipalities of birth are more than five kilometers away from the closest city in the GLR data. As could be expected, columns (2), (4), and (6) show that excluded persons—or their parents—come from more rural regions, as indicated by the lower population density indicators and lower regional per-capita tax returns. Further, the ratio of homeowners is higher among excluded persons in the case of the two second-generation samples.

Table 3.3: Average characteristics of first- and second-generation samples as well as excluded observations

	First generation		Second generation, fathers		Second generation, mothers	
	(1) In sample	(2) Excluded	(3) In sample	(4) Excluded	(5) In sample	(6) Excluded
SOEP data:						
Net wealth (euro)	193,322.80	143,532.26	91,750.67	113,456.35	101,384.76	123,089.28
House net value (euro)	99,872.92	86,117.95	38,887.28	53,816.73	47,209.78	58,982.10
Homeownership (%)	64.56	67.99	39.63	53.07	44.93	57.53
Age	67.12	67.82	39.27	40.72	42.61	42.85
Birth year	1,938.92	1,938.73	1,969.32	1,968.02	1,966.00	1,965.96
Survey year (minimum)	2002	2002	2002	2002	2002	2002
Survey year (maximum)	2017	2017	2017	2017	2017	2017
Historical data:						
Destruction (%)	21.98		23.17		21.98	
City population 1939 (1000 inh.)	401.82		320.55		309.78	
Population density 1939 (inh./km2)	1,430.81	319.35	1,328.15	134.58	1,322.53	167.03
Tax revenues 1938 (RM per capita):						
- Wealth	6.73	3.12	6.24	2.69	6.17	2.76
- Payroll	35.92	14.53	33.24	11.71	32.69	13.46
- Income	55.81	29.41	51.77	23.55	51.59	25.96
- Corporate	44.18	14.22	39.53	10.73	39.61	10.63
Observations	1535	353	1022	309	986	299

Note: Samples shown in columns (1), (3), and (5) are defined as described in Section 3.3.2.3. Excluded individuals in column (2) were born between 1931 and 1945 in West Germany or Berlin and did not live in the GDR, but were born more than five kilometers away from a municipality with destruction information or were born close to a municipality with missing destruction or population information. Excluded individuals in column (4)/(6) did not live in the GDR and had fathers/mothers who were born between 1931 and 1945 in West Germany or Berlin but more than five kilometers away from a municipality with destruction information or who were born close to a municipality with missing destruction or population information. RM are Reichsmark. Source: SOEP v35, Statistisches Reichsamt (1944), Gassdorf and Langhans-Ratzeburg (1950), Brockmann et al. (2022), own calculations.

3.4 Methods

3.4.1 Specification of Regressions

By exploiting the exogenous variation of destruction, we identify the causal effect of the shock exposure on wealth stocks today. The wealth stock is captured by three variables: net-of-debt wealth (in euros), net value of primary residence (in euros), and being the owner of primary residence (dummy variable). Wealth is always measured in the earliest available year between 2002 and 2017. For the first-generation sample, which was treated with different intensities of destruction at the age of 0 to 10 years, the basic OLS regression model takes the form,

$$y_{imrt} = \alpha + \beta_1 D_m + X_i'\beta_2 + V_r^{hist'}\beta_3 + \sum_t D_t\beta_t + \varepsilon_{imrt} \quad . \tag{3.1}$$

The left-hand variable, y_{imt}, measures the wealth stock of a respondent i in year t, born in municipality, m, in region r.

The first right-hand variable, D_m, is the percentage share of the destroyed housing stock in a person's birthplace, m. The proposed coefficient of interest is β_1. If higher destruction in the past implies lower wealth today, β_1 will be negative. X_i' is a set of individual-level control variables including age, age squared, and the federal state where the respondent was born. D_t is a dummy variable indicating the year $t \in (2002, 2007, 2012, 2017)$ when wealth was surveyed.

A potentially important confounding factor is the past economic development of a region that both made bombardment more likely and also affects wealth stocks today. Allied forces indeed targeted specific industries, important infrastructures, and larger cities in general, and it is possible that these factors correlate with post-war growth, affecting income and wealth levels up to the present. To mitigate such effects, we include $V_r^{hist'}$, a set of variables capturing the pre-war economic development of the birth region of i: the income, payroll, corporate, and wealth tax revenue per capita, the regional population density, as well as the number of inhabitants of a municipality.

Finally, ε_{imrt} is a random, idiosyncratic error term, clustered at the level of GLR municipalities to account for correlations in wealth between individuals born in the same region of destruction.

Additionally, our second strategy is to bypass confounding factors by means of IV estimations detailed below.

In the second-generation sample, fathers and mothers were aged between 0 and 10 years at the end of the war, and wealth of their children is measured 60 to 70 years later. To assess the effects of destruction of a parent's city of birth on individual i's wealth, we build on adapted version of model (3.1), taking the form,

$$y_{im_pr_pt} = \alpha^p + \beta_1^p D^p_{m_p} + X_i'\beta_2^p + V_{r_p}^{hist'}\beta_3 + \sum_t D_t\beta_t + \varepsilon_{im_pr_pt} \quad , \; p \in (\text{mother, father}). \tag{3.2}$$

The treatment now is $D^p_{m_p}$, the destruction of the father's or mother's city of birth. Hence, if higher destruction of the father's (mother's) birth place in the past imply lower wealth today, β_1^f (β_1^m) will be negative. $V_{r_p}^{hist'}$ captures the pre-war economic development (as defined above) of the birth region of parent p of i.

Because the wealth data is multiply imputed, in all estimations, we use Rubin's rule (Rubin, 1987).

3.4.2 Instrumental Variables

Following Vonyó (2012) and Akbulut-Yuksel (2014), we use the distance between a municipality and London as an instrumental variable.[30] Figure 3.4 shows that there is a strong negative correlation between the distance to London and the degree of destruction. That the instrument is relevant is indicated by relatively high F statistics: Depending on the sample of interest, these range between 10.1 and 24.5. [31] F-statistics are not higher because we take a conservative approach by clustering at the level of GLR municipalities, and there is, by construction, no variation in the instrument and the instrumented destruction variable within clusters.

There are several possible reasons why more distant municipalities were bombed less. First, the range of aircraft types was limited, particularly in the early years of the war.[32] Second, as argued by Hampe (1963b), most regions in Germany offered valuable targets, and from a simple cost-benefit perspective, it was more convenient to attack nearby regions. Third, the course of the war led to invasion of British and

[30] Miguel and Roland (2011) use distance between Vietnamese regions and the 17th parallel north in their study on bombing during the Vietnam War.

[31] These are Kleibergen-Paap rk Wald F-statistics for a test of weak instruments for the two-stage least squares estimator as reported by Stata's ivreg2 command (Baum et al., 2002). Based on critical values from Stock and Yogo (2005), F-statistics larger than 16.38 imply that the null hypothesis of weak instruments can be rejected at a significance level of 5%. The null hypothesis states specifically that a Wald test on the estimated coefficient of interest (β_1) has a size larger than 10%, meaning that statistically, inference on β_1 is prone to type-I errors under the null hypothesis. A second, looser null hypothesis is that the Wald test size is more than 15%, which corresponds to a critical F-statistic of 8.96. All of our IV regressions surpass this second critical value.

[32] Although bombers had sufficient range to penetrate deep into German territory, the limited range of accompanying fighter aircraft posed a major problem for Allied forces. An important technical innovation was the introduction of the P-47D Thunderbolt and P-51 Mustang long-range fighters in 1943, which made it possible to escort bombers deeper into the German territory (p.6 The United States Strategic Bombing Survey, 1945; Hampe, 1963a, p.125)

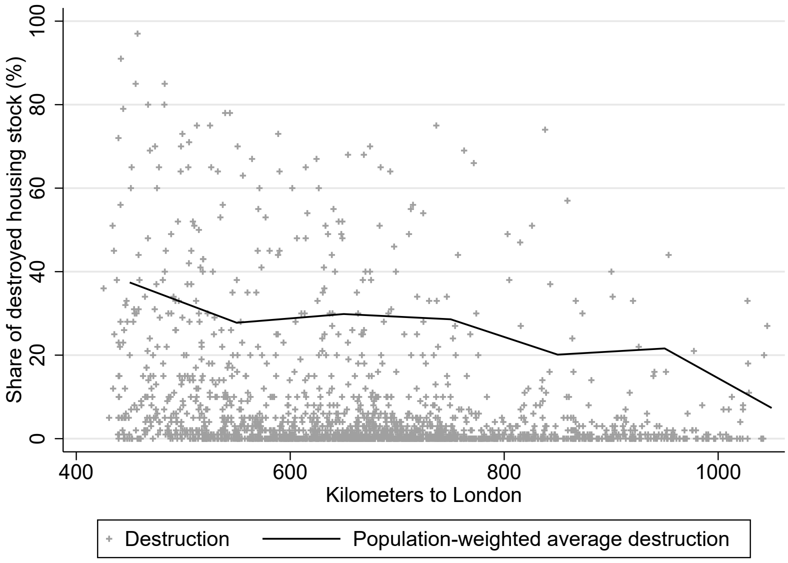


Figure 3.4: Destruction and distance from London of German municipalities.
Note: The figure shows all 1739 municipalities with valid destruction information. Population-weighted average destruction is calculated for bins of 100 kilometers using a municipality's number of inhabitants in 1939 as weight. Source: Gassdorf and Langhans-Ratzeburg (1950); own calculations.

Americans troops from the West, escorted by heavy bombardments that destroyed several municipalities near the German western border.

As regards exogeneity of the instrument, our main concern is that the distance variable picks up peculiarities of the German economic geography. For example, the federal state of Bavaria is in one of the richest areas of Germany and is also far from London. To ensure that our estimates are robust to such unintended links, we repeat all estimations in the robustness section excluding specific states.

3.5 The Long-Run Effect of Bombings on Wealth Holdings

3.5.1 Regression Results

Table 3.4 reports the estimate of the destruction parameter, β_1, from OLS and IV regressions for the first-generation sample for each of our three dependent variables: net wealth, net value of primary residence, and being a homeowner. For both models, OLS and IV, we estimate two specifications, differing in the set of control variables: while in specification (1), X' only contains controls for age, specification (2) also includes the state in which the respondent was born to control for regional effects resulting, e.g., from different developments in real-estate markets.

All OLS estimates of the destruction parameter are significant and negative, suggesting that the experience of bombing during childhood has a permanent negative effect on wealth holdings in later life, including the probability of being a homeowner. The effect is economically relevant. As an example, a one percentage point increase in the proportion of destroyed residential buildings in the municipality of birth reduces net wealth later in life by about −1,015 to −1,131 euros. The effect size equals approximately 0.5 percent of the average net wealth in the sample. A main question is whether the detrimental effect on net wealth is due to lower real estate possessions. This is the case: the higher the level of destruction, the lower the value of real estate and the probability of possessing real estate. With an effect size of about −718 to −836 euros, losses in real estate explain more than two thirds of the losses in net wealth. Moreover, small standard errors indicate that there is a strong link between destruction and real estate holdings.

The IV estimations confirm the negative effect of having experienced bombing in WWII on wealth holdings today. Quantitatively, the effects are stronger and estimated with less precision compared to OLS. In sum, the estimates for the first-generation sample support the idea that WWII destruction has a long-lasting effect on wealth holdings of German households. The regression results for the two second-generation samples support the above assessments. Table 3.5 provides the effects for the treatment of fathers and Table 3.6 of mothers. Quantitatively and qualitatively, the OLS estimates for the second-generation sample are very similar to the first-generation sample. As regards the treatment of fathers, OLS estimates show that a one percentage point increase in destruction experienced by parents reduces the later-life net wealth of their children by −964 to −1,055 euros. For the treatment of mothers, the effects are somewhat smaller (−753 to −974 euros). Again, the detrimental effect of destruction operates strongly through real estate holdings: According to the OLS estimates, a marginal increase in destruction in the father's region reduces the net value of the children's primary residence by −488 to −507 euros and between −385 and −485 euros for mother's region. Again, destruction

Table 3.4: Effect of destruction, first generation, 1931-1945 cohort.

	OLS		IV	
Outcome	(1)	(2)	(3)	(4)
Net wealth	-1130.8***	-1014.6**	-4113.1**	-5725.0*
	(426.6)	(467.5)	(1953.9)	(3037.4)
Net value primary resid.	-835.6***	-718.1***	-3578.6***	-2386.5**
	(211.7)	(174.0)	(1004.7)	(1074.0)
Homeownership	-0.175*	-0.194**	-0.594**	-0.235
	(0.092)	(0.093)	(0.297)	(0.392)

Note: Specifications (2) and (4) include federal state dummy variables. Further controls in all regressions: age and age squared, population in 1939, population in 1939 squared, density in 1939, per-capita wealth, payroll, income, and corporate tax in 1938. Instrument: distance to London. The number of observations is 1,535 in all regressions. Standard errors in parentheses, clustered at the level of GLR municipalities, * significant at 10%, ** significant at 5%, *** significant at 1%. Source: SOEP v35; own calculations.

Table 3.5: Effect of destruction, second generation, 1931-1945 father cohort.

	OLS		IV	
Outcome	(1)	(2)	(3)	(4)
Net wealth	-1055.0***	-963.8***	-3775.7**	-1330.3
	(395.0)	(333.3)	(1515.0)	(1963.7)
Net value primary resid.	-487.7***	-506.8***	-1889.5***	-1557.6**
	(133.5)	(121.1)	(688.1)	(688.6)
Homeownership	-0.293***	-0.358***	-0.563*	-0.651
	(0.098)	(0.099)	(0.316)	(0.436)

Note: Specifications (2) and (4) include federal state dummy variables. Further controls in all regressions: age and age squared, population in 1939, population in 1939 squared, density in 1939, per-capita wealth, payroll, income, and corporate tax in 1938. Instrument: distance to London. The number of observations is 1,022 in all regressions. Standard errors in parentheses, clustered at the level of GLR municipalities, * significant at 10%, ** significant at 5%, *** significant at 1%. Source: SOEP v35; own calculations.

is echoed again in the probability of being a homeowner. Compared to the effects estimated in the first generation, we find a larger effect of the destruction level of fathers' regions on the likelihood of being a homeowner, -0.293 to -0.358 percentage points in case of OLS, but no effect of the destruction level of mothers' regions. One possible explanation is the channels through which assets are transferred between generations. The issue of inheritance is further explored in the following mediation analysis. Table 3.7 shows that bombardment has a substantial detrimental economic impact. According to the point estimates, average net wealth was 16,553 to 22,330

Table 3.6: Effect of destruction, second generation, 1931-1945 mother cohort.

	OLS		IV	
Outcome	(1)	(2)	(3)	(4)
Net wealth	-973.8**	-753.2**	-4564.2**	-34.8
	(409.8)	(366.6)	(1876.0)	(2419.0)
Net value primary resid.	-485.3***	-384.6**	-2643.0***	-1582.3
	(148.0)	(148.7)	(829.5)	(1002.0)
Homeownership	-0.136	-0.131	-0.109	0.083
	(0.090)	(0.099)	(0.327)	(0.553)

Note: Specifications (2) and (4) include federal state dummy variables. Further controls in all regressions: age and age squared, population in 1939, population in 1939 squared, density in 1939, per-capita wealth, payroll, income, and corporate tax 1938. Instrument: distance to London. The number of observations is 986 in all regressions. Standard errors in parentheses, clustered at the level of GLR municipalities, * significant at 10%, ** significant at 5%, *** significant at 1%. Source: SOEP v35.

euros higher in the absence of bombardment, depending on the sample. These absolute differences correspond to 11.5 percent of average net wealth in the first-generation sample, 24.3 percent in the second-generation father sample, and 16.3 percent in the mother sample. For net value of the primary residence, the losses amount to 8,452 to 15,781 euros.[33]

Irrespective of the outcome, effects in the IV regressions are quantitatively larger than in OLS but estimated with lower precision. This result coincides with the results of the study by Akbulut-Yuksel (2014), who use the same identification strategy as ours. A likely explanation is that OLS estimates are somewhat biased towards zero due to omitted variables. Although we include pre-war economic control variables on a small regional level, we do not control for any factors on the municipality level other than the number of inhabitants. One might, for example, think that omitting the municipality-level population density biases the OLS results if wealthier people lived denser municipalities and these municipalities were attacked more heavily. Both assumptions may be true given that population density should correlate positively with property prices and economic development. Moreover, Allied forces might have attacked denser cities more heavily due to their strategy of bombing to break the morale of the German population or other strategic considerations. Overall, OLS estimates may be interpreted as a conservative lower bound of the true effect, which is negative regardless of the estimation method.

[33]To calculate hypothetical distributions of net wealth, we add to each person's net wealth an amount corresponding to the level of destruction that the person experienced times the estimated effect of destruction based on specification (2).

Table 3.7: Hypothetical distribution of average wealth stock assuming no destruction.

		OLS (2)		IV (2)	
	Observed	Hypothetical	Difference	Hypothetical	Difference
Net wealth:					
First generation	193,323	215,621	22,298	319,146	125,823
Second generation, fathers	91,751	114,080	22,330	122,572	30,821
Second generation, mothers	101,385	117,938	16,553	102,149	764
Net value of primary residence:					
First generation	99,873	115,654	15,781	152,322	52,449
Second generation, fathers	38,887	50,629	11,741	74,975	36,087
Second generation, mothers	47,210	55,661	8,452	81,984	34,775

Source: SOEP v35; own calculations.

3.5.2 Mediation Analysis

The mediation analysis seeks to identify and explain the mechanisms underlying the observed relationship between wartime destruction and wealth holdings today through the inclusion of third variables, the so-called mediators. Rather than a direct causal relationship between wartime destruction and wealth, the mediation analysis proposes that wartime destruction influences the mediator variables, which in turn influence wealth holdings.

Against the background of the existing literature on the consequences of wars, we study the role of the following potential mediators:

- *Education.* Several studies show (Table 3.1) the detrimental effect of war and wartime destruction on educational outcomes. According to Akbulut-Yuksel (2014), World War II destruction in Germany reduced the years of school attendance at that time. As higher education, an important component of human capital, implies higher lifetime income and thus a higher propensity to save and accumulate wealth (Card, 1999), we expect education to be an important mediator that explains part of the total effect of destruction on wealth. As a measure for education, we use a person's highest educational degree classified according to the International Standard Classification of Education (ISCED) (UNESCO (2006)).

- *Health.* Health is another important part of human capital and presumably operates in a similar way to education. Several studies find that war-related treatments have long-lasting detrimental effects on health outcomes (see Table

3.1). Hence, we expect that health, like education, explains part of the total effect of destruction on wealth. As a measure of health, we use a person's current satisfaction with her health, self-rated on a 0-to-10 scale.[34]

- *Lifetime labor market outcomes.* For the vast majority of people, work is a central determinant of material well-being. Labor market outcomes may pick up potential effects of destruction on regional economic development, both in regions where individuals were born as well as in regions to which they moved later in life. For example, the results of Brakman et al. (2004) and Bosker et al. (2008) indicate that the WWII bombings in Germany reduced city-level population growth up to 50 years after the war, which might have affected regional economic development in various ways. We use three indicators of labor market success: 1) The age at which a person had her first job, to test whether labor market entrance decisions were affected by destruction. 2) The years of total labor market experience to measure lifetime labor supply. 3) An indicator of lifetime income.

- *Inheritances.* The channel of missed inheritances due to wartime destruction might be the most obvious channel negatively affecting the wealth position today—especially for the second-generation sample. To assess this channel, we construct a dummy variable that indicates whether a person has received an inheritance or inter vivos gift. Depending on the availability of data, the dummy is defined by one of three variables: individual inheritances over the lifetime up to the year 2001; individual inheritances during the period 2002 to 2017; household-level inheritances during the years a person participated in the SOEP. Due to the differences in the measurement, we expect significant measurement error in the variable.

To study the role of the potential mechanisms, we proceed in three steps. First, we re-estimate equations (3.1) and (3.2), but use the potential mechanism as the dependent variable in lieu of wealth (row a. in Table 3.8). That is, we test whether differences in the local level of wartime destruction affect the respective mediator. Second, we test whether the mediator is correlated with wealth (row b.). For a causal pathway to exist, we would expect that wartime destruction directly impacts the mediator, which in turn is correlated with wealth. Third, we estimate the magnitude of the mediated effect using the method of Acharya et al. (2016), which relies on the Average Controlled Direct Effect (ACDE). The ACDE is defined as the direct causal effect of destruction on wealth if there were no mediated effects.[35] Row d. reports

[34] Unfortunately, the data do not contain information on health during childhood or a person's medical history.

[35] More precisely, "[t]he CDE represents the causal effect of a treatment when the mediator is fixed at a particular level." (Acharya et al., 2016). In the analysis, all mediators are fixed at 0 or at the lowest category (education and age (minimum legal working age (13 years)).

the difference between the ACDE and the total effect of destruction as an estimate for the magnitude of the mediated effect, whereas row c. reports the total effect of destruction as a reference.[36]

Table 3.8 summarizes the results for all three samples. Education and destruction are always negatively related, while education correlates positively with net wealth. Depending on the sample, education explains about 25 to 256 euros of the total effect of destruction on wealth. The contribution of education is smaller in the second-generation samples, possibly because the effect of education on net wealth (row b.) is smaller than in the first-generation sample. This is reasonable considering that the second generation is younger than the first generation and education still has not yielded the full lifetime returns. Satisfaction with health correlates positively with net wealth, but the correlation with destruction is rather weak. In sum, we find a small but insignificant mediating effect in the first-generation sample of about 96 euros, and null effects for the second-generation samples. Regarding the labor market outcomes positively correlate with net wealth have a weak negative correlation with destruction, indicating that individuals from more severely destroyed municipalities started to work at a younger age, accumulated less labor market experience, and earned less lifetime income. Yet the mediating effect of the labor market outcomes is negligible. The same holds for inheritances. This runs counter to our expectations because inheritances are an essential channel through which families pass on wealth to their children. We suspect, however, that measurement error in the inheritance indicator introduces significant bias into the estimates.

Overall, the mediation analysis points to education as an important mediator that, in the first-generation sample, contributes about one quarter to the total effect of destruction on net wealth. The mediation analysis also shows that a large share of the total effect is not explained by the mediators we considered, pointing to the possibility that other mediators exist that we could not consider due to data constraints. For example, Second World War destruction triggered large-scale rebuilding programs and important government interventions into the real estate market that may have had long-lasting effects on individual investment behavior and household portfolios. Further, while we considered lifetime labor market outcomes, it is also possible that effects of destruction were mediated through the capital market. Affected individuals potentially had less assets for investment or as collateral for mortgages, such that post-war wealth differences were perpetuated over decades.

[36]The total effects reported in Table 3.8 differ slightly compared to the main results reported in Tables 3.4 to 3.6 because some observations had to be dropped from the sample due to missing values in the mediators.

Table 3.8: Mediation analysis.

	(1) Education	(2) Satisfaction with health	(3) Age at first job	(4) Labor market experience	(5) Lifetime income rank	(6) Inheritances
			First generation:			
a. Effect of destruction on mediator	-0.005**	-0.008*	-0.005	-0.047*	-0.000	-0.011
b. Effect of mediator on net wealth	61499.852***	18628.205***	14092.898***	327.278	73556.078**	642.374**
c. Total effect β_1 (euro)	-994.301**	-994.301**	-994.301**	-994.301**	-994.301**	-994.301**
d. Contribution of mediated effect (euro)	-255.965*	-96.310	-29.248	-41.844	5.498	-5.488
Observations	1497	1497	1497	1497	1497	1497
			Second generation, fathers:			
a. Effect of destruction on mediator	-0.007**	-0.002	0.000	0.006	-0.001**	0.005
b. Effect of mediator on net wealth	22700.676***	9428.377**	8051.045***	-3078.960**	77692.007***	991.517***
c. Total effect β_1^f	-1000.323***	-1000.323***	-1000.323***	-1000.323***	-1000.323***	-1000.323***
d. Contribution of mediated effect (euro)	-25.038	-14.003	-0.844	-11.654	-88.426	4.445
Observations	977	977	977	977	977	977
			Second generation, mothers:			
a. Effect of destruction on mediator	-0.004*	-0.004	-0.009	-0.008	-0.001	0.051
b. Effect of mediator on net wealth	24156.969***	10227.937***	6511.469***	-1840.738	102782.142***	875.357***
c. Total effect β_1^m	-746.427**	-746.427**	-746.427**	-746.427**	-746.427**	-746.427**
d. Contribution of mediated effect (euro)	-28.254	-30.566	-18.517	12.665	-47.766	40.655
Observations	969	969	969	969	969	969

Note: Control variables in all regressions: age and age squared, population in 1939, population in 1939 squared, density in 1939, per-capita wealth, payroll, income, and corporate tax in 1938, survey year and federal state dummy variables. Analytical standard errors clustered at the level of GLR municipalities in rows a. to c.; bootstrap standard errors based on 1,500 bootstrap replications in row d., * significant at 10%, ** significant at 5%, *** significant at 1%. Source: SOEP v35; own calculations.

Table 3.9: Dealing with skewness, effect of destruction, first generation, 1931-1945 cohort.

	OLS		IV	
Outcome	(1)	(2)	(3)	(4)
Net wealth (censored)	-782.822**	-711.803*	-4143.336***	-2924.464
	(376.285)	(387.840)	(1492.887)	(1786.055)
Net value primary resid. (censored)	-681.569***	-618.716***	-2986.519***	-1905.767**
	(185.986)	(159.146)	(850.186)	(889.477)
Net wealth (rank 0-1)	-0.001**	-0.001**	-0.007***	-0.005**
	(0.001)	(0.001)	(0.002)	(0.003)
Net value primary resid. (rank 0-1)	-0.002***	-0.002***	-0.007***	-0.004*
	(0.001)	(0.000)	(0.002)	(0.002)

Note: Specifications (2) and (4) include federal state dummy variables. Further controls on all regressions: age and age squared, population in 1939, population in 1939 squared, density in 1939, per-capita wealth, payroll, income, and corporate tax in 1938. Instrument: distance to London. The number of observations is 1,536 in all regressions. Standard errors in parentheses, clustered at the level of GLR municipalities, * significant at 10%, ** significant at 5%, *** significant at 1%. Source: SOEP v35; own calculations.

3.6 Robustness

3.6.1 Dealing with Skewness

Wealth distributions are known to be skewed, with the effect that a few observations with high assets may drive the results. To check whether this is the case, we repeat the estimation using two transformations of the dependent variable that are robust to outliers. First, we censor the dependent variables at the 1st and 99th percentile. Second, instead of wealth, we use each observation's standardized rank in the wealth distribution (position on the cumulative density).

Estimations results are summarized in Tables 3.9 to 3.11 for the first- and second-generation samples. The tables confirm the results from the main analysis for the transformed two variables: Destruction has a significant negative effect on net wealth and the net value of the primary residence. The robustness checks also show that, in case of net wealth, the effect size is partly driven by observations with high net wealth. Effect sizes for censored net wealth are about one third smaller than in the main analysis. In the case of the net value of the primary residence, the robustness checks show that effect sizes are not driven by observations at the top or bottom of the distribution.

Table 3.10: Dealing with skewness, effect of destruction, second generation, 1931-1945 father cohort.

	OLS		IV	
Outcome	(1)	(2)	(3)	(4)
Net wealth (censored)	-730.426***	-697.513***	-3279.059***	-1294.463
	(252.188)	(267.338)	(1209.172)	(1618.460)
Net value primary resid. (censored)	-412.160***	-454.336***	-1672.774***	-1477.184**
	(118.212)	(114.436)	(588.820)	(644.627)
Net wealth (rank 0-1)	-0.002***	-0.002***	-0.008***	-0.006**
	(0.001)	(0.001)	(0.003)	(0.003)
Net value primary resid. (rank 0-1)	-0.002***	-0.002***	-0.005***	-0.006**
	(0.000)	(0.000)	(0.002)	(0.002)

Note: Specifications (2) and (4) include federal state dummy variables. Further controls on all regressions: age and age squared, population in 1939, population in 1939 squared, density in 1939, per-capita wealth, payroll, income, and corporate tax 1938. Instrument: distance to London. The number of observations is 1,021 in all regressions. Standard errors in parentheses, clustered at the level of GLR municipalities, * significant at 10%, ** significant at 5%, *** significant at 1%. Source: SOEP v35; own calculations.

Table 3.11: Dealing with skewness, effect of destruction, second generation, 1931-1945 mother cohort.

	OLS		IV	
Outcome	(1)	(2)	(3)	(4)
Net wealth (censored)	-661.728**	-491.002*	-3972.678***	-1016.406
	(256.413)	(256.847)	(1525.028)	(1874.654)
Net value primary resid. (censored)	-413.172***	-327.764**	-2213.245***	-1508.658
	(128.145)	(133.907)	(678.882)	(948.701)
Net wealth (rank 0-1)	-0.002***	-0.002***	-0.005**	-0.002
	(0.001)	(0.001)	(0.002)	(0.004)
Net value primary resid. (rank 0-1)	-0.001**	-0.001**	-0.003*	-0.002
	(0.000)	(0.000)	(0.002)	(0.003)

Note: Specifications (2) and (4) include federal state dummy variables. Further controls on all regressions: age and age squared, population in 1939, population in 1939 squared, density in 1939, per-capita wealth, payroll, income, and corporate tax 1938. Instrument: distance to London. The number of observations is 987 in all regressions. Standard errors in parentheses, clustered at the level of GLR municipalities, * significant at 10%, ** significant at 5%, *** significant at 1%. Source: SOEP v35; own calculations.

3.6.2 Dealing with Spurious Correlation

It is not excluded that spurious correlation between destruction and the post-war economic development of specific federal states drive our results. Regions that were formerly relatively weak economically, such as Bavaria and Baden-Württemberg, are now among the strongest and were also less affected by WWII bombings than other states. The opposite is true, for example, for North Rhine-Westphalia, which was heavily destroyed during WWII and saw its mining industry decline in the post-war decades, leading to economic downturn.

To study the influence of individual states on the results, we implement a regional jackknifing procedure. That is, we re-run the basic regressions from our main analysis, always leaving out one region. The jackknife method performs $r = 1, \ldots, 16$ regressions. In each run, all observations living in a state r are left out.

Figures 3.5 to 3.13 in the Appendix show the distribution of the r jackknife coefficients for each dependent variable and sample. The jackknife procedure reconfirms our main analysis. Overall, omitting a federal state changes the results very little. Nonetheless, some tendencies are observable. First, leaving out a state reduces sample size and this tends to widen confidence bands. Second, leaving out North Rhine-Westphalia strengthens the negative effect of wartime destruction, suggesting that the relation between destruction and wealth is stronger in the other states. Leaving out Bavaria or Baden-Württemberg has the opposite effect.

3.7 Qualifications and Extensions

The present study analyzes the long-term effects of WWII destruction on private wealth in Germany, filling a gap in the growing literature on the causal consequences of war-related treatments (Section 3.2.1). The identification of effects relies on unique municipality-level data on housing destruction that is linked in an innovative way to recent survey information using birth place information of two generations. By exploiting regional variations in destruction and using OLS and IV estimation, the study finds a significant detrimental long-term effect of destruction on private wealth.

Despite the several strengths of the study, the empirical results could be strengthened by further improvements. First, the study may benefit from a description of the relationship between destruction and pre-war wealth at the regional level to highlight the extent to which bombing and destruction were selective in terms of wealth. At present, the estimations control for pre-war wealth using tax revenue data, but more detail in this regard could strengthen the interpretation of the OLS estimates. Second, an additional instrumental variable could improve the identification of a casual effect in the IV estimations. This would also allow for testing of overidentification (Sargan, 1958; Basmann, 1960) and exogeneity (Davidson and

MacKinnon, 1993), and would help to assess, whether local average treatment effects differ by instrument (Angrist et al., 1996). Lastly, the study provides relative weak empirical evidence for possible mechanisms that explain why destruction has a long-term effects, especially in the second-generation samples. The analysis could be improved by examining whether destruction had regional-level effects on, for example, growth or real estate markets, and whether these may be possible channels through which destruction operates.

Nevertheless, the study paves way for several relevant future research directions. One possible extension is to examine the large-scale re-construction of the housing stock that followed wartime destruction. An analysis of post-war policy responses and housing market interventions may be particularly relevant for the design of future policies that have to deal with the re-construction of destroyed cities and regions. Next, future research may use the rich destruction data of this study to further analyze and test the robustness of detrimental long-term effects of destruction found in previous studies that relied on substantially less granular data (Section 3.2.1). Lastly, future studies may exploit birthplace information to match recent survey data with historical (destruction) data similar to this study. This could benefit those analyses where post-war internal migration is relevant for the identification of effects and where the place of residence at the time of the survey is not appropriate for matching.

3.8 Conclusion

The Second World War has often been called a “great leveler” (Scheidel, 2017) that markedly reduced persistently high income and wealth concentrations (Roine and Waldenström, 2015; World Inequality Database, 2017). The present work shows that the wartime destruction left its mark on the level of private wealth today: People who were exposed to particularly heavy bombing during the war have fewer assets today. The results indicate that today’s net wealth was lowered by about 12 percent by the bombings. This also carries over to the next generation, where the present work finds reductions of net wealth due to wartime destruction in the range of 16 to 24 percent.

The evidence from the present and previous studies suggests that exposure to war in early life has long-run effects on well-being later in life. This holds for many dimensions of well-being, including wealth, income, health, and education. These long-term welfare costs of ongoing armed conflicts highlight the importance of peaceful resolutions of conflicts.

From a data infrastructure perspective, the paper highlights the advantages of using biographical information from prospective panel studies together with regional indicators—for the currently surveyed sample as well as for surveys of the parent generation. Complemented with digitalized (historical) data from archives, such approaches open up a wide range of research opportunities (see Kesternich et al. (2014) and Schröder et al. (2020)).

3.9 Appendix

3.9.1 IV First-Stage Results

Table 3.12: IV first-stage results.

	IV (1)	IV (2)
First generation:		
Distance to London	-0.034***	-0.057***
	(0.009)	(0.015)
rk Wald F-statistic	13.83	14.55
Observations	1535	1535
Second generation, fathers:		
Distance to London	-0.038***	-0.064***
	(0.008)	(0.017)
rk Wald F-statistic	24.53	14.89
Observations	1022	1022
Second generation, mothers:		
Distance to London	-0.035***	-0.054***
	(0.008)	(0.017)
rk Wald F-statistic	20.96	10.13
Observations	986	986

Note: The table shows IV first-stage results for the three samples of the main analysis, that is, the effect of the distance between London and the municipality of birth in kilometers (instrument) on the share of destroyed housing stock (endogenous variable), controlling for age and age squared, population in 1939, population in 1939 squared, density in 1939, per-capita wealth, payroll, income, and corporate tax in 1938. Specification IV (2) also includes federal state dummy variables. The critical values of the Kleibergen-Paap rk Wald F-statistic of a test of weak instruments are 16.38 (10% maximum relative bias of IV estimates) and 8.96 (15% maximum relative bias of IV estimates). F-statistics larger than the critical value indicate that the null hypothesis of weak instruments given the assumed maximum relative bias can be rejected. Robust standard errors in parentheses, clustered at the level of GLR municipalities, * significant at 10%, ** significant at 5%, *** significant at 1%. Source: SOEP v35; own calculations.

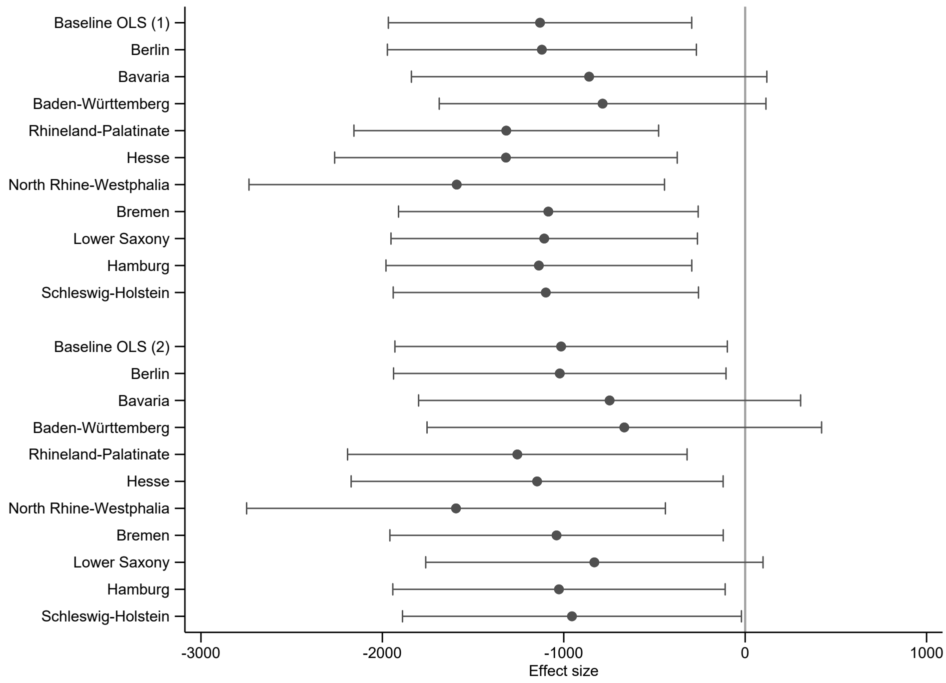
3.9.2 Mediation Analysis—Construction of Lifetime Income Indicator

In the mediation analysis, we use different measures of lifetime income depending on the sample. For the first-generation sample, we base the measure on a person's pension—or pension entitlement in the rare cases where somebody is not yet retired. We rank all individuals according to the pension or pension entitlement and use the rank, standardized to the sample size such that the variable ranges from zero to one, as the mediator. We apply this rank transformation to make the mediator comparable to that of the second-generation sample.

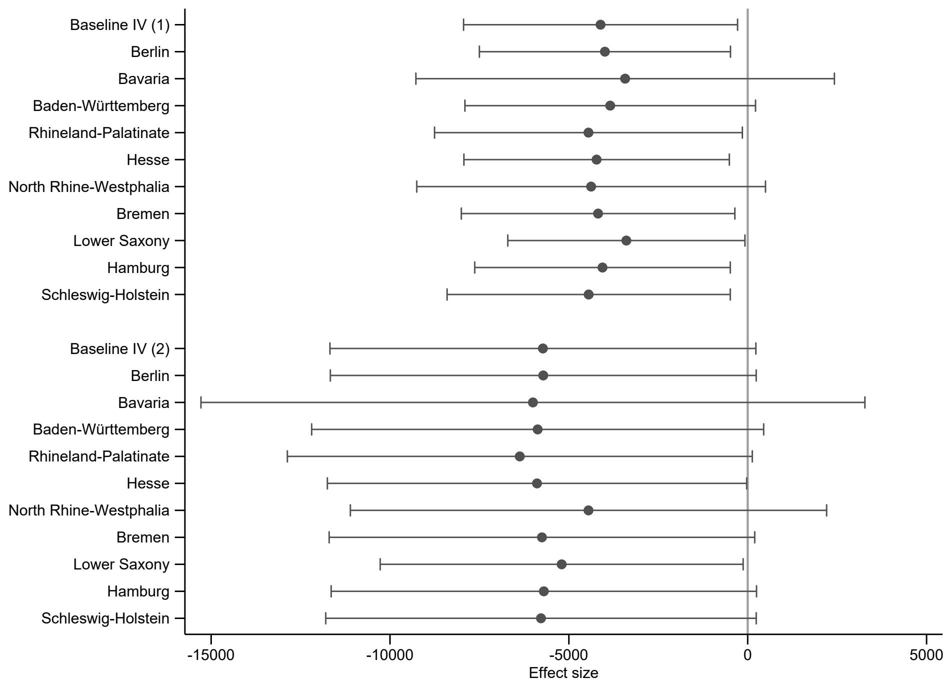
In case of the second-generation sample, pension entitlements are missing for a relatively large part of the sample. Further, the sample is younger than the first-generation sample and there is a larger variation in age within the sample, such that we chose a different measure. We use the rank of a person according to her current labor market earnings, relative to persons of the same age. To calculate the rank of labor market earnings, we group all individuals into five-year age groups. For each age group, we run a Mincerian regression of current gross labor market earnings regressed on age, education, and labor market experience. We predict the labor market earnings a person had if she had the maximum age within that age group to account for the within-age differences in that group. We rank each person according to the predicted income within that age group and standardize the rank with the group-specific number of observations.

3.9.3 Robustness: Estimation Without Specific Federal States

The estimation results are presented on the following pages.



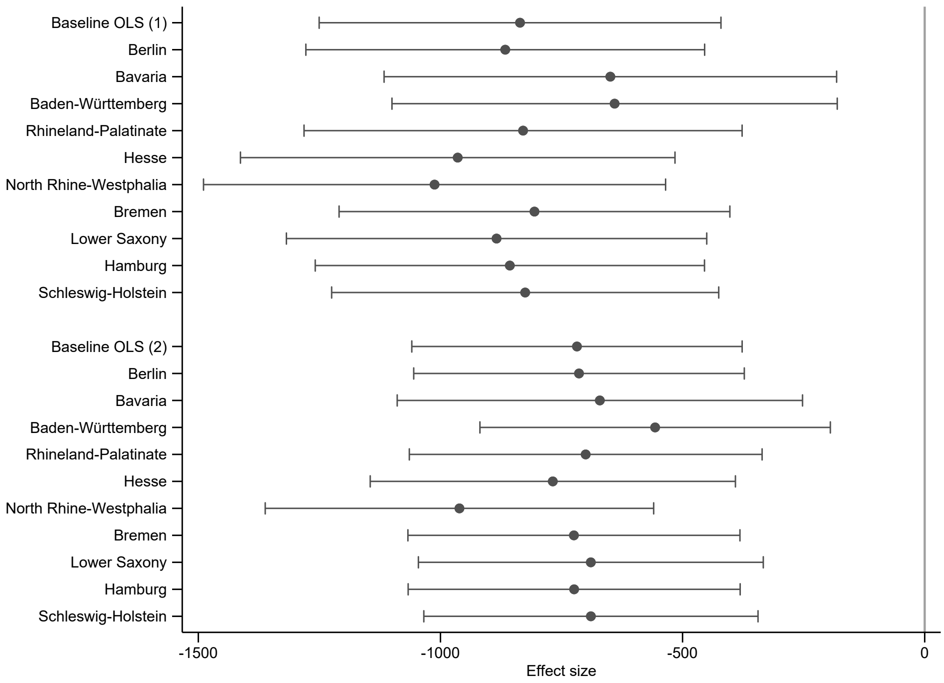
(a) Net wealth, OLS, first generation



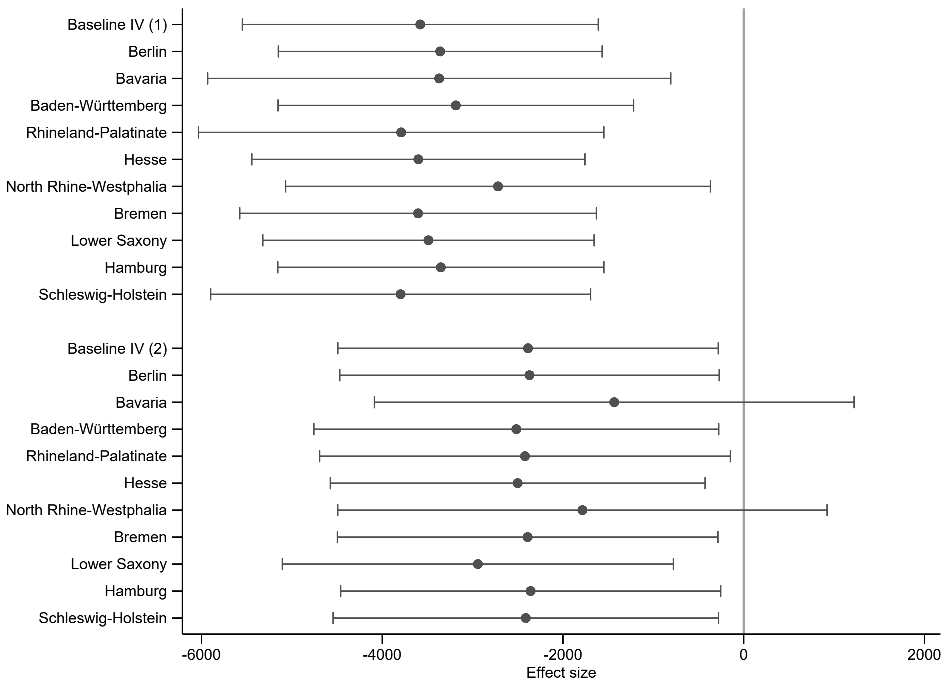
(b) Net wealth, IV, first generation

Figure 3.5: Regional jackknifing, effect of destruction, first generation, 1931-1945 cohort.

Note: The graph shows the point estimates of the effect of destruction and the 95% confidence interval for regressions, in which observations born in a specific federal state are dropped. The federal state is shown on the ordinate axis. Source: SOEP v35; own calculations.



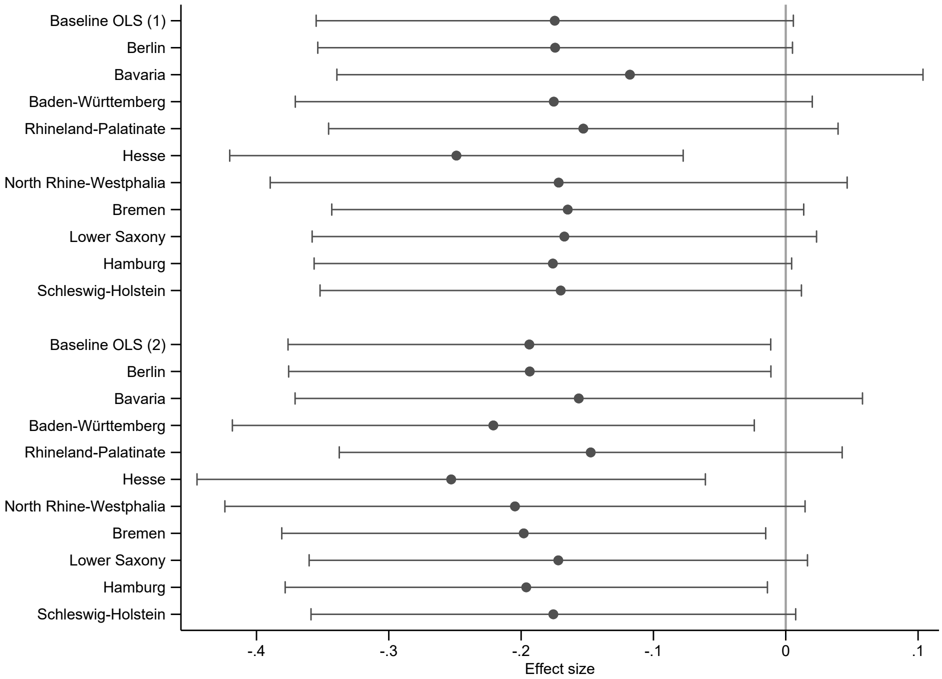
(a) Net value primary residence, OLS, first generation



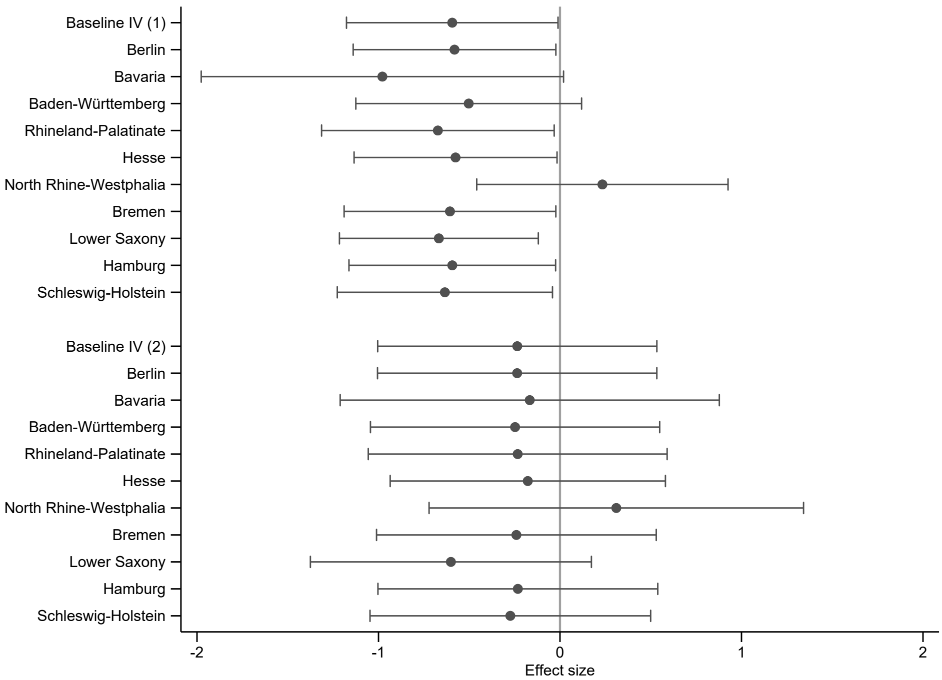
(b) Net value primary residence, IV, first generation

Figure 3.6: Regional jackknifing, effect of destruction, first generation, 1931-1945 cohort.

Note: The graph shows the point estimates of the effect of destruction and the 95% confidence interval for regressions in which observations born in a specific federal state are dropped. The federal state is shown on the ordinate axis. Source: SOEP v35; own calculations.



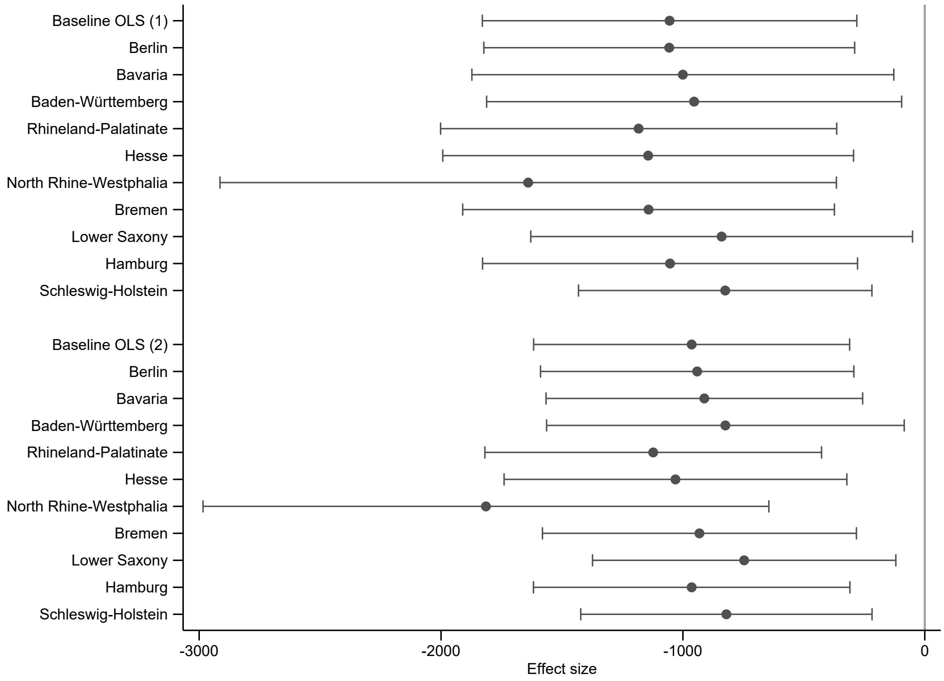
(**a**) Homeownership, OLS, first generation



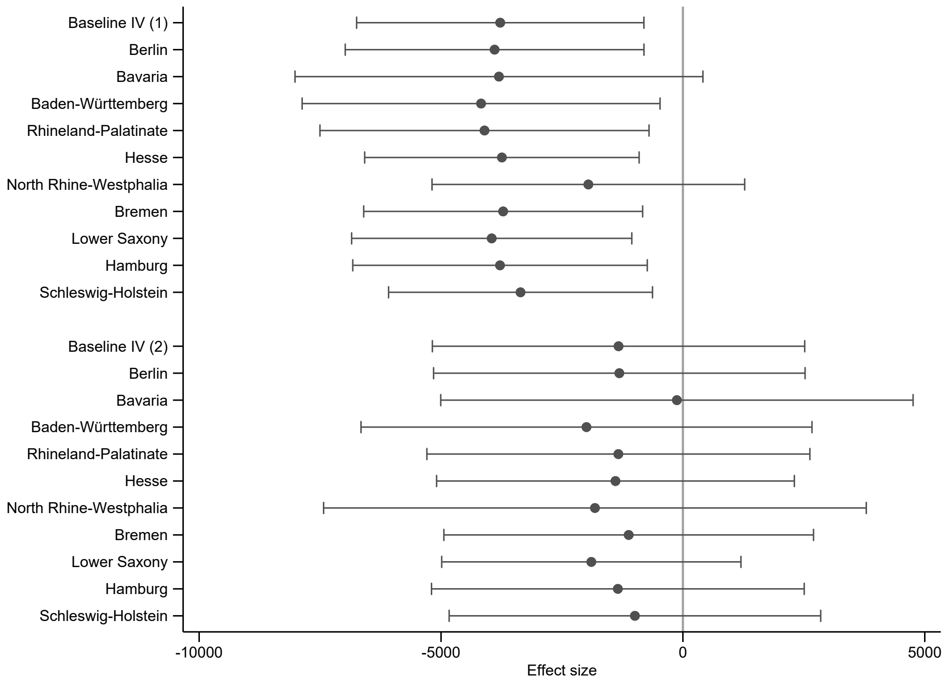
(**b**) Homeownership, IV, first generation

Figure 3.7: Regional jackknifing, effect of destruction, first generation, 1931-1945 cohort.

Note: The graph shows the point estimates of the effect of destruction and the 95% confidence interval for regressions, in which observations born in a specific federal state are dropped. The federal state is shown on the ordinate axis. Data: SOEP v35; own calculations.



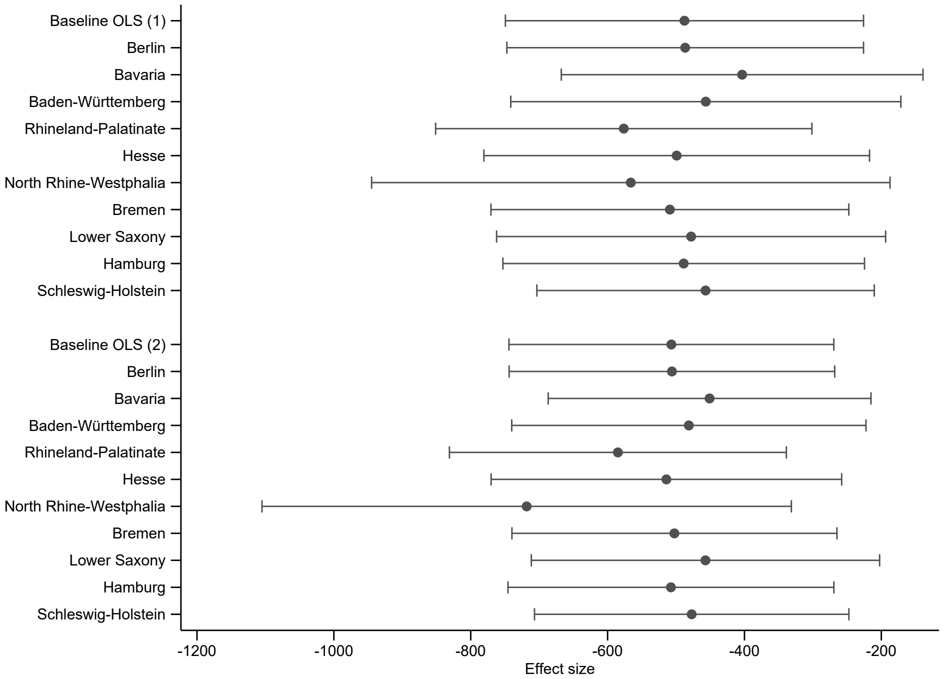
(a) Net wealth, OLS, second generation, fathers



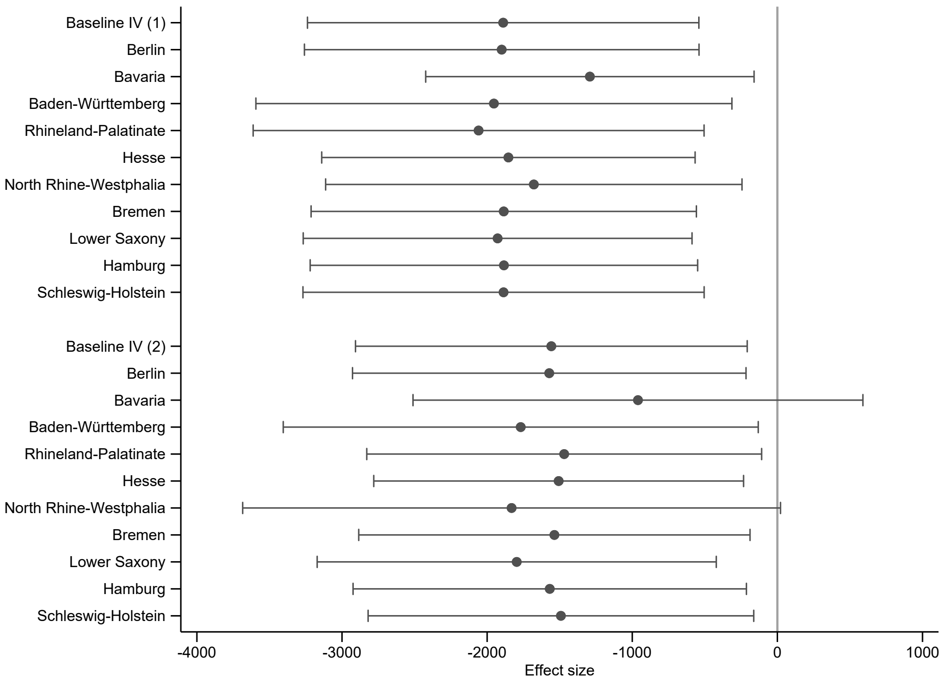
(b) Net wealth, IV, second generation, fathers

Figure 3.8: Regional jackknifing, effect of destruction, second generation, 1931-1945 father cohort.

Note: The graph shows the point estimates of the effect of destruction and the 95% confidence interval for regressions in which observations whose father was born in a specific federal state are dropped. The federal state is shown on the ordinate axis. Data: SOEP v35; own calculations.



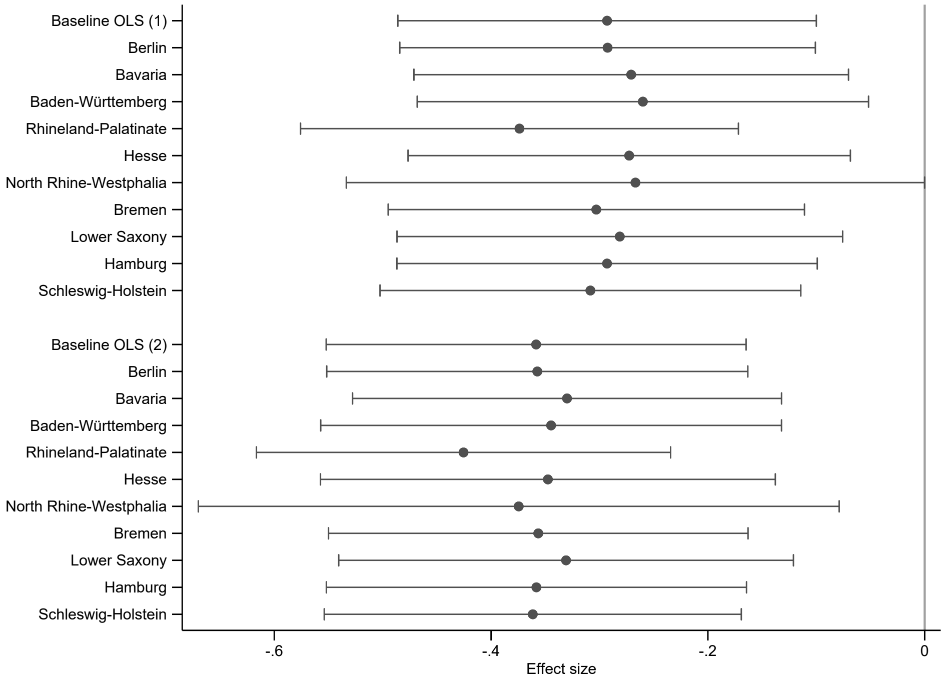
(a) Net value primary residence, OLS, second generation, fathers



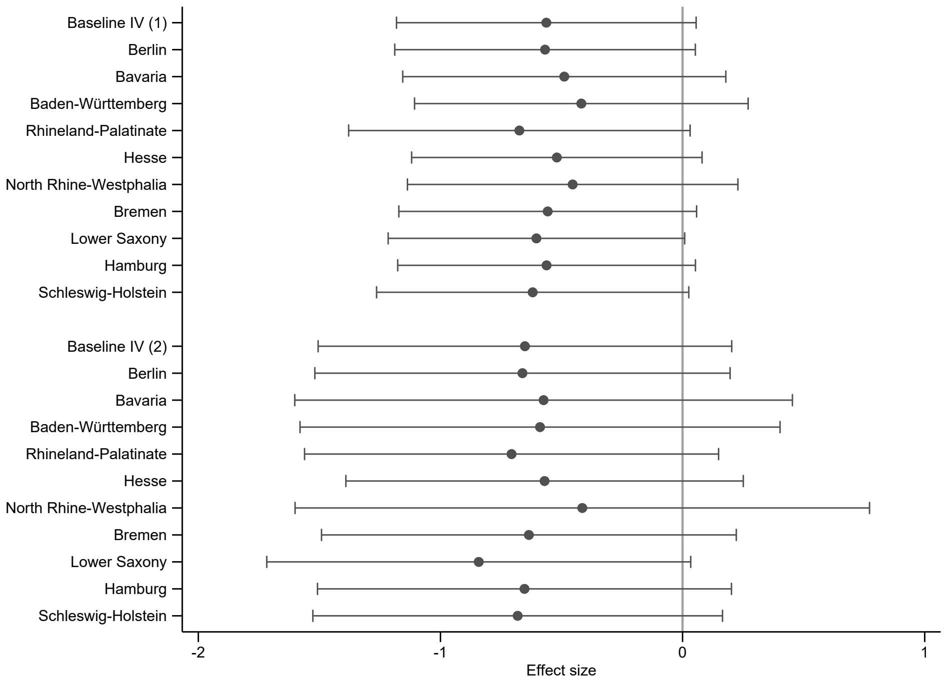
(b) Net value primary residence, IV, second generation, fathers

Figure 3.9: Regional jackknifing, effect of destruction, second generation, 1931-1945 father cohort.

Note: The graph shows the point estimates of the effect of destruction and the 95% confidence interval for regressions in which observations whose father was born in a specific federal state are dropped. The federal state is shown on the ordinate axis. Data: SOEP v35; own calculations.



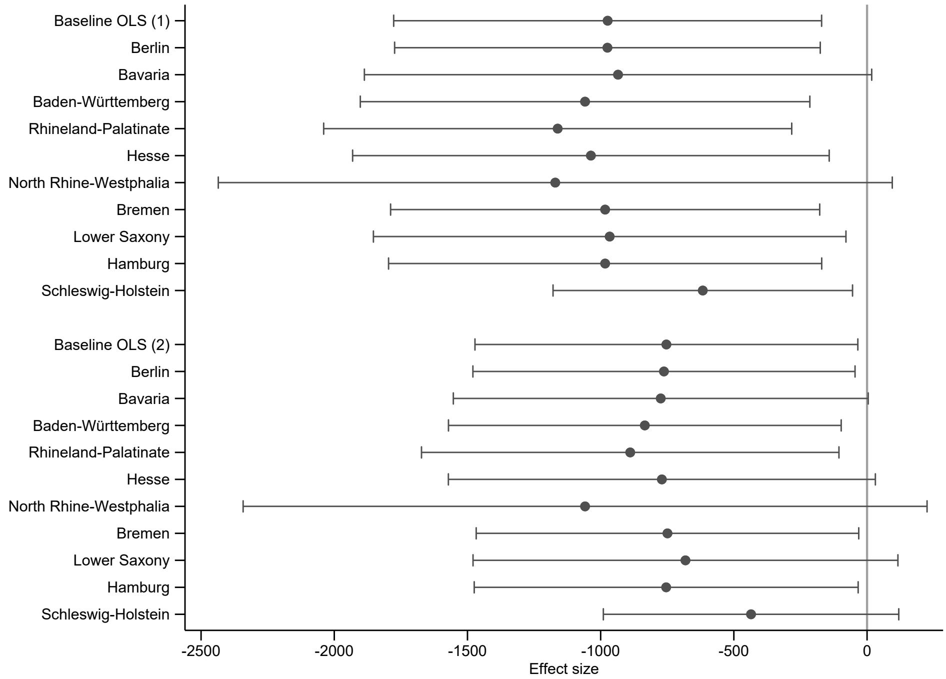
(a) Homeownership, OLS, second generation, fathers



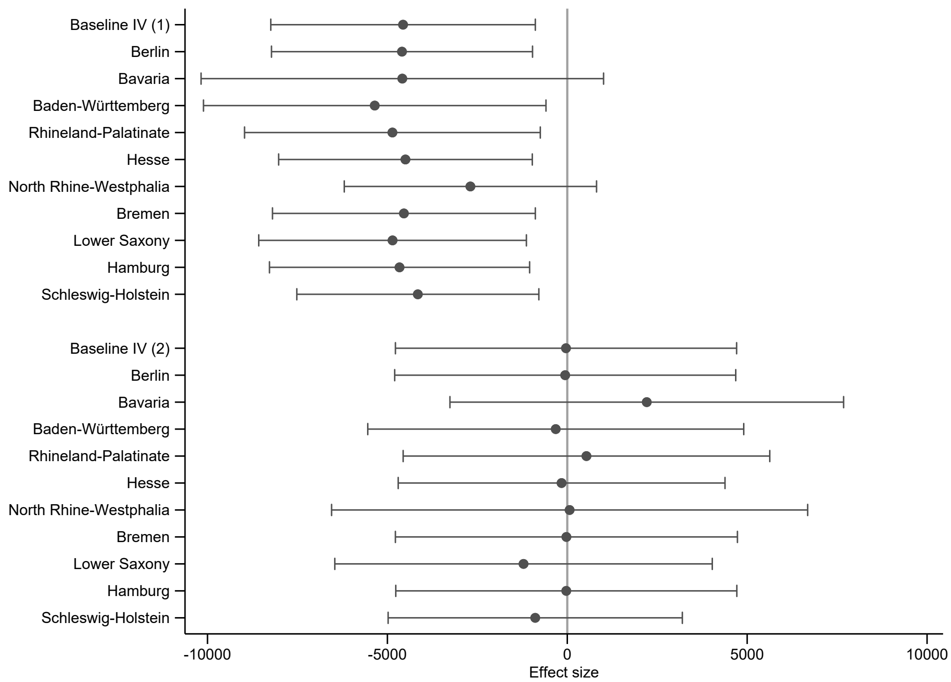
(b) Homeownership, IV, second generation, fathers

Figure 3.10: Regional jackknifing, effect of destruction, second generation, 1931-1945 father cohort.

Note: The graph shows the point estimates of the effect of destruction and the 95% confidence interval for regressions in which observations whose father was born in a specific federal state are dropped. The federal state is shown on the ordinate axis. Data: SOEP v35; own calculations.



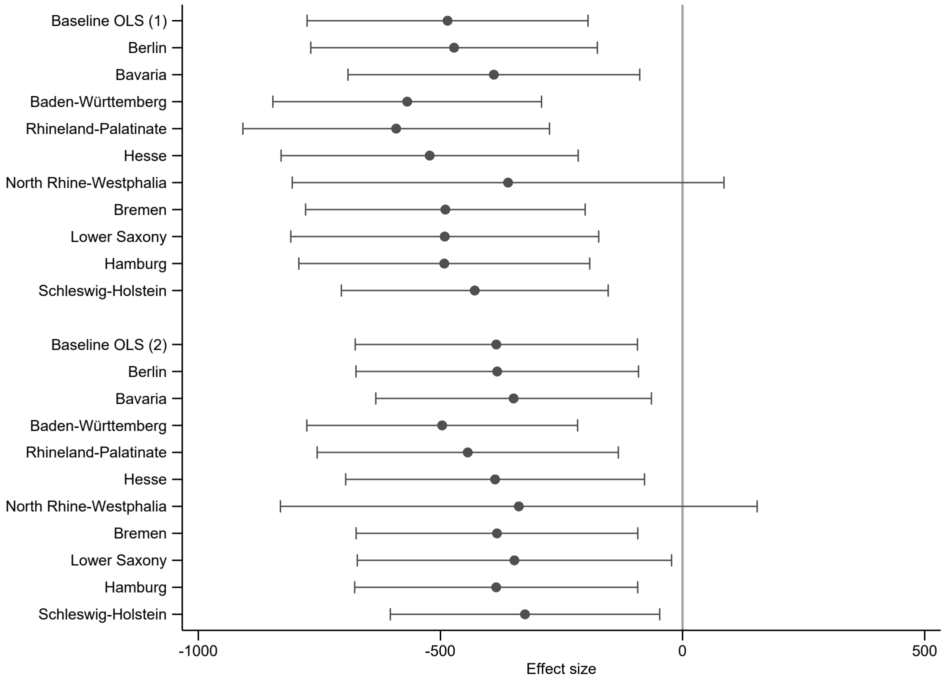
(a) Net wealth, OLS, second generation, mothers



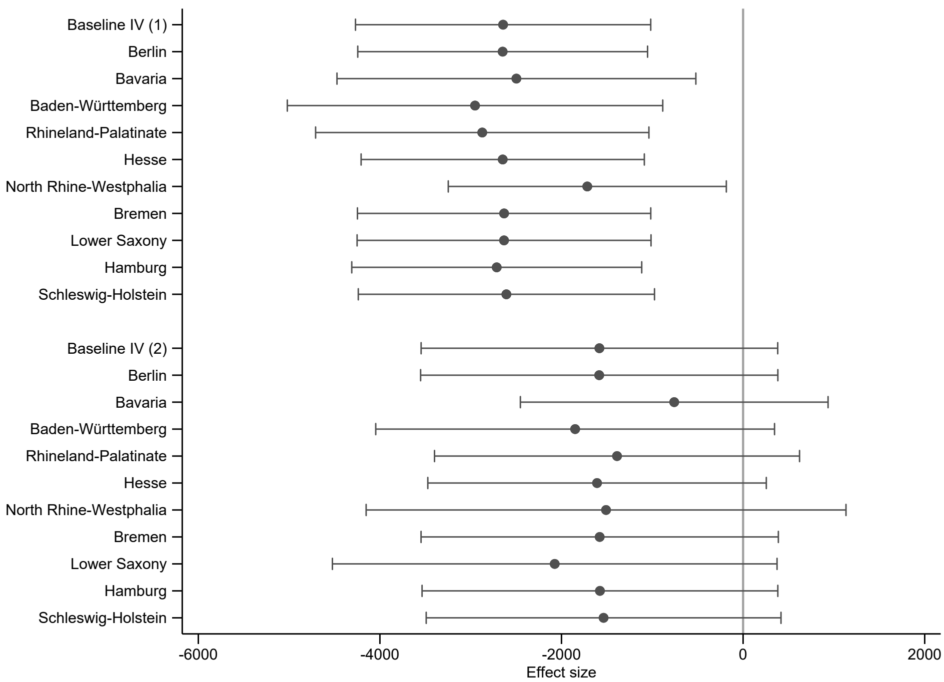
(b) Net wealth, IV, second generation, mothers

Figure 3.11: Regional jackknifing, effect of destruction, second generation, 1931-1945 mother cohort.

Note: The graph shows the point estimates of the effect of destruction and the 95% confidence interval for regressions in which observations whose mother was born in a specific federal state are dropped. The federal state is shown on the ordinate axis. Data: SOEP v35; own calculations.



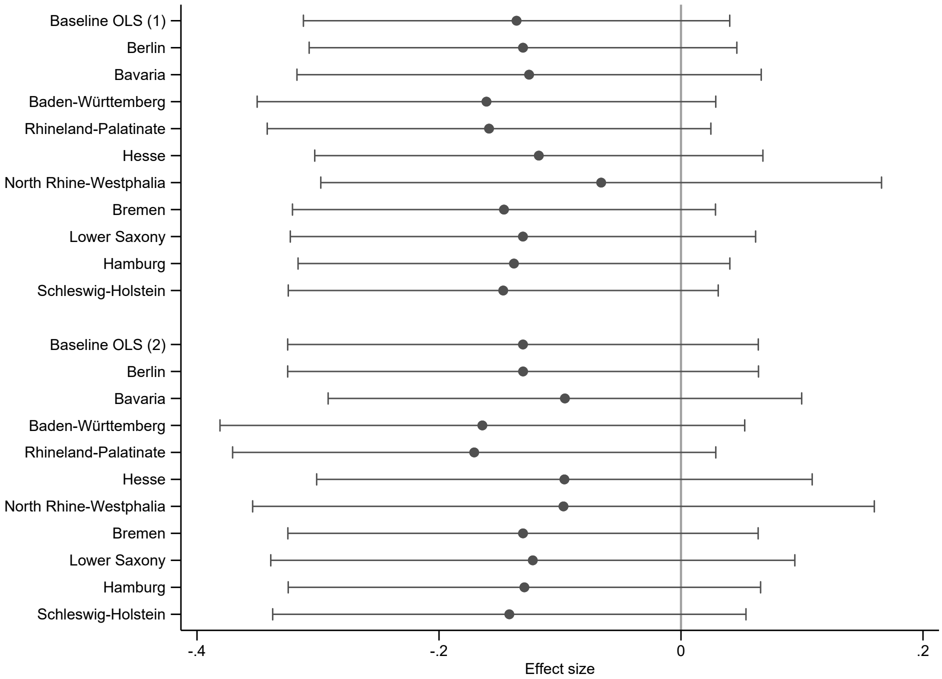
(a) Net value primary residence, OLS, second generation, mothers



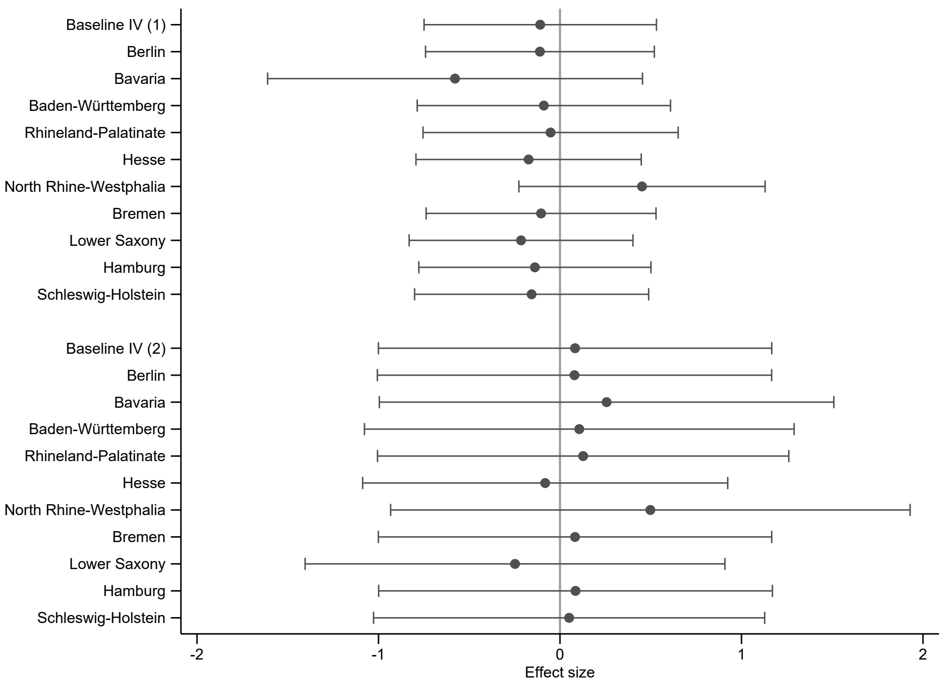
(b) Net value primary residence, IV, second generation, mothers

Figure 3.12: Regional jackknifing, effect of destruction, second generation, 1931-1945 mother cohort.

Note: The graph shows the point estimates of the effect of destruction and the 95% confidence interval for regressions in which observations whose mother was born in a specific federal state are dropped. The federal state is shown on the ordinate axis. Data: SOEP v35; own calculations.



(a) Homeownership, OLS, second generation, mother



(b) Homeownership, IV, second generation, mothers

Figure 3.13: Regional jackknifing, effect of destruction, second generation, 1931-1945 mother cohort.

Note: The graph shows the point estimates of the effect of destruction and the 95% confidence interval for regressions in which observations whose mother was born in a specific federal state are dropped. The federal state is shown on the ordinate axis. Data: SOEP v35: own calculations.

4 The Role of Characteristics and Behavior for the Development of the Wealth Gap Between Migrants and Natives in Germany

4.1 Introduction

Migrants in Germany and in many other countries have very little wealth and substantially less than the native population.[1] Low wealth levels expose migrants to a number of risks, including a greater risk of poverty in old age or during unemployment, a diminished financial well-being, and a lower social status (König et al., 2020). Moreover, large native-migrants wealth gaps are one element of increasing wealth inequality, a topic that has gained increasing attention in academic and political debates in recent years (Kopczuk and Saez, 2004; Piketty et al., 2006; Roine and Waldenström, 2015; Saez and Zucman, 2016; Albers et al., 2020).

This study seeks to contribute to the understanding of the origins of native-migrant wealth gaps by exploring how wealth gaps develop while migrants reside in the host country. The focus of the study differs from previous studies on native-migrant wealth gaps, most of which examine single cross-sections of wealth data to estimate differences in wealth between natives and migrants.[2] However, single cross-sections of wealth data do not show how wealth levels of individuals evolve over time. As a result, it is very unclear, for example, whether migrants are catching up with or falling further behind natives in terms of wealth, and to what extent differences in characteristics such as income, inheritances, or education contribute to this development of the native-migrant wealth gap.

This study exploits rich panel data from the German Socio-Economic Panel (SOEP), which contains several measurements of individual net wealth in different

[1]See Hao (2004) and Cobb-Clark and Hildebrand (2006a) for the United States (US); Cobb-Clark and Hildebrand (2006b) for Mexican-Americans; Gibson et al. (2007) for New Zealand; Bauer et al. (2011) for the US, Germany, and Australia; Mathä et al. (2011) for Italy, Luxembourg, and Germany; Vaira-Lucero et al. (2012) for Australia; Bertocchi et al. (2018) for Italy; Ferrari (2019) for Europe; Muckenhuber et al. (2022) for Austria. Note that Shamsuddin and DeVoretz (1998) find in an early study of Canada that migrants have more wealth than the native population.

[2]See footnote 1. The study by Shamsuddin and DeVoretz (1998) uses two cross-sections of wealth data, but is unable to observer the development of wealth levels at the individual or household level. Vaira-Lucero et al. (2012) uses panel data, but focuses on the relationship between wealth accumulation and subjective assessments of integration. Bertocchi et al. (2018) pool several waves of a rotating panel and do not analyze how individual wealth levels evolve over time.

years. Using these data, the study analyzes individual changes in net wealth over time, referred to as savings.[3] Savings are a measure of individual progress in terms of net wealth. Moreover, savings can be more easily related to monetary flow variables such as income and inheritances than wealth in levels, as argued by Gittleman and Wolff (2004) in their study on savings of blacks and whites in the US.[4] In this study, I therefore compare the savings distributions of migrants and natives and conduct a recentered influence function (RIF) decomposition analysis to identify which characteristics are most strongly associated with savings gaps, thereby giving migrants a relative (dis)advantage in terms of progress in the wealth distribution. The analysis concentrates on working-age migrants and natives, as in this age, individuals build their wealth stock, while in old age, they tend to dissave, which is a distinct process and beyond the scope of this study.

The SOEP survey data have several features that support the objectives of the study. First, respondents are asked every year on a wide range of topics, resulting in a rich set of individual characteristics that can be related to net wealth. Second, wealth data are collected from each individual separately, not collectively for a household. Accordingly, wealth can be readily matched to migration status and other individual characteristics. Finally, the data are representative for Germany, and migrants are oversampled, so that a large sample of migrants is available and reliable statements can be made about the native and migrant population as a whole (Goebel et al., 2019).

Looking at existing research, it is unclear whether migrants can reduce the native-migrant wealth gap over time. Like in other countries, migrants in Germany have, on average, lower education, less income, and lower wages than natives, *ceteris paribus* impeding their capacity to accumulate wealth.[5] In addition, wages do not converge substantially over time.[6] On the other hand, models by Djajić and Milbourne (1988) and Galor and Stark (1990) suggest that migrants have higher

[3]There is no consensus in the literature on whether to use the term savings or (net) wealth changes. For example, Dynan et al. (2004) use the term savings, while Gittleman and Wolff (2004) speak of wealth changes. This study uses the term savings because it is shorter. It shall be made clear that savings depend on active saving, that is non-consumption of income, as well as market returns to assets.

[4]One of the main challenges in explaining wealth in levels is that wealth builds up over many years, while monetary flows are observed for only a small fraction of years in many panel studies. As a result, important information is missing when trying to explain why one group has more wealth than the other. Estimates of lifetime or permanent income based on current income can mitigate the problem (Altonji and Doraszelski, 2005). However, in the case of migration, current income may be only a mediocre predictor of past income before migration because economic conditions in the home and host country may be different. In addition, savings as such may be of interest for policymakers, as savings depend more directly on host country conditions than does wealth in levels.

[5]See Bauer et al. (2005), Aldashev et al. (2012), Dustmann and Frattini (2012), Borjas (2015), De La Rica et al. (2015), or Ingwersen and Thomsen (2019).

[6]For Germany, see Fertig and Schurer (2007), Algan et al. (2010), and Okoampah (2016).

saving rates than natives, based on the assumption that migrants can re-emigrate to their home country, presenting them with distinct intertemporal consumption choices. Dustmann (1997) argues that migrants can also have lower saving rates if wage shocks in the home and host country are negatively correlated, as one labor market serves as an insurance against the other.

Empirical studies by Amuedo-Dorantes and Pozo (2002) in the US and Bauer and Sinning (2011) in Germany show that migrants have lower saving rates than natives. The latter study also finds that differences in saving rates disappear for migrants who intend to re-emigrate when remittances are classified as savings, highlighting the importance of remittances for migrant wealth accumulation. Fuchs-Schündeln et al. (2020) find that cultural background affects saving rates, while earlier work by Carroll et al. (1994) and Carroll et al. (1999) cannot confirm this relationship.[7] Apart from this, migrants and natives often have different wealth portfolios, which may result in distinct asset returns and household portfolio risks (Bertocchi et al., 2018). Similarly, some studies show that migrants are less likely to own a home in their host country,[8] which may affect savings, as homeownership is often positively associated with wealth accumulation and also affects labor market outcomes and consumption decisions (Oswald, 1996; Calcagno et al., 2009; Turner and Luea, 2009; Sierminska and Takhtamanova, 2012; Rossi and Sierminska, 2018).

In line with previous studies, the first finding of this study is that gaps in net wealth between migrants and natives are large. Working-age migrants possess, depending on the year, about 40 to 60 percent less net wealth than the native German population. Migrants also have very different wealth portfolios, marked by lower participation rates in private insurances, financial assets, and owner-occupied housing. Moreover, the study finds that the wealth gap in Germany narrowed between 2002 and 2012, and widened thereafter, partly due to significant immigration of relatively poor migrants. These findings extend previous estimates of the native-migrant wealth gap in Germany by Bauer et al. (2011) and Mathä et al. (2011), who document similar magnitudes, but are limited to specific years and the household level.

The next set of findings concentrates on savings and the convergence of wealth levels between migrants and natives, analyzing two representative balanced panels—one for 2002–2007 and the other for 2012–2017. In the first period, migrants and natives save similar amounts, with the result that the native-migrant wealth gap grows in some parts of the distribution and shrinks in others. In the second period, migrants accumulate less wealth than natives and the wealth gap grows in most

[7] The underlying hypothesis of these studies is that attitudes towards thrift and saving vary by country or culture and that individuals behave, at least to some extent, according to the cultural context from which they stem, potentially affecting wealth accumulation in the host country (Fernández, 2008).

[8] See Borjas (2002); Constant et al. (2009); Dustmann and Mestres (2010); Sinning (2010); Davidov and Weick (2011); OECD/European Union (2015).

parts of the net wealth distribution.[9] A common theme in both periods is that migrants have less income and receive less inheritances and inter-vivos gifts than natives. Decomposition analysis of the native-migrant gaps in savings shows that these are the characteristics that put migrants at the greatest disadvantage in terms of convergence of wealth levels.

Moreover, the study finds that many individuals dissave. Dissaving is more prevalent among natives, which offsets some of the advantages in savings. Dissaving is strongly associated with large shares of owner-occupied real estate in the wealth portfolio, especially in 2002–2007. In these years, natives had a larger share in their portfolios than migrants, which in the decomposition analysis explains to some degree why natives dissave more.[10] At the same time, larger shares in real estate are associated with positive savings, suggesting that homeownership has very heterogeneous effects on the savings distribution, which possibly depend also on the period of analysis. In 2012–2017, migrants and natives held similar shares of owner-occupied real estate, so that this characteristic played a smaller role in explaining savings gaps.

These results contribute to findings from studies that decompose native-migrant differences in wealth levels. Cobb-Clark and Hildebrand (2006a), Cobb-Clark and Hildebrand (2006b), and Bauer et al. (2011) find that wealth gaps are mainly explained by native-migrants differences in education levels and household composition. The present study examines these factors as well, but finds a lower explanatory power, which may be attributed to the fact that the outcome of this study's decomposition analysis—savings—is related to, but different than net wealth in levels. Moreover. the results contribute to a broader literature on the convergence of economic outcomes between migrants and natives. These show that differences, for example in earnings or homeownership, are relatively persistent and that convergence can take several decades or more depending on the country and the migration cohort (Sinning, 2010; Kerr and Kerr, 2011; Bauer et al., 2013). This study finds that wealth gaps are persistent as well, and highlights some of the underlying mechanisms. Lastly, the study contributes to the literature that shows that native-migrant saving rates between migrants and natives differ (Amuedo-Dorantes and Pozo, 2002; Bauer and Sinning, 2011). Results of this study suggest that there are also important differences using an absolute measure of savings.

The remainder of the paper is organized as follows. The next section describes the data and the different samples. Section 4.3 documents the development of the

[9]These trends are somewhat different to the aforementioned trends in the cross-sections of data because the population in a balanced panel is unaffected by in- and outmigration and additionally ages over time, which is not necessarily the case for the cross-sectional populations.

[10]There are various reasons why the share of owner-occupied real estate is associated with dissaving. 2002–2007 was a period when real estate prices in rural areas of Germany stagnated or declined (Knoll et al., 2017). Moreover, home-owners may have sold their home and consumed the proceeds, or gifted the home to other persons.

native-migrant wealth gap in Germany as a whole. Section 4.4 is concerned with the decomposition of savings within a balanced panel, with Section 4.4.1 presenting the savings distribution of migrants and natives, Section 4.4.2 outlining the RIF decomposition method, and Section 4.4.3 showing the distribution of characteristics. The decomposition results and robustness checks are presented in Sections 4.4.4 and 4.4.5. Section 4.5 suggests possible extensions to the present work. Section 4.6 concludes.

4.2 Data

The data I use is the German Socio-Economic Panel (SOEP), one of the few panel surveys that provides both a large and representative sample of migrants as well as several waves of detailed wealth data. The SOEP was initiated in 1984 and, as of 2020, comprises around 30,000 individuals in 20,000 households that are representative for the German population (Goebel et al., 2019). Migrants are oversampled, providing a sufficiently large sample of the population of interest. With respect to wealth, the SOEP has collected detailed information on assets and liabilities on a consistent basis every five years since 2002, allowing me to track the development of each individual's net wealth over time.[11] Assets and liabilities are collected from each adult in a household separately, so that I can conduct the analysis at the individual level.

In the analysis, key concepts are native/migrant, net wealth, and savings, defined as follows. Natives are individuals born in Germany, migrants are born outside Germany, but lived in Germany at the time of the survey. An individual's citizenship is not a criterion for classification.[12] Individuals born in Germany to migrant parents (second generation migrants) are included in the analysis and classified as natives. Next, net wealth is defined as all assets minus liabilities, both in current market values.[13] Assets also include wealth holdings outside of Germany.[14] Net wealth and

[11]The SOEP also included questions about household wealth in 1988, but these are not readily comparable to more recent years, mainly because asset values in 1988 were asked in categories, and not in continuous form.

[12]I do not use citizenship to classify individuals into migrants or natives because the goal of this paper is to analyze the wealth accumulation of individuals after they migrate.

[13]Assets include the primary residence, other real estate, financial assets such as stock and bonds, life insurances and private pension plans, net business assets, and tangible assets. Liabilities include the mortgage on the primary residence and other real estate, and consumer debts. In 2017, the SOEP started collecting the value of cars and debt on student loans, which I exclude from the analysis to make net wealth comparable over time. Further, in the 2002 wave, the SOEP questionnaire asked only for financial and tangible assets and consumer debts over 2,500 euros. In the SOEP data distribution, smaller values were imputed based on longitudinal information to make the different waves comparable (Frick et al., 2010).

[14]Dustmann and Mestres (2010) show that a significant share of migrants in Germany hold part of their assets in their respective home countries.

all other monetary values in this study are deflated with the consumer price index of the German Statistical Office to euros of 2002. The SOEP net wealth variable is multiply imputed, and I use the first set of imputed values as the analysis focuses on percentiles, to which Rubin's combination rules do not apply (Rubin, 2004). Main results for the other imputed datasets are provided in the Appendix. Lastly, savings are defined as the absolute change of net wealth over five years. Both net wealth and savings are censored at the 0.1th and 99.9th percentile to reduce the impact of outliers on averages.

The analysis is divided into two parts. The first part (Section 4.3) uses cross-sections of SOEP data to describe the development of the native-migrant wealth gap in Germany. In contrast, the second part (Section 4.4) relies on a balanced panel to analyze individual changes in net wealth over time (savings). Both parts focus on individuals in working age and exclude all individuals younger than 17 and older than 60.[15] Apart from this, no further sample restrictions are made in the first part and the data are weighted with cross-sectional survey weights to obtain estimates that are representative for the working-age population in Germany.[16]

In the second part, the selected individuals are required to have participated in two consecutive survey waves with a wealth questionnaire as well as in all four survey waves between those. Participation in two consecutive survey waves with a wealth questionnaire is necessary for the calculation of individual savings. The in-between survey waves provide additional necessary information, such as gained income, and they are also required for the calculation of longitudinal weights, as explained below. The analysis focuses on survey waves 2002 to 2007, because the period provides the largest sample of migrants.[17] Moreover, in Section 4.4.4.3, I contrast the results with the more recent 2012–2017 period. The samples of the two periods partly overlap, with 33.0 percent of natives and 18.8 percent of migrants in the 2002–2007 sample being in the 2012–2017 sample. Besides the panel criterion, and in accordance with the first part, I exclude persons who are younger than 17 older than 60 years in 2002 (2012). I also exclude a few observations with missing values in the explanatory characteristics (see Section 4.4.3) and observations lacking common support in these (see Section 4.4.2.1). I use longitudinal survey weights that correct for the selectivity in the sample and produce a sample that is representative for the year 2002 (2012).[18]

[15]The data do not contain wealth information for individuals younger than 17 years.

[16]Note that persons living in non-private households, such as retirement homes, are not part of the SOEP. Moreover, I have to exclude the SOEP refugee samples that were sampled from refugee arrivals beginning in 2015, as they were not surveyed for wealth.

[17]Note that extending the period to ten years reduces the sample size by about a half making the sample too small.

[18]Longitudinal weights are not directly provided by the SOEP, but are calculated by multiplying the 2002 cross-sectional weight with the inverse probability to stay in the survey until 2007 (inverse probability weighting). Probabilities to stay are provided by the SOEP on a year-to-year basis. If an individual does not participate in a given year, the year-to-year probability equals zero,

Table 4.1: Sample size and distribution of characteristics of different weighted samples.

	2002		2012	
	(1) Cross-section	(2) Panel 2002–07	(3) Cross-section	(4) Panel 2012–17
Average net wealth (euro)	69582.1	70255.3	56583.8	58486.3
Median net wealth (euro)	10000.0	11500.0	8560.9	10080.5
Average age	39.2	39.9	40.0	41.4
Average annual income (euro)	23664.3	23907.7	23499.3	24326.6
Homeownership rate (%)	32.9	33.7	32.1	34.0
Share migrants (%)	14.2	12.5	14.3	13.0
Migration-specific:				
Average age at immigration	21.3	21.3	19.6	18.8
Average years since migration	17.8	18.8	21.3	22.4
Born in EU-15 (%)	17.3	17.3	16.0	16.4
Born in Turkey (%)	20.1	18.5	11.4	13.2
Born in CEEC or Post-Soviet countries (%)	51.0	53.2	57.9	58.2
Born in other or unknown region (%)	11.6	11.0	14.7	12.2
Observations	17484	11445	19515	9989

Note: All variables are measured at the individual level. Net wealth and income in prices of 2002, and top- and bottom-coded at the 0.1th and 99.9th percentile. Income is before taxes as defined in Footnote 32. EU-15 are countries that belonged to European Union in April 2004. CEEC are Central and Eastern European Countries. Post-Soviet countries are successor states of the Soviet Union. The percentages of the countries of origin add up to 100. All figures weighted. Source: SOEP v35, own calculations.

A comparison of the different samples in Table 4.1 shows that the balanced panel samples of the second part are very similar in their characteristics to those of the cross-section samples of the first part. In the table, average weighted characteristics of the 2002 cross-sectional sample are shown in Column (1), and those of the 2002-2007 panel sample in Column (2). Analogously, Columns (3) and (4) present characteristics for the 2012 cross-sectional and the 2012–2017 panel sample. As becomes clear, the balanced panel requirement reduces the sample size significantly, but average characteristics in the cross-sectional and panel samples are very similar, suggesting that the longitudinal weights compensate for possible selectivity caused by panel attrition. For example, average net wealth in 2002 is 69,582 euros in the cross-sectional sample and 70,255 euros in the panel sample. Comparable small

implying that the longitudinal survey weight becomes zero and the individual is excluded from the sample. Requiring yearly participation appears very restrictive, but is, in fact, not much more restrictive than only requiring participation at the start and end of the panel (2002 and 2007), as most individuals who participated at the start and at the end also participated in the in-between years. An advantage of selecting persons who participated in all years is that income, inheritances, and other characteristics are observed for every year.

differences are found for the other characteristics—age, income, homeownership rate, and the share of migrants.

The table also shows migration-specific characteristics for the migrant subgroup. In 2002, migrants resided in Germany for an average of 17.8 to 18.8 years, depending on the sample. This time span is about 3.5 years higher in the 2012 samples. In both periods, the largest share of migrants originated from Central and Eastern European Countries (CEEC) as well as post-Soviet countries. Between 2002 and 2012, the share has grown by 6.9 percentage points at the expense of migrants from Turkey and EU-15 countries, indicating that the composition of migrants in Germany has somewhat changed over the decade.

4.3 The Native-Migrant Wealth Gap from 2002 to 2017

This first part of the analysis focuses on how the wealth gap between natives and migrants developed in Germany as a whole. As shown in Figure 4.1, levels of net wealth differ significantly between migrants and natives resulting in a large wealth gap that persists over time.[19] Depending on the year, migrants possess about zero euros at the median, while natives have between 9,000 to 13,000 euros. Mean net wealth of migrants is about 29,000 to 43,000 euros, while that of natives is between 60,000 to 76,000 euros, resulting in a gap of 41.4 to 60.2 percent.[20] The estimates are in line with estimates by Bauer et al. (2011) and Mathä et al. (2011), who find native-migrant wealth gaps of about 50 to 60 percent for Germany at the household level. In terms of development, Figure 4.1 shows that the native-migrant wealth gap decreased by about 44 percent from 2002 to 2012, and widened thereafter. The temporary decline is partly attributable to shrinking net wealth levels of natives and partly to an improved wealth position of migrants.

The development of native-migrant wealth gap depends, on the one hand, on the wealth accumulation of those individuals who stay and reside in Germany, which I examine in more detail in the next section. On the other hand, it depends on changes in the migrant population that result from renewed immigration and re-emigration. In Figure 4.2, net wealth of recently immigrated migrants is shown. Recently immigrated migrants—defined as migrants who arrived within the last five years—constitute between 3.8 and 11.8 percent of the total migrant population, depending on the year. In most years, their average net wealth is significantly lower

[19] The underlying numbers are presented in Table 4.10 in the Appendix.

[20] To calculate the percentages, I divided the mean gap by the mean net wealth of natives. If migrants' net wealth had been used in the denominator instead, the percentages would be between 70.7 and 151.0 percent.

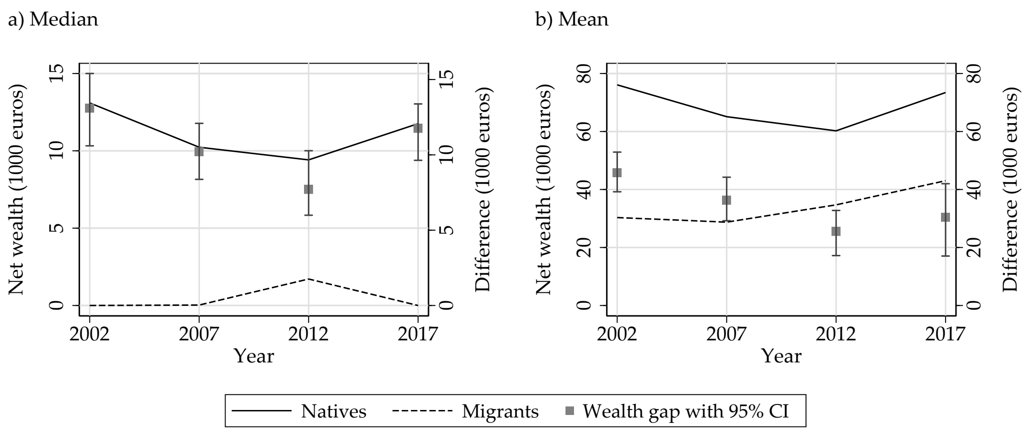


Figure 4.1: Native-migrant wealth gap in Germany 2002–2017.
Note: Representative sample of population in Germany between 17 and 60 years. Net wealth is in thousand euros of 2002, at the individual level, and top- and bottom-coded at the 0.1th and 99.9th percentile. Results for the imputation implicate *a* are shown. Results for the other implicates can be found in Table 4.10. All calculations weighted with cross-sectional weights. Confidence intervals based on clustered bootstrap standard errors. Source: SOEP v35, own calculations.

than that of the residing migrant population, indicating that their arrival on average widens the existing native-migrant wealth gap.[21]

Net wealth of re-emigrated migrants—defined as migrants who left Germany in the five years after the last wealth survey—is shown in Figure 4.3. They constitute a smaller share of the migrant population (2.6 to 4.3 percent), so that their departure has a limited effect on the native-migrant wealth gap. Although it is unclear how their net wealth would have developed had they stayed in Germany, the net wealth levels before departure give an indication. In 2002, their average net wealth is lower than that of the migrants who stay, suggesting that the departure decreased the existing wealth gap to some extent. In other years, re-emigrated migrants possess more wealth on average, so that their departure possibly increased the native-migrant wealth gap. Overall, the data indicate that changes in the migrant population have some effect on the native-migrant wealth gap, but the direction and magnitude of the effect vary over the years.

In addition to differences in net wealth, migrants and natives differ in the composition of their wealth portfolios, potentially affecting the saving behavior and wealth accumulation. As shown in Figure 4.4, the most prevalent asset classes among natives are private insurances and building loan contracts (60.5 percent in 2002), financial assets such as company shares (43.4 percent), and owner-occupied

[21] A large group of migrants arrived after 2011 from Eastern European countries, when the unconditional right to work was granted to citizens of new EU member countries (Baas and Brücker, 2011; Clemens and Hart, 2018). Note, though, that a relatively large inflow of refugees who arrived between 2015 and 2017 is not covered in the data, such that the native-migrant wealth gap in 2017 is likely to be underestimated.

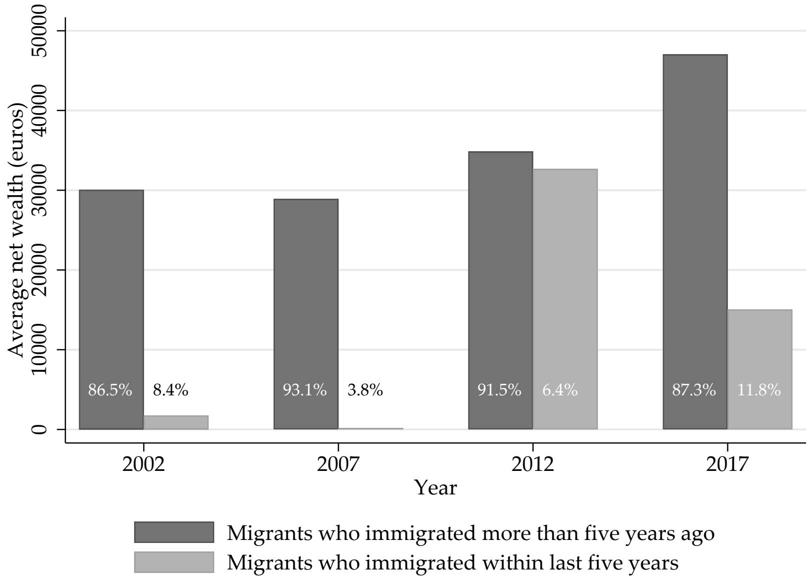


Figure 4.2: Average net wealth of recently immigrated migrants.

Note: Percentage numbers indicate each group's share of all migrants. Shares do not sum up to 100, as the immigration year is missing for some observations. Each sample is weighted such that it represents the migrant population aged 17 to 60 years in Germany in a given year. Net wealth in euros of 2002 at the individual level and top- and bottom-coded at the 0.1th and 99.9th percentile. Source: SOEP v35, own calculations.

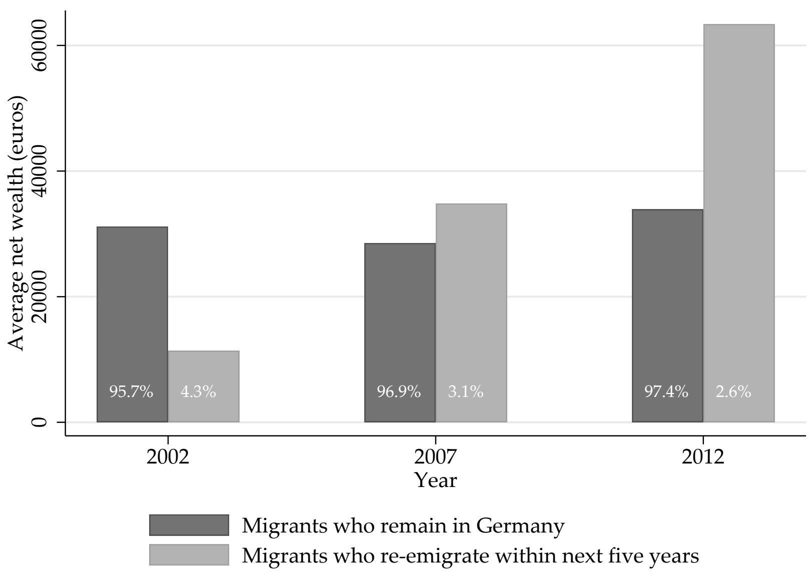


Figure 4.3: Average net wealth of re-emigrated migrants.

Note: Percentage numbers indicate each group's share of all migrants. Each sample is weighted such that it represents the migrant population in Germany in a given year. Net wealth in euros of 2002 at the individual level and top- and bottom-coded at the 0.1th and 99.9th percentile. Source: SOEP v35, own calculations.

primary residences (35.1 percent). These are also the most common assets among migrants, but participation rates are significantly lower. In 2002, the rates are 37.7 percent for private insurances, 21.9 percent for financial assets, and 19.8 percent for owner-occupied primary residences, suggesting that, compared with natives, a smaller share of migrants' wealth portfolios is impacted by, for example, real estate price developments.

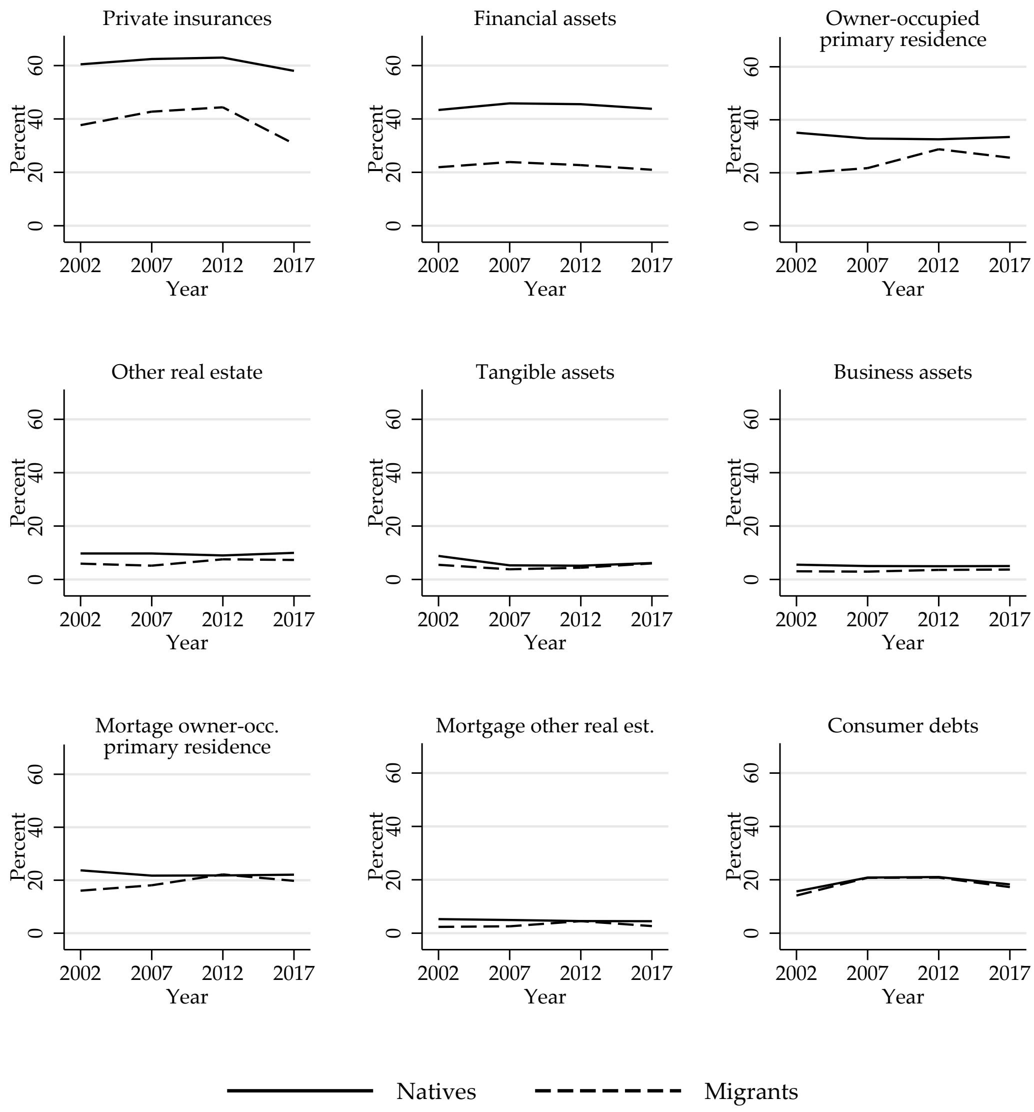


Figure 4.4: Asset participations rates of migrants and natives.
Note: Representative sample of the population in Germany aged 17 to 60 years. All calculations weighted with cross-sectional weights. Source: SOEP v35, own calculations.

An important trend over the sample period is the increase of owner-occupied housing among migrants from 21.9 percent to 25.7 percent, while the rate among natives declined slightly to 33.5 percent in 2017. This indicates that a certain economic integration took place and that a larger share of migrants is bound to Germany investment-wise. Further, the acquisition of a home is likely to have long-term effects on wealth accumulation, saving behavior, portfolio risk, and labor supply decisions.[22] Apart from this, among both groups, participation in private insurances and building loan contracts declined after 2012, a trend that might be related to the low interest rate environment of that time.

In summary, this section shows that there is large wealth gap between migrants and natives in Germany. The development of this gap is partly influenced by the arrival and departure of migrants, but potentially also depends on the wealth accumulation of those individuals who reside in the country. Differences in portfolio composition and—as the following sections show—economic means suggest that migrants and natives may accumulate wealth distinctively, which affects not only the overall native-migrant wealth gap but also individual welfare.

4.4 Decomposition of Savings

Given the substantial native-migrant wealth gap in Germany, this section explores whether migrants can improve their wealth position over time. Individual improvements in net wealth are measured by the savings variable, which is the outcome of decomposition analysis that follows. I analyze the difference in savings between natives and migrants and examine to what extent these differences can be related to differences in characteristics such as income, inheritances, and education. As described in Section 4.2, the analysis relies on a balanced panel of six survey waves (2002–2007), with the result that the population is kept fixed and the results are not affected by a changing population.

4.4.1 Distribution of Savings

During the sample period, migrants show a very different distribution of savings compared to natives, which becomes apparent from Figure 4.5. The figure depicts the percentiles of each group's saving distribution, as well as the gaps between them, which are decomposed thereafter. Notably, many percentiles are negative, meaning that a large share of individuals in both groups dissaves. The share is

[22] A large literature studies the effects of homeownership on wealth accumulation and related outcomes. For a review, see Dietz and Haurin (2003) and Rossi and Sierminska (2018). Often, homeownership is found to correlate positively with net wealth (Di et al., 2007; Turner and Luea, 2009; Rossi and Sierminska, 2018; Grabka and Halbmeier, 2019), while Kaas et al. (2019) find a negative causal effect.

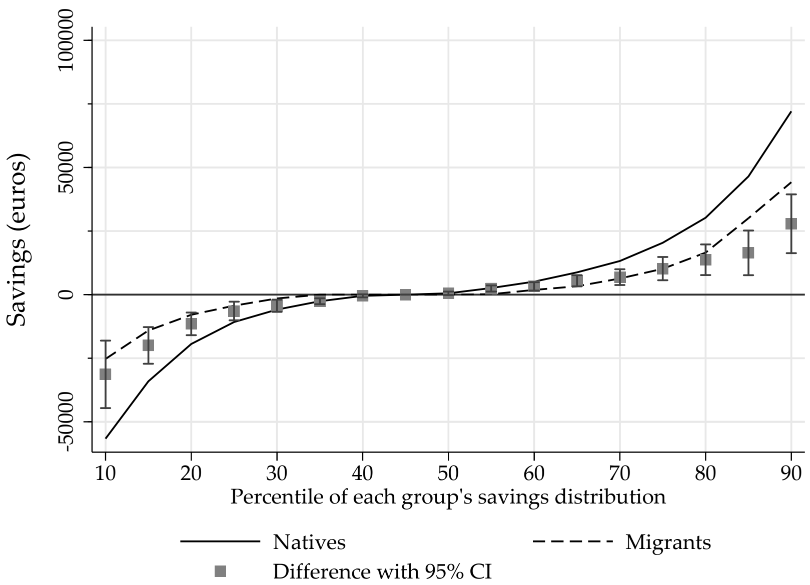


Figure 4.5: Savings between 2002 and 2007.

Note: Savings are defined as the absolute change of individual net wealth between 2002 and 2007. Panel sample as defined in Section 4.2. All calculations weighted with longitudinal weights. Confidence intervals based on clustered bootstrap standard errors. Source: SOEP v35, own calculations.

41.2 percent for natives, and 33.4 percent for migrants.[23] The gaps between each group's percentiles are relatively large and the sign of the gap varies depending on the location in the distribution. Approximately below the median, savings gaps are negative. For instance, the 25th percentile of natives is −10,766 euros, while that of migrants is −4,337 euros, resulting in a gap of −6,429 euros. Above the median, savings gaps are positive. For example, the 75th percentile of natives is 20,398 euros, and that of migrants equals 10,163 euros, resulting in a gap of 10,235 euros. Overall, the figure indicates that natives save and dissave larger amounts than migrants.

It is worth taking a step back and looking again at the gaps in net wealth to see whether migrants as a group improved in this regard relative to natives. On average, the native-migrant wealth gap in the panel sample does not change substantially between 2002 and 2007 (Table 4.2). In 2002, the average native-migrant wealth gap is 41,500 euros, and in 2007 it is 40,900 euros, a small decrease which corresponds to an average savings gap of −600 euros. In other parts of the net wealth distribution, gaps develop more substantially. For example, the median gap increases from 13,000 to 19,100 euros. This development of the native-migrant wealth gap differs from the trends found in the cross-sectional samples, which show a shrinking native-migrant wealth gap for the same period (Section 4.3). The different trends are likely due to the fact that the population changes in the cross-sectional samples, but is fixed in

[23]Negative savings can result from increased borrowing, but also from a devaluation or sale of assets, as savings are defined as the absolute change of net wealth over a five-year period.

Table 4.2: Distribution of individual net wealth (in 1000 euros).

		Percentiles				
	Mean	10th	25th	50th	75th	90th
			2002:			
Natives	75.4	0.0	0.0	15.0	85.2	195.2
Migrants	33.9	0.0	0.0	2.0	30.0	89.1
Gap	41.5***	0.0	0.0	13.0***	55.2***	106.1***
			2007:			
Natives	80.1	0.0	0.9	21.4	93.0	204.2
Migrants	39.2	−2.8	0.0	2.3	37.2	108.9
Gap	40.9***	2.8	0.9**	19.1***	55.8***	95.3***

Note: Panel sample 2002–2007 as defined in Section 4.2. All calculations weighted with longitudinal weights. Significance stars based on clustered bootstrap standard errors. * significant at 10%, ** significant at 5%, *** significant at 1%. Source: SOEP v35, own calculations.

the panel sample. This can have many implications. For example, by construction, individuals in the panel sample become older, while the population in the cross-sectional samples does not necessarily age on average, as young individuals enter and elderly individuals exit over the course of five years. Overall, it is worth pointing out the general differences regarding the native-migrant wealth gap in the two samples.

4.4.2 Methodology

4.4.2.1 RIF Decomposition Method

For the decomposition of savings gaps, I use the recentered influence function (RIF) decomposition method in its reweighted version laid out by Firpo et al. (2009), Fortin et al. (2011), and Firpo et al. (2018). Similar to other decomposition methods, the method decomposes the gap in savings into a part that results from differences in characteristics (composition effect) and another part that results from different returns to those characteristics (coefficient effect). The RIF decomposition is better suited than other decomposition methods for the data at hand because it allows for decomposing gaps between percentiles. Moreover, it can also estimate effects for specific characteristics conditional on the remaining characteristics.[24]

The RIF decomposition is similar to a Blinder-Oaxaca decomposition, but can also be used for distributional statistics other than the mean. Both methods estimate the composition and coefficient effect by calculating a hypothetical distributional

[24] The popular Blinder-Oaxaca decomposition (Blinder, 1973; Oaxaca, 1973), for example, only allows for decomposing means, while the DiNardo-Fortin-Lemieux decomposition (DiNardo et al., 1996) can only estimate the joint effect of several characteristics.

statistic of savings (quantile or mean) that would prevail if one group (natives) had the characteristics of the other group (migrants). In both methods, the hypothetical statistic is approximated linearly by multiplying one group's average characteristics with the other group's regression coefficients obtained from linear regression.[25] While a Blinder-Oaxaca decomposition uses the coefficients from an ordinary least squares (OLS) regression, the RIF decomposition uses those of a RIF regression. The reason is that RIF regression coefficients are an estimate of the so-called unconditional quantile partial effects (UQPE), that is, the response of an unconditional quantile to an infinitesimal shift of the distribution of characteristics. In comparison, OLS regression coefficients express the response of the unconditional (and conditional) mean (Fortin et al., 2011; Firpo et al., 2018).[26]

In case the UQPE is non-linear with respect to the characteristics, the linear approximation of the decomposition causes an approximation error as coefficients that express the effects of infinitesimal shifts in the characteristics are used to approximate the effects of large shifts.[27] To deal with a potential approximation error, I use the reweighted version of the RIF decomposition, as proposed in Firpo et al. (2018). While the reweighted version cannot correct for a potential error, it gives an idea of its magnitude. The idea is to estimate an additional set of regression coefficients for one group (natives) in a weighted regression, where this group is weighted in such a way that their characteristics resemble those of the other group (migrants). In other words, the reweighted natives coefficients are estimated in a domain of the characteristics that is akin to migrants. Differences between the unweighted and reweighted native coefficients are an indicator of non-linearities in the UQPE and are used as an estimate of the approximation error, also called specification error (Firpo et al., 2018).

That said, the principle decomposition equation of this study is the following:

[25] In theory, it is also possible to use the alternative counterfactual scenario in which migrants take the characteristics of natives. In practice, the data are such that there are more natives in the sample and it is easier to find natives who resemble migrants than the other way round. In addition, most studies on the native-migrant wealth gap chose the counterfactual I use here.

[26] As shown below, a RIF regression is an OLS regressions that has the RIF of savings on the left-hand and the characteristics of interest on the right-hand side. The RIF itself is the sum of the quantile of interest and the influence function. The influence function measures by how much the statistic of interest—in this case a quantile (percentile)—changes in response to marginal changes of the probability density distribution of the left-hand-side variable. Adding the quantile itself ensures that the expectation of the RIF equals the quantile, which is a necessary property for the decomposition.

[27] Note that the approximation error, in case of the RIF decomposition, has two sources. First, and similar to the Blinder-Oaxaca decomposition, the UQPE may be non-linear in the characteristics the same way the conditional expectation of an OLS regression may be non-linear. Second, and as becomes clearer from equations (4.2) and (4.3) shown below, the UQPE also depends on the overall distribution of the outcome. Every hypothetical change to the characteristics shifts the unconditional distribution of the outcome, thereby causing changes to the UQPE that are not embodied in the initial coefficients (Firpo et al., 2018).

$$
\begin{aligned}
\Delta_O^\theta &= q_\theta(Y_n) - q_\theta(Y_m) \approx \overline{\text{RIF}(Y_n, q_\theta)} - \overline{\text{RIF}(Y_m, q_\theta)} \\
&= \overline{X}_n\hat{\beta}_n^\theta - \overline{X}_m\hat{\beta}_m^\theta \\
&= \underbrace{(\overline{X}_n - \overline{X}_c)\hat{\beta}_n^\theta}_{\hat{\Delta}_{X,p}^\theta} + \underbrace{\overline{X}_c(\hat{\beta}_n^\theta - \hat{\beta}_c^\theta)}_{\hat{\Delta}_{X,e}^\theta} \\
&\quad + \underbrace{\overline{X}_m(\hat{\beta}_c^\theta - \hat{\beta}_m^\theta)}_{\hat{\Delta}_{S,p}^\theta} + \underbrace{(\overline{X}_c - \overline{X}_m)\hat{\beta}_c^\theta}_{\hat{\Delta}_{S,e}^\theta} \quad .
\end{aligned}
\tag{4.1}
$$

Δ_O^θ is the savings gap between the θth quantile of the native savings distribution ($q_\theta(Y_n)$) and the migrant savings distribution ($q_\theta(Y_m)$). $\overline{\text{RIF}(Y_n, q_\theta)}$ is the RIF value for natives averaged over all natives and $\overline{\text{RIF}(Y_m, q_\theta)}$ is the corresponding term for migrants. $\overline{X}_n$, $\overline{X}_m$, and $\overline{X}_c$ are vectors that contain the average characteristics of natives, migrants, and reweighted natives, respectively. $\hat{\beta}_n^\theta$, $\hat{\beta}_m^\theta$, and $\hat{\beta}_c^\theta$ are vectors of RIF regression coefficients for the native, migrant, and reweighted native sample.

$\hat{\Delta}_{X,p}^\theta$ is the RIF composition effect indicating the reduction—or increase if negative—of the savings gap if natives had the characteristics of migrants, but kept their coefficients. $\hat{\Delta}_{S,p}^\theta$ is the RIF coefficient effect indicating the reduction of the savings gap if migrants had the coefficients of natives, but kept their characteristics. $\hat{\Delta}_{X,e}^\theta$ is the specification error, as explained above. $\hat{\Delta}_{S,e}^\theta$ is the so-called reweighting error, which measures the quality of the reweighting by comparing average migrant characteristics to those of reweighted natives. Ideally both errors are zero; then the estimates of the reweighted and unweighted RIF are equal.

In the results section, I also report the sum of the RIF composition effect and the specification error, which equals $\overline{X}_n\hat{\beta}_n^\theta - \overline{X}_c\hat{\beta}_c^\theta$. This term equals in expectation the composition effect obtained from a DiNardo-Fortin-Lemieux (DFL) decomposition (DiNardo et al., 1996). As the DFL decomposition does not rely on linearity assumptions, the term can be regarded as a robust estimate of the true composition effect. In the results section, I refer to it as the overall composition effect.

Before estimating equation (4.1), the reweighting factors, the RIF of savings, and the beta coefficients have to be estimated.[28] As common in the literature, I estimate the reweighting factors using a logit model on the pooled sample of natives and migrants. The model has a zero-one dummy variable on the left-hand side indicating whether an observation is a migrant. On the right-hand side, I use all explanatory variables of the decomposition in a rich specification with quadratic terms and interactions, as shown in Table 4.8 in the Appendix. The method corresponds to the

[28]I use the Stata package oaxaca_rif by Rios-Avila (2019) for estimation.

estimation of reweighting factors in a DFL decomposition, as explained in DiNardo et al. (1996).

The RIF is, in the case of quantiles, defined and estimated as follows:

$$\text{RIF}\,(Y_{ig};q_\theta) = q_\theta(Y_g) + \frac{\theta - I(Y_{ig} \leq q_\theta(Y_g))}{f_{Y_g}(q_\theta)}\ , \quad \text{for } g \in \{m,n,c\}. \tag{4.2}$$

$f_{Y_g}(q_\theta)$ is the density of the distribution of Y_g at the θth quantile, $q_\theta(Y_g)$. $I(.)$ is an indicator function equaling zero if individual i's Y_{ig} is larger than the quantile, and one otherwise. Because of this, the RIF has only two values and can be thought of as a scaled dummy variable indicating whether an observation has more savings than the quantile. I estimate the density $f_{Y_g}(q_\theta)$ using a kernel density estimator with different bandwidths for different quantiles to avoid oversmoothing (see Section 4.7.2 in the Appendix).

The RIF regression coefficients are obtained from the following regressions:

$$\text{RIF}(Y_{ig}, q_\theta) = X'_{ig}\beta_g^\theta + \varepsilon_{ig} \quad , \quad \text{for } g \in \{m,n,c\}\ . \tag{4.3}$$

X_{ig} is a $N_g \times k$ matrix of $k-1$ explanatory variables and a constant, with N_g being the number of migrants or natives. The specification of X_{ig} is discussed in Section 4.4.2.2. β_g^θ is a $k \times 1$ vector of coefficients. ε_{ig} is an error term.

The following statistical properties of the RIF guarantee that decomposition equation (4.1) holds in expectation (Firpo et al., 2009). First, the expectation of the RIF equals the unconditional quantile: $\text{E}[\text{RIF}(Y_g;q_\theta)] = q_\theta(Y_g)$. This property ensures that the average RIF, $\overline{\text{RIF}(Y_g, q_\theta)}$, is a valid approximation for the observed unconditional quantile. Second the law of iterated expectations applies to the conditional RIF: $\text{E}_X\{\text{E}[\text{RIF}(Y_g;q_\theta)|X]\} = q_\theta(Y_g)$, where $\text{E}[\text{RIF}(Y_g;q_\theta)|X]$ is the conditional expectation of the RIF and E_X is the expectation with respect to X. This property ensures that the RIF coefficients, if multiplied with the average of X, equal the unconditional quantile (Firpo et al., 2009).

Apart from that, the identification of the composition and coefficient effect relies on various assumptions, two of which I want to discuss: First, a common support of X_m and X_n and, second, conditional independence of the error term (Fortin et al., 2011).[29] I ensure a common support by estimating the probabilities of being a migrant conditional on X and using the full sample. 24 observations have very small

[29] The conditional independence assumption is also called ignorability or unconfoundedness assumption. Fortin et al. (2011) list further assumptions that I assume as given: Mutually exclusive groups; existence of a structural form that describes the relation between observable and unobservable characteristics and the outcome; a simple counterfactual treatment (assumption of no general equilibrium effects); and the invariance of conditional distributions.

or very large probabilities. They are consequently dropped from the analysis.[30] The remaining observations have a common support.

The conditional independence assumption deals with the distribution of the error term. For identification of the decomposition effects, the conditional distribution, $\varepsilon_g | X_g$, is required to be the same among migrants and natives. Only then it is ensured that the error term is equally distributed among both groups in the counterfactual scenario, when natives get the observable characteristics of migrants. Otherwise, if the assumption is violated, any observed differences in Y_g possibly could stem from differences in ε_g, and, as a consequence, the estimated decomposition effects over- or underestimate the importance of group differences in X_g and β_g^θ.

The assumption of conditional independence is in this study, like in many decomposition studies, not testable. Moreover, this study does not rely on a random or quasi-random assignment of migration status so that no causal interpretation of the results should be made. This does not mean that the results are meaningless. Rather, the results show which characteristics are possibly relevant in the process of wealth accumulation, indicating relevant directions for future research on the wealth accumulation of migrants and natives.

4.4.2.2 Specification of the RIF Regressions

For the RIF regression presented in equation (4.3), I use a specification that adopts the idea that savings are a function of income, wealth transfers, asset returns, and additional characteristics of interest. For each quantile and group (migrants, natives, and reweighted natives), the specification is as follows, with the group index g being omitted for simplicity:

$$\begin{aligned} \mathrm{RIF}(\mathrm{Savings}_i^{02-07}, q_\theta) &\equiv \mathrm{RIF}(W_i^{07} - W_i^{02}, q_\theta) \\ &= \alpha^\theta + \boldsymbol{X}_i^{02-06}\beta_1^\theta + \boldsymbol{X}_i^{02}\beta_2^\theta + \varepsilon_i \quad . \end{aligned} \tag{4.4}$$

[30]Specifically, I calculate probabilities using Stata's teffects psmatch command, which implements propensity-score matching for treatment effects estimation. Treatment effects estimation and decomposition analysis are technically similar and require the same assumptions for unbiasedness and consistency (Fortin et al., 2011). I use the default limit of teffects psmatch for the exclusion of observations, which correspond to scores smaller than 0.000,01 or greater than 1 − 0.00001. To estimate propensity scores, I condition on the variables shown in Table 4.3, on interactions of the continuous variables (years of education, income, two risk measures, amount of wealth transfers, remitted money, change in household size, and the portfolio shares) with age group dummy variables, and further dummy variables (windfall income received, money remitted to foreign and to Germany). I also condition on all continuous variables squared and on all dummy variables interacted with each other. Household size enters as a set of dummy variables with one dummy per household size. Apart from this, I drop individuals from households with more than eight individuals because household size is a perfect predictor in their case.

Savings_i^{02-07} are individual i's savings between 2002 and 2007, defined as the absolute difference between net wealth of 2007 and 2002 ($W_i^{07} - W_i^{02}$). α^θ is a constant. β_1^θ and β_2^θ are vectors of RIF regression coefficients. ε_i is an error term as discussed in the previous section.

$\boldsymbol{X}_i^{02-07}$ is a matrix of of monetary flow variables observed for each year from 2002 to 2007: income, remittances, and wealth transfers. Before estimation, I sum up the yearly values to get a single total value and hence, a simpler specification. For example, the income variable is defined as: $\text{income}_i = \sum_{t=2002}^{2006} \text{income}_{it}$, where income_{it} is i's total income before taxes in year t.[31,32] One may consider including year-specific income variables or non-linear income terms to account for findings from the literature that saving rates increase with income (Dynan et al., 2004). On the other hand, Firpo et al. (2018) recommend using a linear specification for RIF decomposition analyses because of better interpretability of the results and the fact that a certain approximation error is unavoidable, since RIF regression coefficients are already a linear approximation to possibly non-linear effects (Section 4.4.2.1).[33] In the main analysis, I therefore use a linear specification and show in the robustness section that the main results are not affected by more complex specifications. In case of remittances and wealth transfers, I proceed analogously to income, but I also include zero-one indicator variables indicating whether an individual received a wealth transfer or made a remittance during the sample period, since the incidence of wealth transfers and remittances is relatively low.

[31] Values from 2007 are not included because I treat the data as if net wealth was observed at the beginning of 2002 and 2007. In reality, net wealth is observed during the year at the time of the survey. I have to make this simplification because monthly wealth or income data are not available, so that a breakdown by month is not possible.

[32] Specifically, the income variable I use incorporates labor income, asset income and dividends, public and private transfers, as well as social security and private pensions. Asset income is asked at the household level and I approximate the individual asset income by dividing household asset income by the number of adults in the household. I use the revised income variables provided in the SOEP, which contain imputed missing values. The total five-year amounts of income are censored at the 99th percentile to reduce the influence of very large values and to ensure convergence of models in the robustness section.

[33] Firpo et al. (2018, p.11) write: "Note that using a linear specification for the RIF-regression instead of a general function $m^\nu(X) = \mathrm{E}[\mathrm{RIF}(Y; \nu_t, F_t)|X]$ simply changes the interpretation of the specification error R^ν by adding an error component linked to the fact that a potentially incorrect specification may be used for the RIF-regression. We nonetheless suggest using the linear specification in practice for three reasons. First, we get an approximation error anyway since FFL's [(Firpo et al., 2009)] procedure only gives a first-order approximation to the impact of "large" changes in the distribution of X. Second, the linear specification does not affect the overall estimates of the wage structure and composition effects that are obtained using the reweighting procedure. Third, using a linear specification has the advantage of providing a much simpler interpretation of the decomposition, as in the OB decomposition. Our suggestion is thus to use the linear specification but also look at the size of the specification error to make sure that the FFL approach provides an accurate enough approximation for the problem at hand."

$\boldsymbol{X}_{i,02}$ is a matrix of explanatory variables measured for 2002 that have some notion of stock in it and can therefore be considered fixed over the sample period. These are indicator variables for age groups, indicators for the highest educational degree, indicators for household size, the change in household size between 2002 and 2007 to account for changes in the household composition, and self-assessed risk attitude in financial matters.[34] Among these explanatory variables are also proxies for the unobserved growth in the market value of the initial wealth portfolio. As a proxy, I use five asset share variables, each defined as the asset value in 2002 divided by total gross wealth in 2002.[35] It is, of course, possible that individuals change their wealth portfolio during the sample period. In the robustness section, I therefore test whether changes in homeownership—arguably the most important change in portfolio composition—affect the decomposition results. I find no significant impact, so that I use the simpler specification for the main analysis. Descriptive statistics for all variables are presented in the following section.

4.4.3 Descriptive Statistics of Explanatory Characteristics 2002–2007

The explanatory characteristics of the decomposition are summarized in Table 4.3, showing important differences between migrants and natives. Natives have, on average, more education, more income, and are more likely to receive wealth transfers (inheritances and inter-vivos gifts). Their total income gained over five years is 30,176 euros larger and the incidence of wealth transfers differs by 6.33 percentage points. The received amount of wealth transfers conditional on receiving some kind of wealth transfer is also higher, but does not differ statistically. Moreover, a smaller percentage of natives remits money to persons outside the household, but the remitted amount conditional on remitting is 1,855 euros larger. In terms of portfolio composition, natives hold a higher share of their wealth in the form of real estate and financial assets. In addition, the willingness to take risks in financial matters, measured on an 11-point scale from zero to ten (low to high), is 0.39 points higher among natives, while household size is significant smaller. For the other characteristics, no significant differences are found.

[34]Questions for risk attitude were only asked in the 2004, 2009, and 2014 survey waves, so that I use the 2004 measure.

[35]The five different asset types are “real estate”; “financial assets”; “business assets”; “debt”, including mortgages and other types of debt; and “other assets”, including life insurances, private pension plans, building loan contracts, and tangible assets. Together the five asset types sum up to total net wealth. I calculate the share in relation to gross wealth, not net wealth, because net wealth can be negative. In case net wealth is negative, asset shares are expressed relative to the total gross value of debt. In an earlier version of the paper, the absolute asset value instead of the asset share was used, which yielded similar decomposition results.

Table 4.3: Average characteristics of weighted panel sample 2002–2007.

	Reference year	Mean		Difference
		Natives	Migrants	
Age group (%):	2002			
Age less than 25		13.57	10.34	3.23*
Age 25–34		20.32	21.71	−1.39
Age 35–44		28.98	27.74	1.24
Age 45–54		24.28	25.85	−1.57
Age 55–60		12.85	14.36	−1.51
Female (%)	2002	50.84	53.84	−3.00
Highest educational degree (%):	2002			
No degree		2.59	3.50	−0.90
Compulsory		9.59	24.81	−15.22***
Secondary II		57.04	38.27	18.77***
Tertiary		30.77	33.43	−2.65
Total income in last 5 years (euro)	2002–06	125,171.79	94,995.77	30,176.03***
Wealth transfers in last 5 years:	2002–06			
Share (%)		13.32	6.99	6.33***
Received amount (euro)		25,335.00	17,847.51	7,487.49
Remittances in last 5 years:	2002–06			
Share (%)		26.12	37.62	−11.50***
Remitted amount (euro)		7,014.89	5,160.01	1,854.88***
Share of gross wealth (%):	2002			
Real estate		30.79	21.02	9.77***
Financial assets		17.07	9.92	7.16***
Business assets		1.88	1.36	0.51
Other assets		27.02	24.55	2.47
Liabilities		17.72	17.65	0.07
Willingness to take financial risks (0–10)	2004	2.61	2.23	0.39***
Household size (%):	2002			
1 person		18.46	7.80	10.65***
2 persons		28.84	23.29	5.55**
3 persons		23.13	24.58	−1.45
4 persons		21.23	27.82	−6.59***
5 and more persons		8.34	16.51	−8.16***
Change of HH size in last 5 y. (persons)	2002–06	−0.16	−0.12	−0.04
Observations		10130	1315	11445

Note: The column *Reference year* indicates the year to which a characteristic refers. The highest degree of schooling indicators correspond to the International Standard Classification of Education 1997 (ISCED97) provided by UNESCO (2006) as follows: *No degree* corresponds to ISCED97 code 0 (pre-primary education). *Compulsory* corresponds to codes 1 and 2 (primary education; lower secondary education). *Secondary II* corresponds to code 3 ((upper) secondary education). *Tertiary* corresponds to codes 4, 5, and 6 (post-secondary non tertiary education; first stage of tertiary education; second stage of tertiary education). The received amount of wealth transfers and remittances is shown conditional on having a positive amount. Based on panel sample 2002–2007 as defined in Section 4.2. All calculations weighted with longitudinal weights. Significance stars based on clustered bootstrap standard errors. * significant at 10%, ** significant at 5%, *** significant at 1%. Source: SOEP v35, own calculations.

4.4.4 Results

4.4.4.1 RIF Regression Coefficients for 2002–2007

Before presenting the descomposition results, it is instructive to examine the RIF regression coefficients, as they indicate how the different characteristics relate to savings. Point estimates are shown together with standard errors for the 25th, 50th, and 75th percentile in Table 4.4. Point estimates for additional percentiles are shown in Figures 4.6 and 4.7.[36] In the table, estimates are shown for migrants and unweighted natives, while those of reweighted natives are included in the figures. All indicator variables are scaled to one hundred, and the reference category is comprised of males between the ages of 18 and 24 who have no school degree, live in one-person households, and have not received any wealth transfer or made any remittance to friends or relatives between 2002 and 2007.

The estimates reveal several interesting patterns. Having more income has a positive and significant effect on savings, mainly on percentiles above the median. Among natives, for example, one additional euro of income is associated with a 0.129 euros increase of the 75th percentile of savings. Below the median, the effects of income are smaller and statistically insignificant, implying that there are some individuals in the sample with high income and negative savings. Wealth transfers have a clear positive effect as well, which is found for most percentiles. For migrants, the two wealth transfer variables—the yes-no indicator and the transferred amount—fit the data somewhat differently than for natives. These differences do not translate into significant coefficient effects for the wealth transfer variables (Section 4.4.4.2) and may be due to the relatively small number of wealth transfers within the migrant sample.

Among migrants, it stands out that returns to education are somewhat larger than those of natives and statistically significant, especially between the 50th and 75th percentile. This may result from different returns for individuals in the reference category, who have no school degree. With respect to remittances in the migrant sample, the yes-no indicator is negative and the remitted amount is positively associated with savings, indicating that larger remittances are associated positively with savings. This may indicate that migrants use remittances to build up wealth, for example, in their home country, or that the remittance variables capture unobserved characteristics such as a capacity to forgo consumption.[37]

The coefficients on the age indicators suggest that the life-cycle pattern of savings differs between migrants and natives. Among natives, older individuals generally

[36]The figures show coefficients for the 10th to the 90th percentile. I excluded coefficients for the 5th and 95th percentile as some of these are very large, distorting the overall picture and hiding more relevant aspects of other percentiles.

[37]The SOEP questionnaire defines remittances as payments or support to relatives or *other individuals* outside the household, which is a relative broad definition and may be understood as any type of monetary transfer.

Table 4.4: RIF regression coefficients for various percentiles.

	Natives			Migrants		
	25th	50th	75th	25th	50th	75th
Age 25–34	−64.525*	−18.657	70.501	−10.365	−1.997	−76.555
	(33.78)	(11.45)	(49.45)	(47.69)	(1.35)	(79.89)
Age 35–44	−118.902***	−37.107***	20.362	−30.150	−2.021	−153.499**
	(33.35)	(11.06)	(47.37)	(47.28)	(1.36)	(76.00)
Age 45–54	−117.593***	−27.915**	59.595	−18.924	−1.600	−80.858
	(32.48)	(11.37)	(50.00)	(44.49)	(1.30)	(75.04)
Age 55–60	−196.600***	−31.014**	45.292	−61.708	−2.851**	−129.533*
	(39.38)	(12.60)	(56.45)	(45.62)	(1.33)	(74.74)
Female	17.722	18.343***	53.351**	1.309	0.146	−5.841
	(17.96)	(5.47)	(23.81)	(19.47)	(0.57)	(29.32)
Income	0.009	0.023***	0.129***	0.017	0.001***	0.093***
	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.02)
Willingness to take financ. risk	−682.135*	45.637	390.239	−108.010	24.198**	635.159
	(408.33)	(121.36)	(489.82)	(368.54)	(11.69)	(597.35)
Compulsory schooling degree	−26.210	−20.301	69.409	1.989	3.303**	129.805**
	(30.36)	(17.99)	(59.75)	(49.17)	(1.42)	(58.74)
Secondary II schooling degree	−32.374	0.986	93.196	−9.786	4.229***	117.226*
	(29.74)	(17.37)	(57.12)	(46.16)	(1.44)	(63.75)
Tertiary schooling degree	−27.337	8.072	105.658*	4.044	4.292***	146.944**
	(34.12)	(18.03)	(60.89)	(49.40)	(1.49)	(63.62)
Wealth transfers (yes/no)	77.181***	41.314***	121.040***	92.094***	2.270**	29.538
	(25.50)	(7.64)	(37.20)	(33.01)	(1.04)	(62.06)
Wealth transfers (amount)	0.069*	0.027***	0.354***	0.009	0.002	0.176**
	(0.04)	(0.01)	(0.05)	(0.04)	(0.00)	(0.09)
Remittances (yes/no)	−16.380	6.072	−25.214	−16.468	−0.279	−47.244
	(21.52)	(6.64)	(28.47)	(19.68)	(0.61)	(32.12)
Remittances (amount)	−0.254*	−0.109***	−0.315*	0.083	0.006	0.624**
	(0.14)	(0.04)	(0.16)	(0.18)	(0.00)	(0.25)
2-person household	−13.537	−0.133	14.988	−52.355*	−0.728	9.683
	(27.23)	(8.15)	(37.51)	(29.31)	(1.03)	(52.36)
3-person household	29.761	7.517	6.633	−60.534*	−0.650	34.986
	(28.06)	(8.73)	(38.27)	(32.40)	(1.12)	(56.64)
4-person household	1.880	1.006	46.264	−45.668	−0.606	24.722
	(28.99)	(9.33)	(43.18)	(32.42)	(1.10)	(56.04)
5-and-more-person household	29.247	6.657	93.178*	−57.564	−0.988	22.317
	(40.45)	(11.54)	(51.81)	(35.88)	(1.18)	(59.37)
Change of household size	−839.613	−119.915	1,037.898	−912.922	30.615	1,187.957
	(1,013.58)	(307.17)	(1,160.69)	(982.71)	(34.28)	(1,539.06)
Share real estate	−556.529***	−37.205***	176.132***	−199.004***	−1.077	163.548***
	(26.29)	(8.35)	(35.92)	(30.06)	(0.86)	(46.44)
Share financial assets	−95.417***	44.747***	205.618***	−121.984***	0.369	116.831**
	(26.21)	(10.24)	(45.72)	(33.98)	(1.09)	(50.89)
Share business assets	−805.937***	−83.630***	44.393	−201.068**	−4.078	−309.822**
	(90.47)	(19.30)	(86.44)	(94.95)	(3.15)	(142.85)
Share other assets	−219.524***	−17.284**	23.450	−171.025***	−2.430***	48.185
	(23.73)	(8.64)	(35.94)	(26.25)	(0.76)	(38.21)
Share liabilities	228.009***	125.329***	192.410***	34.447	4.355***	232.958***
	(22.54)	(7.82)	(39.56)	(22.36)	(0.73)	(45.19)
Constant	23,119.638***	−3,098.722*	−29,498.299***	10,809.453**	−320.374**	−13,719.012**
	(3,055.72)	(1,706.73)	(5,830.66)	(4,249.47)	(154.41)	(5,860.03)
Observations	10130	10130	10130	1315	1315	1315

Note: Dependent variable is the recentered influence function of savings between 2002 and 2007. All monetary values in euros of 2002. All indicator variables are scaled to one hundred. Reference category: male persons aged between 18 and 24, no school degree, living in one-person households, no wealth transfers or remittances over the 2002 to 2007 period. Panel sample as defined in Section 4.2. Regressions weighted with longitudinal weights. Standard errors in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%. Source: SOEP v35, own calculations.

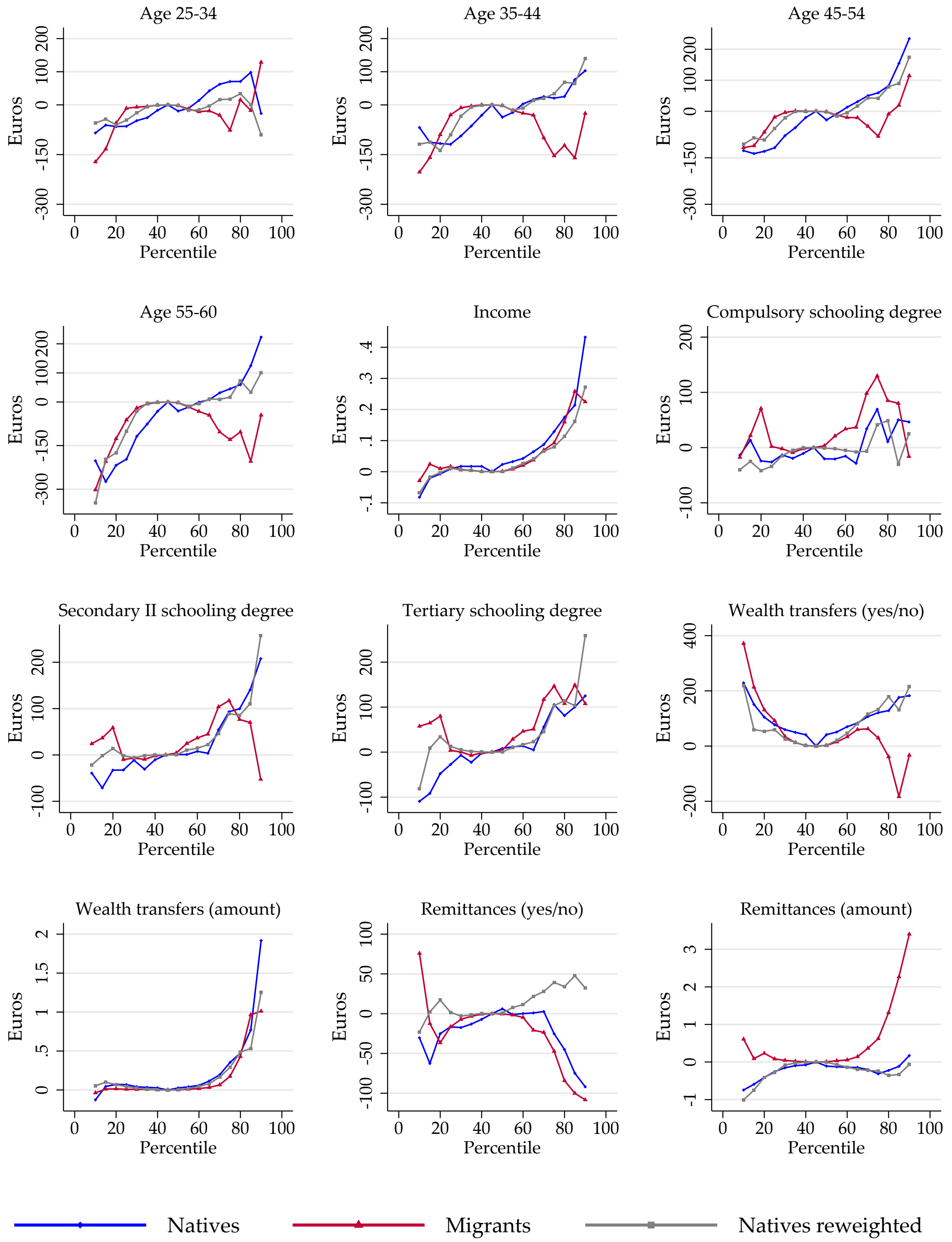


Figure 4.6: RIF regression coefficients (part 1).

Note: Dependent variable is the recentered influence function of savings between 2002 and 2007. All calculations weighted with longitudinal weights. Source: SOEP v35, own calculations.

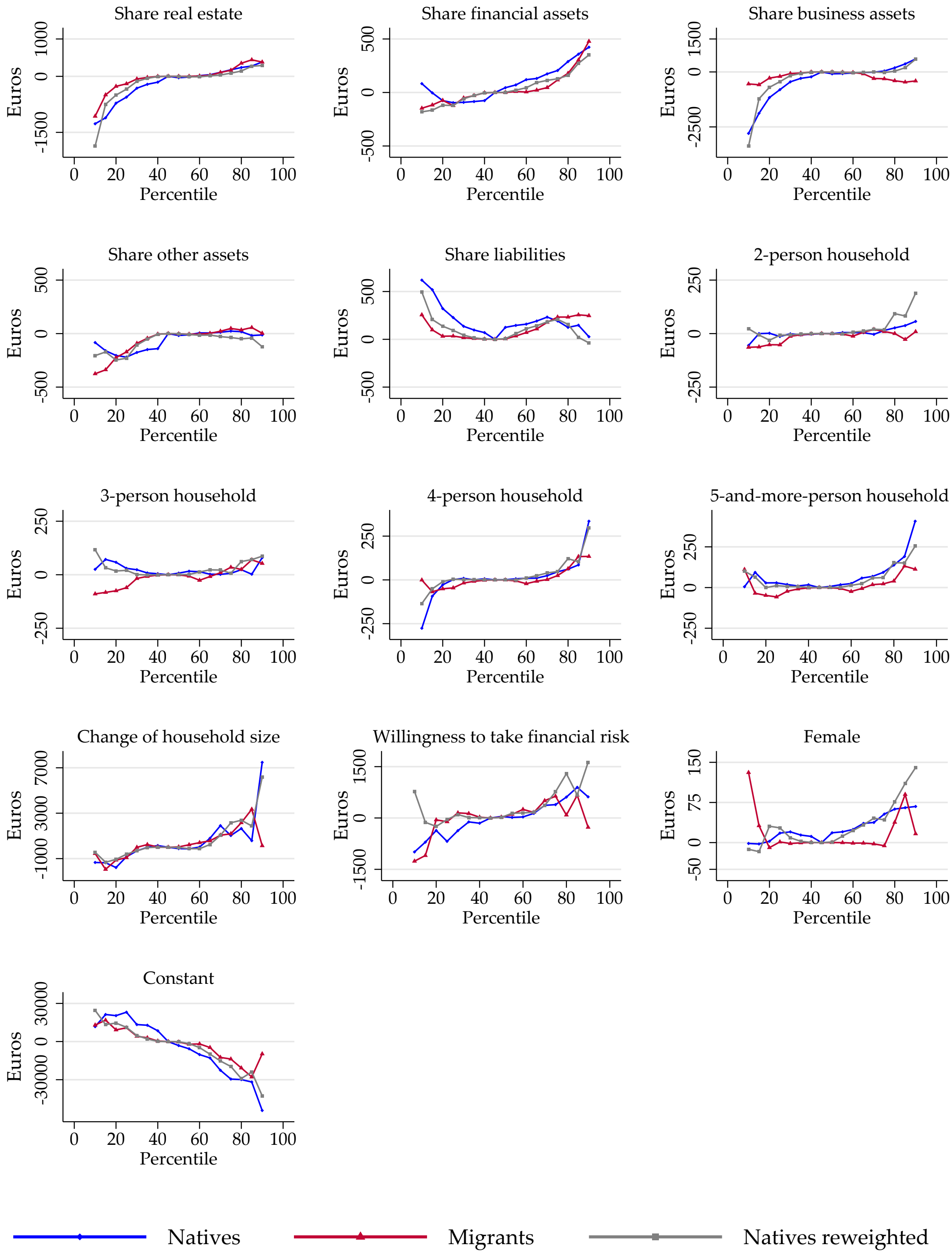


Figure 4.7: RIF regression coefficients (part 2).
Note: Dependent variable is the recentered influence function of savings between 2002 and 2007. All calculations weighted with longitudinal weights. Source: SOEP v35, own calculations.

save and dissave larger amounts than younger individuals. For example, the effect of being between 55 and 60 years old is negative at the 25th percentile (−196.6 euros) and positive at the 75th percentile (45.292 euros), which suggest that variance of the savings distribution is larger among older individuals compared to younger individuals. Among migrants, the positive effect associated with higher age is less evident in the data. It is possible that age interacts with migration-specific cohort effects, like the year of immigration or the country of origin, thereby picking up other unobserved characteristics, as for example, a capacity or willingness to save.[38]

Similar to age, larger proportions of real estate in the base-year portfolio have a negative and a positive effect, indicating that homeownership is associated with a larger variance of savings.[39] In the same manner, larger proportions of financial assets and business assets correlate positively and negatively with savings. Among migrants, the business asset share variable has substantially smaller effect sizes, suggesting that migrant business owners experience a smaller absolute volatility in the (business) asset stock compared to natives, potentially because they run smaller businesses, run them more cautiously, or have different access to finance.

Lastly, coefficients on household size indicate that larger households save—statistically insignificantly—more, potentially reflecting scale economies in consumption, or the desire of parents to save for their children. Moreover, being female is associated positively and significantly with savings, mostly among natives. In contrast, the self-evaluated risk attitude has no statistically significant effect.

Coefficients of the reweighted native regression resemble, by and large, those of unweighted native regression, indicating that the linear RIF coefficients are a good approximation for the decomposition. This is also confirmed by small and insignificant estimates of the specification error presented in the next section. In some cases, coefficients of reweighted natives differ markedly from those of unweighted natives, particularly for the remittances indicator and the age 25–34 indicator. In these cases, the linear RIF coefficients are potentially an inadequate representation of the effects of a hypothetical shift in characteristics. There is no clear guidance from theory as to which coefficients are a better approximation. However, in case of the two indicators, a back-of-the-envelop calculation indicates that the qualitative results

[38] I do not include migration-specific characteristics in the analysis since a decomposition requires that all characteristics are observed in both groups to obtain a common support.

[39] The two-sided effect of homeownership may reflect regionally heterogeneous real estate markets that developed in either a negative or positive direction. Further, the estimates might pick up transfers of real estate to other family or household members that are not taken into account by the remittance variable. Homeownership might also be associated with expenses for repairs and maintenance. Self-selection of saving-prone persons into homeownership is a possible reason for the positive effect. Moreover, homeownership might also constitute a commitment to save, either due to mortgage repayments or the relative low liquidity of real estate wealth, which prevents consumption, as pointed out by Rossi and Sierminska (2018). This is in line with the positive coefficients on the proportion of debt, which consists mostly of mortgages. Homeowners might also save more to hedge against expected future maintenance expenses.

of article would not change if, for example, coefficients of reweighted natives were used to weight shifts in characteristics. That is, in both cases, neither differences in age nor in remittances can significantly explain savings gaps between natives and migrants.[40] I therefore stick to the coefficients of unweighted natives.

4.4.4.2 RIF Decomposition Results for 2002–2007

RIF decomposition results for the 2002 to 2007 period are presented in Table 4.5, jointly for all characteristics and for the 10th, 25th, 50th, 75th, and 90th percentiles. The first row presents the savings gap as observed in the data, defined as native savings less migrant savings. The second row shows the estimated gap, which is the gap after the RIF transformation of savings. The next two rows show the overall composition and the overall coefficient effect, which are estimated without a linear approximation and equal in expectation the decomposition effects of a DFL decomposition. The next two rows state the RIF composition effect ($(\overline{X}_n - \overline{X}_c)\hat{\beta}^{\theta}_n$) and the specification error ($\overline{X}_c(\hat{\beta}^{\theta}_n - \hat{\beta}^{\theta}_c)$). Together these sum up to the overall composition effect. Similarly, the last two rows show the RIF coefficient effect ($\overline{X}_m(\hat{\beta}^{\theta}_c - \hat{\beta}^{\theta}_m)$) and the reweighting error ($(\overline{X}_c - \overline{X}_m)\hat{\beta}^{\theta}_c$), which sum up to the overall coefficient effect. All effects are also depicted for more percentiles in Figure 4.8.

The results show that the overall composition effect is relatively large, indicating that the savings gap was significantly smaller if natives assumed the characteristics of migrants while retaining the coefficients. At the 10th and 25th percentiles, the overall composition effect accounts for about 50 to 75 percent of the total savings gap. At percentiles above the median, it accounts for about 70 percent. Mirroring these estimates, the overall coefficient effect is specifically large and negative at the 5th to 15th percentile, where it accounts for about 50 percent of the (dis)savings gap. This means that migrants had similarly large negative savings as natives if they assumed the native coefficients, while retaining the characteristics. However, the coefficient effect is estimated with large standard errors and is not statistically different from zero on a five percent level at all percentiles except the 5th and 10th. The specification and reweighting error are small indicating that the linear RIF decomposition is a valid approximation to a decomposition based on reweighting. Relative to the total gap, the specification error ranges from an absolute minimum of 1.2 percent at the 55th percentile to a maximum of 22.3 percent at the 5th percentile. The reweighting error ranges from 0.6 percent at the 95th percentile to 3.9 percent at the 25th percentile.[41]

[40] The joint composition effect of all age variables would be approximately −38.2 euros at the 75th percentile if reweighted native coefficients were used, instead of the statistically insignificant −184.477 as shown in the main results. For remittances, the composition effect would be −417.4 euros instead of the statistically insignificant 317.715 euros in the main results.

[41] The percentages are expressed in absolute terms. Moreover, the percentage errors are larger at the 40th to 50th percentile as the total gap is very small.

Table 4.5: RIF decomposition of savings between 2002 and 2007.

	Percentile				
	10th	25th	50th	75th	90th
Observed gap	−31,338.844***	−6,429.194***	528.220	10,235.361***	27,872.145***
	(6,766.12)	(1,868.09)	(342.73)	(2,333.02)	(5,898.64)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
RIF decomposition, native coefficients					
Estimated gap	−32,258.116***	−6,477.923***	538.496	10,237.927***	27,229.149***
	(6,402.27)	(1,851.70)	(350.38)	(2,409.09)	(5,979.63)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
Overall composition effect	−16,331.285***	−5,045.345***	538.594*	6,772.424***	21,087.337***
	(5,137.45)	(1,288.13)	(308.19)	(1,628.40)	(4,290.83)
	[50.6]	[77.9]	[100.0]	[66.2]	[77.4]
Overall coefficient effect	−15,926.831***	−1,432.578	−0.098	3,465.503	6,141.812
	(6,170.06)	(1,478.93)	(148.55)	(2,676.78)	(5,921.43)
	[49.4]	[22.1]	[0.0]	[33.8]	[22.6]
RIF composition effect	−12,356.510***	−6,002.633***	996.167*	7,732.047***	22,348.156***
	(4,351.75)	(1,763.02)	(549.37)	(1,867.30)	(5,157.40)
	[38.3]	[92.7]	[185.0]	[75.5]	[82.1]
Specification error	−3,974.776	957.288	−457.574	−959.622	−1,260.819
	(3,060.90)	(882.75)	(403.06)	(1,209.60)	(4,766.51)
	[12.3]	[−14.8]	[−85.0]	[−9.4]	[−4.6]
RIF coefficient effect	−15,010.957**	−1,181.771	−6.901	3,149.550	5,510.475
	(6,188.07)	(1,447.97)	(141.10)	(2,623.10)	(5,920.33)
	[46.5]	[18.2]	[−1.3]	[30.8]	[20.2]
Reweighting error	−915.874	−250.807	6.803	315.953	631.337
	(942.51)	(199.04)	(17.32)	(206.45)	(609.31)
	[2.8]	[3.9]	[1.3]	[3.1]	[2.3]
Observations	11445	11445	11445	11445	11445

Note: Observed gap defined as native savings minus migrants savings. Panel sample as defined in Section 4.2. All calculations weighted with longitudinal weights. Clustered bootstrap standard errors in parentheses. Effect as percentage of estimated gap in square brackets. * significant at 10%, ** significant at 5%, *** significant at 1%. Source: SOEP v35, own calculations.

Composition effects of specific characteristics are shown in Figure 4.9 and Table 4.11, highlighting that the most important characteristics driving the composition effect are income, wealth transfers, and the base-year portfolio.[42] The importance of each characteristic varies substantially over the savings distribution. Below the median, gaps are mostly explained by differences in the base-year portfolio,

[42]Effects of similar characteristics are added up for the ease of interpretation: "age" includes the effects of the age dummies, "education" the effects of the education dummies, "income" includes simply income, "wealth transfers" the two wealth transfer variables, "remittances" the two remittances variables, "household size" the household size and the change of household size, "share of real estate" the share of owner-occupied real estate in the base-year portfolio, "shares of other assets" includes the other portfolio shares of assets, and "debt" the share of debt and mortgages. "risk attitude" corresponds to the willingness to take risks in financial matters, and "female" to the indicator for females.

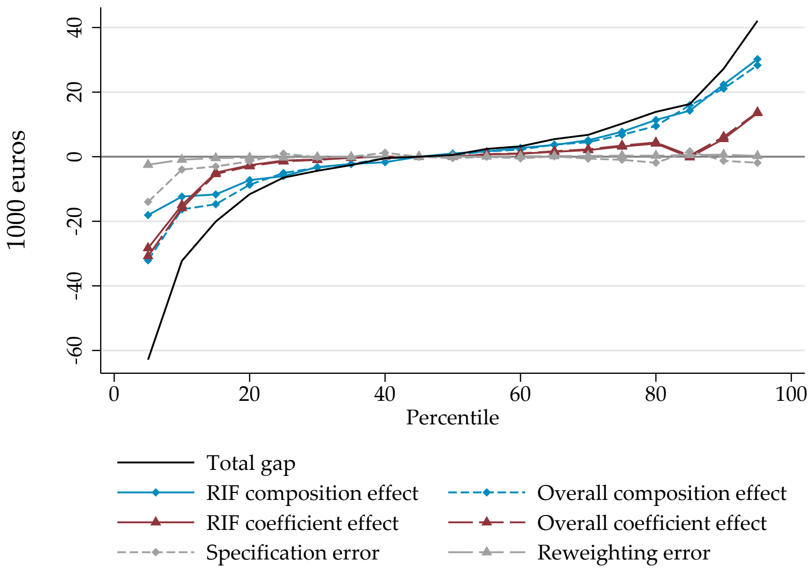
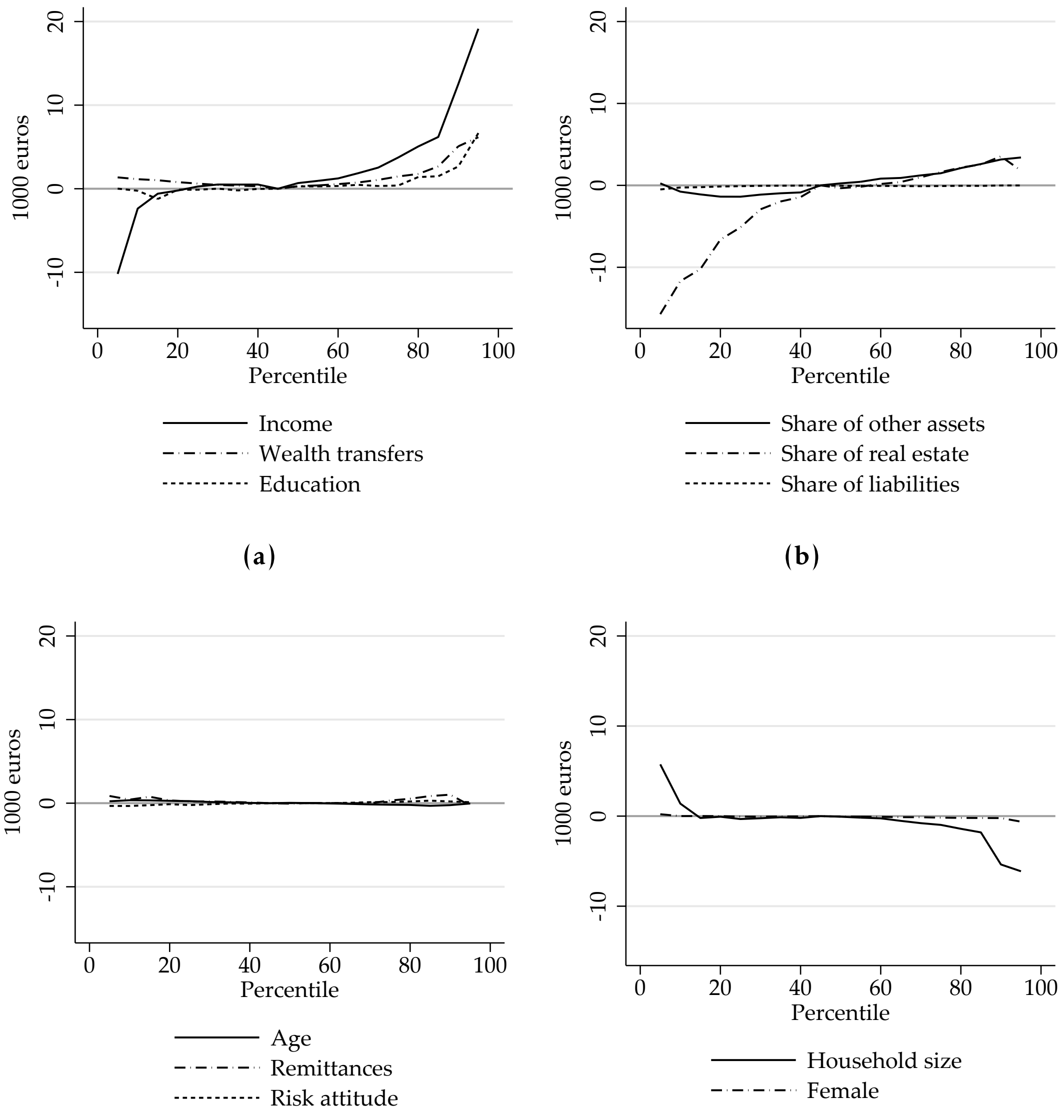


Figure 4.8: Decomposition effects, 2002–2007.

Note: Panel sample 2002–2007 as defined in Section 4.2. All calculations weighted with longitudinal weights. Source: SOEP v35, own calculations.

especially real estate, meaning that the native percentiles were up to 15,700 euros less negative and the gap 25 to 80 percent smaller if natives had the same smaller real estate shares as migrants. Above the median, it is a combination of higher income (35 to 45 percent of the gap), more and higher wealth transfers (15 percent), higher shares of real estate (5-15 percent), and higher shares of other assets (10-25 percent) that give natives an advantage in terms of savings. These effects are also statistically significant, as shown in Table 4.11. Differences in other characteristics explain relatively little, as presented in the lower subfigures. Only the base-year household size has an absolute effect of around 5,500 euros (around 10-20 percent of the total gap) at the tails of the distribution, implying that the (dis)savings gap were larger if natives had the same larger households as migrants.

Coefficient effects for specific characteristics are presented in Figure 4.10 and Table 4.12, showing that the sign and magnitude of the effects varies substantially over the savings distribution. Further, most of these coefficient effects are statistically insignificant due to the statistical uncertainty in the migrant coefficients. Nevertheless, some minor—statistically insignificant—patterns can be discerned. Migrants had more savings if they had the same high returns to household size as natives, as reflected by mostly positive coefficient effects of this characteristic. This might point to different scale economies of consumption, resource sharing, and labor supply in the household. On the contrary, migrants had less savings if they had the same returns to real estate as natives, which are often large and negative among natives at percentiles below the median. Similarly, the returns to education are



(c) (d)

Figure 4.9: Detailed RIF composition effects, 2002–2007.

Note: Panel sample 2002–2007 as defined in Section 4.2. All calculations weighted with longitudinal weights. Source: SOEP v35, own calculations.

somewhat lower for natives, resulting in mostly negative coefficient effects. Lastly, the coefficient effect of age is ambiguous, reflecting the different life-cycle patterns between natives and migrants pointed out in the previous section.

Overall, the results have different implications for the native-migrant wealth gap. Estimates of the composition effects suggest that disparities in income and wealth transfers are one of the main reasons why wealth levels between migrants

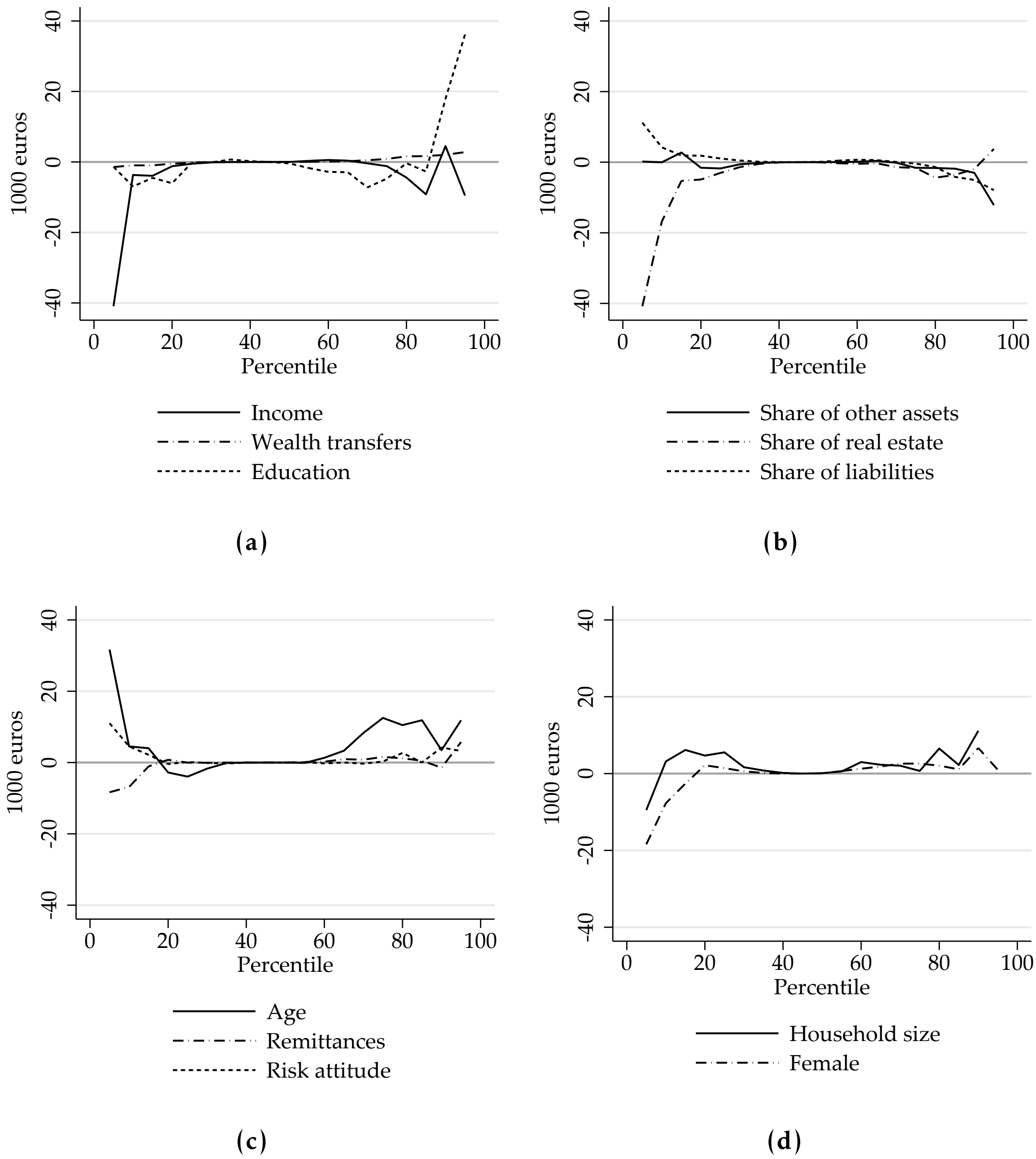


Figure 4.10: Detailed RIF coefficient effects, 2002–2007.

Note: Panel sample 2002-2007 as defined in Section 4.2. All calculations weighted with longitudinal weights. Source: SOEP v35, own calculations.

and natives do not converge. With respect to homeownership and real estate, results are ambiguous as real estate is associated with dissaving and savings alike. Some of the negative effects of real estate among natives may stem from wealth transfers to other family members such that owning real estate may have positive welfare effects on the family-level that are not captured in the analysis. Moreover, the period from 2002 to 2007 was characterized by a sluggish real estate market and homeownership is possibly more beneficial for wealth accumulation when markets boom (Kindermann et al., 2021). Contrary to findings by Bauer et al. (2011), I do not find significant effects for education, which may be attributable to the fact that I decomposes savings for a relatively short period of five years, while Bauer et al. (2011) decompose native-migrant gaps in total net wealth. On the other hand, it is possible that the estimates in Bauer et al. (2011) capture effects caused by selective immigration of low-educated, poor migrants. It should be reminded that also this article is no causal study, which means that, for example, an increase in migrants' income levels does not necessarily improve their wealth position if migrants respond by, for example, increasing the propensity to consume or remitting more money to their country of origin.

4.4.4.3 RIF Decomposition Results for 2012–2017

In this section, I summarize the results of a repeated decomposition analysis using the 2012–2017 panel sample to further highlight the mechanisms of migrant and native wealth accumulation. In 2012–2017, conditions are broadly similar to those in 2002–2007 and the main results of the previous section still hold. As in 2002–2007, migrants have less wealth than natives in 2012–2017 and the results indicate that they are unable to narrow the wealth gap due to having less income and receiving fewer wealth transfers.

However, there are some differences between the two periods that are worth pointing out. In 2012, the initial wealth gap is smaller on average than in 2002 (29,200 euros compared to 41,500 euros, Table 4.13), mostly because the average net wealth of natives is lower. Moreover, the growth of the native-migrant wealth gap is different in the two periods. While between 2002 and 2007 the native-migrant wealth gap grew in some parts of the distributions and shrank in others, between 2012 and 2017 native-migrant wealth gaps grew at most percentiles (Figure 4.12).[43] Savings of both groups are larger in the later period than they are in 2002–2007, reflecting the generally positive development of net wealth between 2012 and 2017. However, gaps in savings between both groups still exist, but these are smaller than in 2002–2007 and mostly not statistically different from zero—in part due to a smaller sample size of 947 migrants (Figure 4.13). In terms of characteristics, in 2012–2017, migrants still have significantly less wealth, earn less income, and receive less wealth transfers than natives (Table 4.14). On the other hand, migrants of the second period are more often homeowners than before and they invest a similar share of wealth into real estate as natives. Differences in terms financial and other assets still exist.

[43] Between 2012 and 2017, the average native-migrant wealth gap actually decreases. However, the development of the average wealth gap is largely driven by the 99th percentile of the migrant wealth distribution that developed far more positively among migrants than among natives.

Table 4.6: RIF decomposition of savings between 2012 and 2017.

	Percentile				
	10th	25th	50th	75th	90th
Observed gap	−9,538.740	−3,243.781***	184.763	4,417.592	6,424.094
	(5,849.16)	(903.60)	(1,561.19)	(5,687.51)	(19,786.66)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
RIF decomposition, native coefficients					
Estimated gap	−10,259.531*	−3,205.178***	164.578	4,402.967	6,210.026
	(5,504.79)	(873.69)	(1,583.84)	(5,535.00)	(20,086.96)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
Overall composition effect	−8,492.164**	−2,299.575**	699.635	6,325.687**	18,418.039***
	(3,732.74)	(911.87)	(741.99)	(2,721.15)	(6,196.46)
	[82.8]	[71.7]	[425.1]	[143.7]	[296.6]
Overall coefficient effect	−1,767.368	−905.603	−535.057	−1,922.721	−12,208.013
	(5,263.59)	(1,133.25)	(1,307.26)	(4,864.77)	(18,409.31)
	[17.2]	[28.3]	[−325.1]	[−43.7]	[−196.6]
RIF composition effect	−6,459.150	−1,828.839***	974.387	6,288.206**	17,326.952***
	(4,126.07)	(664.89)	(874.14)	(2,922.81)	(6,006.07)
	[63.0]	[57.1]	[592.1]	[142.8]	[279.0]
Specification error	−2,033.013	−470.736	−274.752	37.481	1,091.088
	(2,259.96)	(601.38)	(495.09)	(1,396.15)	(4,970.53)
	[19.8]	[14.7]	[−166.9]	[0.9]	[17.6]
RIF coefficient effect	−1,783.058	−900.552	−379.119	−1,739.194	−12,521.336
	(5,226.72)	(1,106.46)	(1,282.93)	(4,759.62)	(18,501.03)
	[17.4]	[28.1]	[−230.4]	[−39.5]	[−201.6]
Reweighting error	15.690	−5.051	−155.938	−183.526	313.323
	(667.67)	(92.15)	(143.22)	(467.35)	(1,279.60)
	[−0.2]	[0.2]	[−94.7]	[−4.2]	[5.0]
Observations	9989	9989	9989	9989	9989

Note: Observed and estimated gap defined as native savings minus migrants savings. Based on panel sample 2012–2017 as defined in Section 4.2. All calculations weighted with longitudinal weights. Clustered bootstrap standard errors in parentheses. Effect as percentage of estimated gap in square brackets. * significant at 10%, ** significant at 5%, *** significant at 1%. Source: SOEP v35, own calculations.

Decomposition analysis of the now smaller gaps in savings shows that the characteristics still give migrants a disadvantage in terms of savings (Table 4.6). Above the median, the composition effect is positive and its magnitude is larger than the savings gap itself, implying that migrants had larger savings than natives if they had the same characteristics. Characteristic-specific composition effects indicate that savings of migrants were larger if they had the same levels of income and wealth transfers. Effects for the latter are statistically insignificant, though (Figure 4.15 and Table 4.15). Different to the previous period, the share of real estate explains very little composition-wise, given that the share is similar among migrants and natives. Below the median, especially, the relatively small shares of financial and other assets in migrants portfolios—both are associated with dissaving—explain why migrants do not dissave as much as natives. Different to 2002–2007, the share in liabilities has a significant negative effect, meaning that migrants had more dissavings if they had the same smaller portfolio share of liabilities and mortgages as natives. This is an indication that mortgages and homeownership give migrants a relative advantage in 2012 to 2017.

The large composition effects for percentiles above the median are balanced by large negative, but statistically insignificant, coefficient effects, indicating that migrants receive generally larger returns to their characteristics than natives in the later period (Figure 4.16 and Table 4.16). This is one of the main differences to the 2002–2007 period, where migrants had smaller returns than natives at percentiles above the median. Most importantly in the period 2012–2017, migrants receive higher returns to the age dummy variables than natives, which points to a distinct saving behavior that is not explained by other characteristics in the analysis. Moreover, at some percentiles, migrants receive larger returns to income, household size, and risk attitude. Coefficient effects on the share of real estate and other assets have a minor importance in the later period, pointing to a convergence between the two groups.

In summary, the decomposition results show that differences in income and wealth transfers still put migrants at a savings disadvantage. However, migrants receive relatively larger returns to their characteristics in 2012–2017, although there is no clear indication in the data as to what drives these returns. One can only speculate here. It is possible that a ability or willingness to save is larger among migrants than among natives. It is also possible that migrants made better investment decisions or benefited differently from market price developments compared to natives. However, further analysis is required here.

4.4.4.4 Differences in Saving Rates

The previous sections show that migrants and natives have very different economic capacities to accumulate wealth and generate savings. Another fundamental aspect of wealth accumulation is the saving rate, which was only indirectly considered in the previous section by the estimation of RIF regression coefficients.[44] This section therefore compares the saving rates of natives and migrants more directly by asking whether the average saving rate differs significantly between the two groups. To stick closely to the previous analysis, I define the saving rate as the percentage ratio of absolute savings relative to all income and wealth transfers over five years, with savings, income, and wealth transfers defined as previously. Hence, the saving rate includes the growth of the existing wealth stock and differs conceptually from other definitions that define the saving rate, for example, as the share of unconsumed income.

[44] RIF regression coefficients, for example, on income cannot be interpreted as the saving rate per se, since they depend on the probability density distribution of savings as shown in Section 4.4.2.1.

Table 4.7: Differences in saving rates, OLS regression results.

	All observations		Negative savings		Positive or zero savings	
	I	II	I	II	I	II
			2002–2007:			
β_{Migrant}	7.564	4.330	25.198**	23.385**	−17.853***	−14.382***
	(5.93)	(6.21)	(9.89)	(9.68)	(4.23)	(4.32)
Income	Yes	Yes	Yes	Yes	Yes	Yes
Covariates of decomp.	No	Yes	No	Yes	No	Yes
Observations	11440	11440	4640	4640	6800	6800
			2012–2017:			
β_{Migrant}	12.701**	9.553**	4.194	8.240	9.358	7.494
	(5.17)	(4.57)	(4.83)	(5.14)	(6.20)	(5.41)
Income	Yes	Yes	Yes	Yes	Yes	Yes
Covariates of decomp.	No	Yes	No	Yes	No	Yes
Observations	9988	9988	3313	3313	6675	6675

Dependent variable: Saving rate in percent, censored at the 1th and 99th percentile. Both specifications include a migrant indicator variable with the coefficient β_{Migrant}. In addition, specification I includes income and specification II includes the same variables as the RIF decomposition in Section 4.4.4.1. Based on panel samples as defined in Section 4.2, excluding a few observations with zero income. All calculations weighted with longitudinal weights. Standard errors of clustered bootstrap standard errors in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%. Source: SOEP v35, own calculations.

For the comparison, I use the panel samples of the previous sections and regress the saving rate on an indicator variable that equals one if an individual is a migrant and zero otherwise. The coefficient on the indicator, β_{Migrant}, is an estimate of the average difference in the saving rate between migrants and natives in percentage points. Results are given in Table 4.7 for both 2002–2007 and 2012–2017 as well as for two different specifications. Specification I includes income as a control variable, while specification II includes the same explanatory variables as the previous RIF decomposition. In the table, the first two columns show results for the entire panel sample, while the following columns consider subgroups of individuals with negative savings and individuals with zero or positive savings.

Results for 2002–2007 show that migrants have an average saving rate that is 4.33 to 7.564 percentage points higher than that of natives, depending on the specification. However, the difference is not statistically significant. The higher saving rate results from the subgroup of individuals with negative savings, in which migrants have a significantly larger saving rate compared to natives, reflecting the large amounts of dissaving among natives between 2002 and 2007. To the contrary, among those with non-negative savings, migrants have a significantly lower saving rate than natives, resembling the positive coefficient effects estimated in the previous decomposition analysis for quantiles above the median. Results for 2012–2017 differ from those for the first period in that migrants have a larger saving rate than natives in both subgroups, resulting in a significant positive coefficient in the full sample.

This result is consistent with the negative coefficient effects found in the previous decomposition analysis.

Overall, the results indicate that native-migrant saving rate differentials depend on the period of analysis and the extent of dissaving in each group. This dependence suggests that periodic market price developments and cohort effects may play a role for the link between income and wealth accumulation. The results are also driven by the specific definition of the saving rate in this section, which captures reevaluations of the private wealth portfolio and (un)voluntary relinquishment of wealth.

4.4.5 Robustness

4.4.5.1 Alternative Income Specifications in the RIF Regressions

In the main analysis, the RIF regressions use a linear specification for the income variable, implicitly assuming that high-income individuals save the same share of their income as low-income individuals. This assumption may be unjustified, as some studies show that saving rates tend to increase with income (Dynan et al., 2004). Therefore, I assess the robustness of the previous results by relaxing the linearity assumption and repeating the decomposition analysis using different non-linear specifications of income in the RIF regressions: (1) a linear and a quadratic income term, (2) a linear, quadratic, and cubic term, (3) a linear and a logarithmic term, (4) five separate linear income variables for income in each year between 2002 and 2006.

The results in Table 4.17 indicate that none of the non-linear specifications yield results that differ substantially from the main findings. The aggregated decomposition effects are very similar across all specifications and percentiles. Moreover, the income-specific composition effects are very similar to those found in the main analysis as well, confirming the result that lower levels of income give migrants a disadvantage in savings. There are more substantial variations in the income-specific coefficient effect, which is estimated with large statistical uncertainty, as well as the income-specific specification error, which is an indicator for the fit of the model. Based on this measure, the best overall fit is achieved by the linear specification, although the other specifications outperform the linear specification at some percentiles. However, using different specifications for different percentiles would worsen the comparability between estimates. Moreover, the adjusted R^2 statistics of the underlying RIF regressions reported at the bottom of the table indicate that no specification outperforms the other, strengthening the notion that that the linear specification is the preferred specification.

4.4.5.2 Portfolio Changes: Transitions into and out of Homeownership

In the main analysis, the regression specifications use the portfolio composition in 2002 as a proxy for asset returns in subsequent years. The approximation may be inadequate if portfolios change and generate returns over the sample period for which the 2002 portfolio does not control. To assess the importance of portfolio changes for the robustness of the results, I repeat the analysis, introducing two indicator variables that control for possibly the most important portfolio change: changes in homeownership. The first indicator controls for the transition into homeownership. It equals one if an individual does not own a home in 2002, but does in 2007. Otherwise, it equals zero. Accordingly, the second indicator controls for the transition out of homeownership. It is equal to one if an individual owns a home in 2002, but not in 2007, and is zero otherwise.

Descriptive statistics for the 2002–2007 panel sample show that natives and migrants have very similar incidences of homeownership changes. Between 2002 and 2007, 8.91 percent of natives and 9.95 percent of migrants transition to homeownership, and 3.55 percent of natives and 1.82 of migrants transition out of homeownership. In the RIF regressions, transitions into homeownership are associated with positive savings, while transitions out of homeownership are negatively correlated with savings, both of which seem reasonable. These correlations result in negative RIF composition effects for the two indicators, which are statistically significant only for transitions out of homeownership (Tables 4.18 to 4.20). The interpretation is that natives had fewer dissavings and more savings if they had the same lower incidence of transitions out of homeownership as migrants. At first, this suggests that the relative high incidence of exits from homeownership among natives is beneficial for the convergence of wealth levels between migrants and natives. On the other hand, it is not clear from the data to whom homeownership is transferred, and it may be that there is a zero net effect at the household or family level.

Regarding the results of the main analysis, the repeated decomposition analysis shows that the introduction of the two indicators does not substantially affect the previous estimates, although the overall composition effect and some variable-specific composition effects become somewhat smaller. For instance, the RIF composition effect at the 75th percentile decreases from 75.5 percent to 65.3 percent in terms of the total savings gap after the introduction of the two indicators. Similarly, the composition effects of income and wealth transfers become somewhat smaller, indicating that these two correlate with—possibly—transitions into homeownership. In addition, the estimates for the share of real estate in the wealth portfolio change somewhat as well, with the composition effect becoming slightly smaller (less negative) at percentiles below the median and larger at percentiles above the median. All in all, these characteristics still explain a substantial portion of the savings gap, underlining the robustness of the main results.

4.5 Qualifications and Extensions

In many countries, there is a significant wealth gap between migrants and natives (Section 4.1). This study adds to understanding of the origins of the wealth gap by focusing on the development of native and migrant wealth levels over the individual life course. The strength of this study compared to other studies is the application of detailed panel data that allow the measurement of changes in net wealth at the individual level (savings). Savings measure each individual's progress in terms of net wealth, and by relating them to various individual characteristics, the main drivers of wealth convergence and divergence between natives and migrants are examined. Using RIF decomposition analysis, the study finds that especially less income, inheritances, and inter-vivos gifts put migrants at a disadvantage in catching up with natives in terms of wealth.

Despite the high quality of the data, several data improvements could strengthen the empirical findings of this study and reveal additional relevant aspects of the native-migrant wealth gap. First, sample size considerations result in a limitation of the period of analysis to five years. A larger sample would allow for the analysis of longer time periods, leading to a more complete picture of wealth formation. Similarly, a larger sample of migrants would reduce some of the relatively large statistical uncertainty in the estimation of RIF regression coefficients and allow for better analysis of differences in returns between migrants and natives. Second, the study lacks data on portfolio returns and investments. Such data would eliminate the need for the current approximation of returns by asset shares and allow for a more rigorous analysis of investment behavior of migrants and natives. Similarly, data on consumption and transfers of assets might reveal more interesting aspects of dissaving.

Methodologically, the principle limitation of the study is that the decomposition results are not causal and only valid under the assumption that migrants behave and decide like natives conditional on observed characteristics. While conceiving (quasi-)experimental studies in the context of wealth, inheritances, and income appears difficult, more detailed data, for example on investment decisions, may at least reduce the number of unobserved factors and possible biases. Apart from this, the study may benefit from additional robustness checks of the decomposition results. An alternative decomposition, in which differences in characteristics are weighted by the coefficients of migrants instead of those of native, would show how sensitive results are to the choice of coefficients. However, the possibilities here are somewhat limited due to the statistical uncertainty in the migrant coefficients mentioned previously. On a related note, additional robustness checks could be conducted with respect to the specification between savings and income, since the specification error for the income variable is relatively large (Table 4.17). Better specifications than those tested may exist, leading to smaller specification errors and possibly affecting mainly the (insignificant) income coefficient effects, since the composition effects do not seem to vary as much across the specifications tested. Another approach to the issue might be to split the sample into individuals who save and those who dissave. Saving and dissaving may be regarded as distinct processes that can be better analyzed by using separate samples and regressions.

There are several interesting extensions to the current study. One relevant extension, based on the feedback the study has received, is to analyze net wealth and savings of second-generation migrants. Second-generation migrations nowadays constitute a significant part of the German society (Statistisches Bundesamt (Destatis), 2021), and it is important to analyze whether there are systematic differences between them and natives with respect to wealth and savings. With additional data on the wealth of parents, questions of social mobility and migration could be explored. Next, future research may examine wealth accumulation of those migrants who recently immigrated. This would allow an analysis of the role of initial conditions, skills, and job market integration for subsequent savings. Similarly, the analysis of other specific subgroups of migrants, such as entrepreneurs, may reveal further relevant dynamics of the native-migrant wealth gap. Moreover, the importance of homeownership and initial settlement for wealth accumulation could be analyzed using regional data on housing prices. Such data would show whether migrants live in areas that are systematically different in terms of house prices than areas where natives live and whether this affects savings. Next, the study could be directed towards the literature on saving rates, which uses, among other measures, wealth changes relative to income (Dynan et al., 2004). This would complement previous empirical work on native-migrant differences in saving rates by, for example, Amuedo-Dorantes and Pozo (2002) and Bauer and Sinning (2011). Another possible direction is the analysis of savings at the household level. Finally, it would be interesting to assess to what extent results of this study differ depending on whether savings or net wealth in levels are decomposed.

4.6 Conclusion

This study adds to understanding of the origins of the native-migrant wealth gap by examining the development of native and migrant wealth levels over the individual life course. Based on detailed wealth data from the Socio-Economic Panel (SOEP), the study finds that migrants have significantly less net wealth than natives throughout the 2002 to 2017 period. Exploiting the panel dimension of the data, the study shows that migrants in working age cannot catch up significantly with natives in terms of net wealth over a period of five years because they lack sufficient levels of income, inheritances, and inter-vivos gifts. The results also indicate that especially native individuals consume, transfer, or lose significant amounts of wealth over time, which reduces the pace at which the wealth gap widens.

Overall, the results suggest that is very difficult for migrants to reach the same levels of wealth as natives given the low initial levels of wealth and the reduced availability of economic means. These aspects imply that many migrants cannot benefit to the same degree as natives from the many positive functions that wealth has for financial security, well-being, and social status. Migrants are also more likely to depend on the social security system in times of economic hardship and in old age, as many of them lack substantial amounts of savings. Moreover, the lower likelihood of inheritances among migrants indicates that disadvantages in wealth persist across generations, with potentially negative consequences for intergenerational mobility.

The results point to several interesting directions for future research to improve the understanding of native-migrant wealth gaps. First, it would be interesting to analyze the development of wealth gaps over longer periods than the five years in this study to obtain a more complete picture of wealth formation. Similarly, an analysis of migrants' wealth levels starting from the point of immigration would reveal the role of selective immigration policies and subsequent host country conditions for the native-migrant wealth gaps. Moreover, for aspects regarding intergenerational mobility, it would be relevant to analyze net wealth and savings of second-generation migrants to determine the extent to which differences in wealth persist across generations. Finally, future studies could examine the role of regional conditions, such as local housing markets and real estate price developments, for wealth formation and the native-migrant wealth gap.

4.7 Appendix

4.7.1 Specification of Logit Model for the Estimation of Reweighting Factors

Reweighting of the native sample requires the estimation of reweighting factors, which I estimate using a logit model on the pooled sample of natives and migrants. The model has a zero-one dummy variable on the left-hand side indicating whether an observation is a migrant. On the right-hand side, I use all explanatory variables of the decomposition analysis in form of a flexible specification shown in Table 4.8.

Table 4.8: Specification of logit model for the estimation of reweighting factors.

	Linear	Squared	Interacted with
Age group (0–1):			
Age less than 25	x		Total income, change of household size
Age 25–34	x		Total income, change of household size
Age 35–44	x		Total income, change of household size
Age 45–54	x		Total income, change of household size
Age 55–60	x		Total income, change of household size
Female (0-1)	x		Total income, change of household size
Highest educational degree (0–1):			
No degree	x		
Compulsory	x		
Secondary II	x		
Tertiary	x		
Total income in last 5 years (euro)	x	x	Age group indicators, female indicator
Wealth transfers in last 5 years:			
0–1 indicator	x		
Received amount (euro)	x		
Remittances in last 5 years:			
0–1 indicator	x		
Remitted amount (euro)	x		
Share of gross wealth:			
Real estate	x	x	
Financial assets	x	x	
Business assets	x	x	
Other assets	x	x	
Liabilities	x	x	
Willingness to take financial risks (0–10)	x		
Household size (0–1):			
1 person	x		
2 persons	x		
3 persons	x		
4 persons	x		
5 and more persons	x		
Change of HH size in last 5 y. (persons)	x		Age group indicators, female indicator

4.7.2 Bandwidths for Kernel Density Estimation

RIF regressions require estimating the probability density of the savings distribution. For this, I use a kernel density estimator, which requires specifying a bandwidth. For the tails of the distribution, I use Silverman's rule-of-thumb bandwidth. For the center of the distribution, I use fractions of the rule-of-thumb bandwidth to avoid over-smoothing. Over-smoothing occurs because there is a substantial heaping of observations at the center of the distribution. The effects of it are depicted in Figure 4.11, which shows estimated densities around the 60th percentile for different bandwidths. From the picture it is clear that Silverman's rule-of-thumb bandwidth over-smoothes the substantial spike of observations around the 45th percentile leading to densities that are too low at the 45th percentile and too high at the 60th percentile. Therefore, for each percentile, I use a specific fraction of the rule-of-thumb bandwidth that, first, gives a similar density to an even smaller fraction and, second, gives densities that are smooth and do not wobble around the percentile of interest. All fractions of the estimation are summarized in Table 4.9.

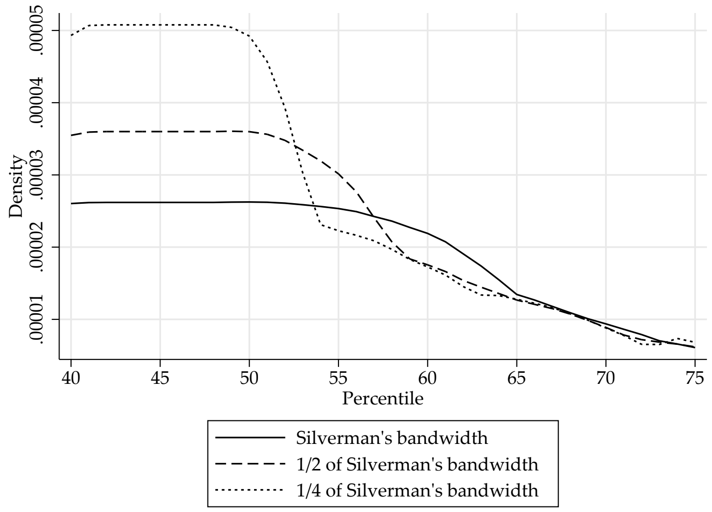


Figure 4.11: Kernel density plot of native savings distribution using different bandwidths, 2002–2007
Source: SOEP v35, own calculations.

Table 4.9: Fractions of Silverman's rule-of-thumb bandwidth used in estimation.

Quantile	Fraction of Silverman's bandwidth	Percentile	Fraction of Silverman's bandwidth
5-20	1	50	1/32
25	1/2	55	1/4
30	1/2	60	1/2
35	1/4	65	1/2
40	1/32	70	1/2
45	1/4096	75-95	1

Note: The estimation of kernel densities at various percentiles uses bandwidths that are a fraction of Silverman's rule-of-thumb bandwidth.

4.7.3 Native-migrant Wealth Gap in the Cross-Sectional Samples

Table 4.10: Native-migrant wealth gaps in Germany, 2002 to 2017.

Net wealth (euros)	2002	2007	2012	2017
Median, imputation implicate a				
Natives	13,099.0	10,234.0	9,417.0	11,758.7
Migrants	0.0	27.9	1,712.2	0.0
Wealth gap	13,099.0	10,206.1	7,704.8	11,758.7
95% upper bound of wealth gap	15,400.0	12,092.9	10,273.1	13,380.6
95% lower bound of wealth gap	10,600.0	8,373.3	5,992.6	9,636.5
Mean, imputation implicate a				
Natives	76,103.3	65,118.1	60,232.1	73,444.2
Migrants	30,324.5	28,747.2	34,705.5	43,033.1
Wealth gap	45,778.8	36,370.9	25,526.6	30,411.1
95% upper bound of wealth gap	52,891.4	44,237.3	32,771.3	41,997.5
95% lower bound of wealth gap	39,213.7	29,228.7	17,208.9	17,056.9
Median, imputation implicate b				
Natives	13,000.0	10,234.0	9,588.2	12,042.5
Migrants	0.0	27.9	1,712.2	0.0
Wealth gap	13,000.0	10,206.1	7,876.1	12,042.5
95% upper bound of wealth gap	15,200.0	12,094.7	10,444.3	13,737.4
95% lower bound of wealth gap	10,680.0	8,466.3	5,949.8	9,731.3
Mean, imputation implicate b				
Natives	75,023.9	67,782.4	59,197.3	72,650.0
Migrants	29,658.3	29,970.1	36,204.7	42,286.6

Continued on next page

Net wealth (euros)	2002	2007	2012	2017
Wealth gap	45,365.5	37,812.3	22,992.6	30,363.4
95% upper bound of wealth gap	51,318.5	47,506.9	31,467.0	40,636.8
95% lower bound of wealth gap	38,558.3	27,548.0	13,215.5	18,315.6
Median, imputation implicate c				
Natives	13,982.0	10,699.2	9,716.6	11,792.8
Migrants	0.0	93.0	1,712.2	0.0
Wealth gap	13,982.0	10,606.2	8,004.5	11,792.8
95% upper bound of wealth gap	15,100.0	12,839.0	10,701.2	13,786.1
95% lower bound of wealth gap	11,000.0	8,512.8	5,992.6	9,731.3
Mean, imputation implicate c				
Natives	74,128.8	65,627.8	60,673.6	73,013.5
Migrants	29,456.7	30,645.4	36,003.7	39,297.0
Wealth gap	44,672.1	34,982.5	24,669.9	33,716.5
95% upper bound of wealth gap	50,388.2	44,002.6	32,537.2	43,874.8
95% lower bound of wealth gap	38,118.1	24,502.9	14,753.6	21,422.6
Median, imputation implicate d				
Natives	13,500.0	10,699.2	9,713.2	11,353.2
Migrants	0.0	32.6	1,712.2	0.0
Wealth gap	13,500.0	10,666.6	8,001.0	11,353.2
95% upper bound of wealth gap	15,000.0	12,513.4	10,615.5	12,975.1
95% lower bound of wealth gap	10,500.0	8,559.4	6,463.5	9,325.9
Mean, imputation implicate d				
Natives	74,588.9	66,649.4	59,923.1	72,884.4
Migrants	29,868.4	32,005.9	35,689.1	39,272.8
Wealth gap	44,720.6	34,643.5	24,234.0	33,611.7
95% upper bound of wealth gap	51,067.1	46,043.0	31,198.3	43,998.0
95% lower bound of wealth gap	38,269.8	18,232.9	15,224.5	21,221.3
Median, imputation implicate e				
Natives	13,000.0	10,699.2	9,845.1	12,002.0
Migrants	0.0	27.9	1,712.2	0.0
Wealth gap	13,000.0	10,671.3	8,132.9	12,002.0
95% upper bound of wealth gap	15,000.0	13,025.1	10,529.9	13,380.6
95% lower bound of wealth gap	10,500.0	8,838.5	6,420.7	9,731.3
Mean, imputation implicate e				
Natives	75,656.1	65,999.0	59,742.5	72,520.4
Migrants	31,497.9	28,723.5	34,421.3	40,815.8
Wealth gap	44,158.2	37,275.6	25,321.2	31,704.6
95% upper bound of wealth gap	51,614.7	46,002.0	32,673.7	42,268.9
95% lower bound of wealth gap	36,761.9	29,667.6	16,629.9	20,119.8

Note: Representative sample of the population in Germany between 17 and 60 years. Net wealth in euros of 2002, at the individual level, and top- and bottom-coded at the 0.1 and 99.9th percentile. All calculations weighted with cross-sectional weights. Confidence intervals based on clustered bootstrap standard errors. Source: SOEP v35, own calculations.

4.7.4 Detailed RIF Decomposition Effects, 2002–2007

Table 4.11: Detailed RIF composition effects, 2002–2007.

	Percentile				
	10th	25th	50th	75th	90th
Estimated gap	−32,258.116***	−6,477.923***	538.496	10,237.927***	27,229.149***
	(6,402.27)	(1,851.70)	(350.38)	(2,409.09)	(5,979.63)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
Age	354.180	247.522	38.784	−184.477	−234.150
	(383.35)	(275.40)	(76.47)	(211.02)	(707.99)
	[1.1]	[3.8]	[7.2]	[−1.8]	[−0.9]
Female	5.211	−56.992	−58.989	−171.574	−217.046
	(189.18)	(80.97)	(47.85)	(134.56)	(314.46)
	[0.0]	[−0.9]	[−11.0]	[−1.7]	[−0.8]
Income	−2,373.685*	268.087	676.177**	3,735.757***	12,518.822***
	(1,377.96)	(321.47)	(266.28)	(723.58)	(2,769.98)
	[−7.4]	[4.1]	[125.6]	[36.5]	[46.0]
Risk attitude	−334.933	−230.499	15.421	131.865	208.792
	(454.64)	(169.36)	(43.91)	(187.60)	(592.40)
	[−1.0]	[−3.6]	[2.9]	[1.3]	[0.8]
Education	−240.364	−123.640	292.196	383.384	2,661.188
	(965.95)	(516.10)	(181.57)	(628.65)	(1,936.96)
	[−0.7]	[−1.9]	[54.3]	[3.7]	[9.8]
Wealth transfers	1,142.603*	615.206***	308.364**	1,471.562***	5,075.917***
	(583.12)	(203.48)	(130.20)	(398.43)	(1,461.58)
	[3.5]	[9.5]	[57.3]	[14.4]	[18.6]
Remittances	422.586	211.817	−56.581	317.715	1,015.700
	(738.40)	(274.25)	(84.03)	(353.00)	(995.80)
	[1.3]	[3.3]	[−10.5]	[3.1]	[3.7]
Household size	1,388.275	−325.758	−66.822	−974.138	−5,371.650**
	(1,568.32)	(476.23)	(140.02)	(669.70)	(2,219.44)
	[4.3]	[−5.0]	[−12.4]	[−9.5]	[−19.7]
Share of real estate	−11,693.062***	−5,127.094***	−342.756**	1,622.640***	3,542.786***
	(3,244.18)	(1,391.17)	(150.12)	(513.80)	(1,343.31)
	[−36.2]	[−79.1]	[−63.7]	[15.8]	[13.0]
Share of other assets	−763.559	−1,384.238**	243.715*	1,481.205***	3,159.455**
	(1,613.44)	(669.00)	(145.96)	(445.16)	(1,393.53)
	[−2.4]	[−21.4]	[45.3]	[14.5]	[11.6]
Share of liabilities	−263.761	−97.044	−53.342	−81.892	−11.657
	(1,177.11)	(427.87)	(226.55)	(356.08)	(211.05)
	[−0.8]	[−1.5]	[−9.9]	[−0.8]	[0.0]
Observations	11445	11445	11445	11445	11445

Note: Estimated gap defined as native savings less migrant savings. Based on panel sample as defined in Section 4.2. All statistics weighted with longitudinal weights. Clustered bootstrap standard errors in parentheses. Percentage share of estimated gap in square brackets. * significant at 10%, ** significant at 5%, *** significant at 1%. Source: SOEP v35, own calculations.

Table 4.12: Detailed RIF coefficient effects, 2002–2007.

	Percentile				
	10th	25th	50th	75th	90th
Estimated gap	−32,258.116***	−6,477.923***	538.496	10,237.927***	27,229.149***
	(6,402.27)	(1,851.70)	(350.38)	(2,409.09)	(5,979.63)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
Age	4,526.831	−3,945.333	−9.062	12,518.551*	3,472.156
	(11,250.18)	(4,841.20)	(803.84)	(7,126.37)	(12,159.22)
	[14.0]	[−60.9]	[−1.7]	[122.3]	[12.8]
Female	−7,725.350	1,413.785	53.942	2,605.098	6,662.485
	(5,690.23)	(1,239.82)	(196.06)	(2,098.25)	(5,954.81)
	[−23.9]	[21.8]	[10.0]	[25.4]	[24.5]
Income	−3,689.068	−507.282	3.596	−1,190.002	4,505.964
	(8,291.65)	(1,552.73)	(362.41)	(2,932.39)	(11,310.61)
	[−11.4]	[−7.8]	[0.7]	[−11.6]	[16.5]
Risk attitude	4,532.686	143.880	−19.664	291.120	4,219.571
	(6,807.85)	(993.26)	(172.90)	(1,791.45)	(5,609.79)
	[14.1]	[2.2]	[−3.7]	[2.8]	[15.5]
Education	−6,954.735	−271.795	−386.061	−4,711.670	17,919.415
	(10,921.75)	(5,543.35)	(1,733.05)	(7,701.06)	(12,947.17)
	[−21.6]	[−4.2]	[−71.7]	[−46.0]	[65.8]
Wealth transfers	−959.047	−184.912	6.147	863.780	2,034.730
	(1,382.51)	(359.81)	(69.65)	(603.98)	(1,768.59)
	[−3.0]	[−2.9]	[1.1]	[8.4]	[7.5]
Remittances	−6,857.132	−24.005	6.330	1,584.363	−1,444.752
	(6,084.29)	(912.05)	(134.53)	(1,480.01)	(5,491.52)
	[−21.3]	[−0.4]	[1.2]	[15.5]	[−5.3]
Household size	3,157.033	5,530.524	108.378	695.713	11,145.155
	(15,958.37)	(3,542.12)	(350.49)	(6,140.00)	(21,849.26)
	[9.8]	[85.4]	[20.1]	[6.8]	[40.9]
Share of real estate	−16,769.916	−3,037.163	−32.506	−1,625.094	−1,931.727
	(11,365.25)	(2,030.26)	(132.14)	(1,879.10)	(4,081.76)
	[−52.0]	[−46.9]	[−6.0]	[−15.9]	[−7.1]
Share of other assets	−23.369	−1,767.131	33.003	−1,591.782	−3,008.281
	(4,117.17)	(2,266.60)	(69.44)	(1,659.58)	(4,995.79)
	[−0.1]	[−27.3]	[6.1]	[−15.5]	[−11.0]
Share of liabilities	4,204.453	1,052.421	51.329	−411.788	−5,032.031
	(3,047.82)	(663.24)	(561.18)	(1,305.04)	(3,349.81)
	[13.0]	[16.2]	[9.5]	[−4.0]	[−18.5]
Constant	11,546.658	415.241	177.667	−5,878.739	−33,032.209
	(25,692.51)	(6,209.69)	(1,914.30)	(10,720.33)	(33,661.21)
	[35.8]	[6.4]	[33.0]	[−57.4]	[−121.3]
Observations	11445	11445	11445	11445	11445

Note: Estimated gap defined as native savings less migrant savings. Based on panel sample as defined in Section 4.2. All statistics weighted with longitudinal weights. Clustered bootstrap standard errors in parentheses. Percentage share of estimated gap in square brackets. * significant at 10%, ** significant at 5%, *** significant at 1%. Source: SOEP v35, own calculations.

4.7.5 Decomposition Results for 2012–2017

Table 4.13: Distribution of individual net wealth (in 1000 euros), 2012–2017.

		Percentiles				
	Mean	10^{th}	25^{th}	50^{th}	75^{th}	90^{th}
			2012:			
Natives	62.3	0.0	0.0	12.0	71.1	162.7
Migrants	33.1	−6.0	0.0	1.7	36.8	101.1
Gap	29.2***	6.0	0.0	10.3***	34.3***	61.6***
			2017:			
Natives	82.0	0.0	0.1	20.3	100.9	206.0
Migrants	60.3	0.0	0.0	6.5	61.8	129.8
Gap	21.7**	0.0	0.1	13.8***	39.1***	76.2***

Note: Panel sample 2012–2017 as defined in Section 4.2. All calculations weighted with longitudinal weights. Significance stars based on clustered bootstrap standard errors. * significant at 10%, ** significant at 5%, *** significant at 1%. Source: SOEP v35, own calculations.

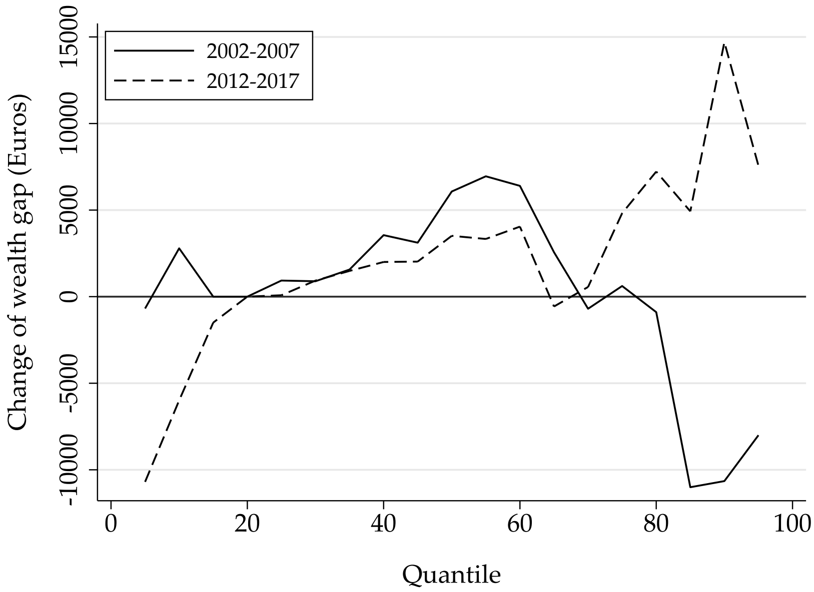


Figure 4.12: Five-year change of native-migrant wealth gap, 2002–2007 versus 2012–2017.

Note: The graph shows the native-migrant wealth gap in 2007 (2017) minus the native-migrant wealth gap in 2002 (2012) within the longitudinal samples as defined in Section 4.2. The native-migrant wealth gap is defined as net wealth of natives minus net wealth of migrants. For example the figure shows that between 2012 and 2017 the native-migrant wealth gap between the 90th quantile of natives and the 90th quantile of migrants grew by about 15,000 euros. All calculations weighted with longitudinal weights. Source: SOEP v35, own calculations.

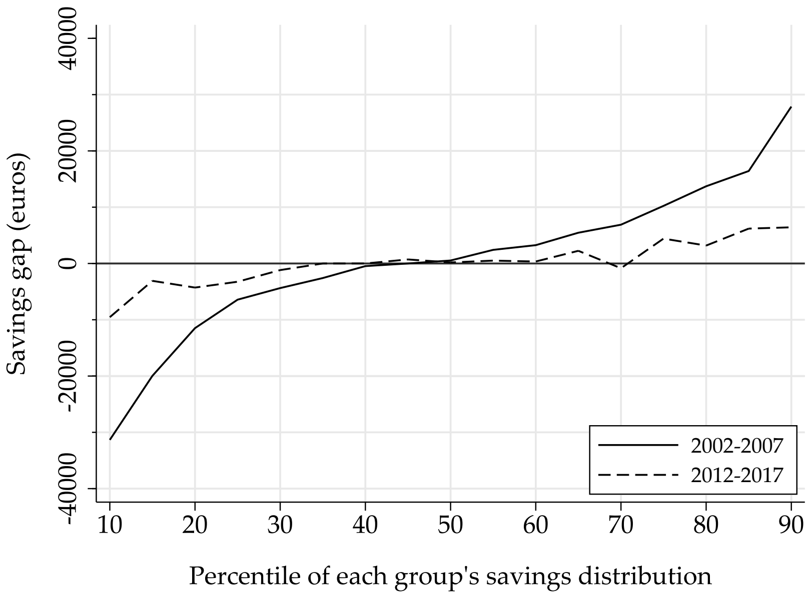


Figure 4.13: Comparison of savings gaps 2002–2007 versus 2012–2017.

Note: The savings gap is defined as native savings less migrant savings. Savings over five years on the individual level. Based on panel sample as defined in Section 4.2. All calculations weighted with longitudinal weights. Source: SOEP v35, own calculations.

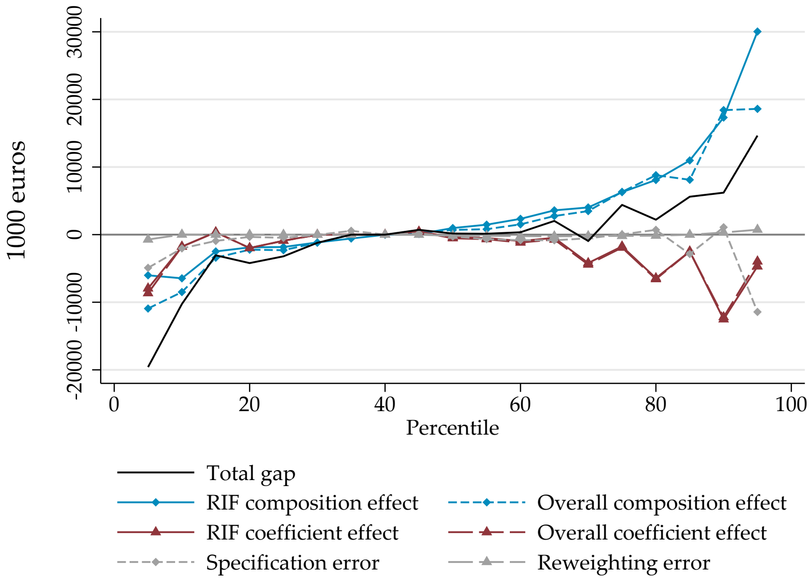


Figure 4.14: Decomposition effects, 2012–2017

Note: Panel sample 2012–2017 as defined in Section 4.2. All calculations weighted with longitudinal weights. Source: SOEP v35, own calculations.

Table 4.14: Average characteristics of weighted panel sample 2012–2017.

	Mean		Difference	
	Natives	Migrants	2012	2002
Age group (%):				
Age less than 25	11.27	7.39	3.88**	3.23*
Age 25–34	20.09	22.35	−2.26	−1.39
Age 35–44	22.38	29.78	−7.39**	1.24
Age 45–54	30.94	25.84	5.10*	−1.57
Age 55–60	15.32	14.65	0.67	−1.51
Female (%)	51.06	57.74	−6.69***	−3.00
Highest educational degree (%):				
No degree	2.78	2.50	0.28	−0.90
Compulsory	9.07	20.58	−11.51***	−15.22***
Secondary II	51.75	38.44	13.30***	18.77***
Tertiary	26.52	35.08	−8.57**	−2.65
Total income in last 5 years (euro)	132614.73	106,785.47	25,829.26***	30,176.03***
Wealth transfers in last 5 years:				
Share (%)	18.02	9.61	8.40***	6.33***
Received amount (euro)	24055.43	17,775.46	6,279.97	7,487.49
Remittances in last 5 years:				
Share (%)	29.95	36.10	−6.15*	−11.50***
Remitted amount (euro)	7005.07	3,601.94	3,403.14***	1,854.88***
Share of gross wealth (%):				
Real estate	29.36	29.75	−0.38	9.77***
Financial assets	17.10	6.54	10.55***	7.16***
Business assets	1.75	2.10	−0.35	0.51
Other assets	28.54	22.03	6.50***	2.47
Liabilities	21.86	31.02	−9.16***	0.07
Willingness to take financial risks (0–10)	2.52	2.21	0.31*	0.39***
Household size (%):				
1 person	22.76	13.79	8.97***	10.65***
2 persons	33.23	21.43	11.80***	5.55**
3 persons	21.32	27.90	−6.58**	−1.45
4 persons	16.37	26.98	−10.61***	−6.59***
5 and more persons	6.32	9.89	−3.58**	−8.16***
Change of HH size in last five y. (persons)	-0.11	−0.06	−0.05	−0.04
Observations	9042	947	9989	11445

Note: The last column shows the difference between natives and migrants in 2002, as reported in Table 4.3. Received amount of wealth transfers and remittances conditional on positive amount. Based on panel samples as defined in Section 4.2. All calculations weighted with longitudinal weights. Significance stars based on clustered bootstrap standard errors. * significant at 10%, ** significant at 5%, *** significant at 1%. Source: SOEP v35, own calculations.

Table 4.15: Detailed RIF composition effects, 2012–2017.

	Percentile				
	10th	25th	50th	75th	90th
Estimated gap	−10,259.531*	−3,205.178***	164.578	4,402.967	6,210.026
	(5,504.79)	(873.69)	(1,583.84)	(5,535.00)	(20,086.96)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
Age	411.733	−9.916	−179.146	−303.737	−776.786
	(913.43)	(138.52)	(184.35)	(441.00)	(1,161.95)
	[4.0]	[−0.3]	[−108.9]	[−6.9]	[−12.5]
Female	−151.352	22.751	7.594	−637.842**	−2,225.061**
	(394.71)	(71.21)	(61.97)	(323.99)	(1,009.84)
	[−1.5]	[0.7]	[4.6]	[−14.5]	[−35.8]
Income	−1,238.373	186.562	690.725***	4,807.565***	11,483.027***
	(1,051.52)	(126.39)	(265.58)	(1,148.29)	(2,753.12)
	[−12.1]	[5.8]	[419.7]	[109.2]	[184.9]
Risk attitude	730.077	102.426	131.209	798.413	2,219.409
	(539.18)	(81.95)	(92.06)	(487.77)	(1,349.91)
	[7.1]	[3.2]	[79.7]	[18.1]	[35.7]
Education	−1,886.533*	−196.808	343.102	450.105	214.164
	(1,093.64)	(227.71)	(291.07)	(663.14)	(1,641.51)
	[−18.4]	[−6.1]	[208.5]	[10.2]	[3.4]
Wealth transfers	1,406.105*	350.432***	395.237**	2,679.214***	6,593.397**
	(721.04)	(123.73)	(171.66)	(767.96)	(2,717.77)
	[13.7]	[10.9]	[240.2]	[60.9]	[106.2]
Remittances	191.548	34.111	−47.945	177.533	1,056.140
	(551.93)	(90.90)	(79.70)	(326.99)	(1,049.87)
	[1.9]	[1.1]	[−29.1]	[4.0]	[17.0]
Household size	546.004	97.216	−145.219	−1,487.633*	−561.565
	(1,040.77)	(209.98)	(199.54)	(820.80)	(2,310.05)
	[5.3]	[3.0]	[−88.2]	[−33.8]	[−9.0]
Share of real estate	−71.887	−10.804	7.946	48.504	50.668
	(2,221.69)	(316.57)	(277.00)	(1,452.17)	(1,774.62)
	[−0.7]	[−0.3]	[4.8]	[1.1]	[0.8]
Share of other assets	−2,462.840*	−1,913.858***	790.801**	611.930	−919.339
	(1,457.08)	(375.67)	(348.13)	(613.35)	(1,772.19)
	[−24.0]	[−59.7]	[480.5]	[13.9]	[−14.8]
Share of liabilities	−3,933.634***	−490.953**	−1,019.918**	−855.844	192.897
	(1,437.82)	(219.42)	(511.02)	(556.70)	(970.63)
	[−38.3]	[−15.3]	[−619.7]	[−19.4]	[3.1]
Observations	9989	9989	9989	9989	9989

Note: Estimated gap defined as native savings less migrant savings. Based on panel sample 2012–2017 as defined in Section 4.2. All statistics weighted with longitudinal weights. Clustered bootstrap standard errors in parentheses. Percentage share of estimated gap in square brackets. * significant at 10%, ** significant at 5%, *** significant at 1%. Source: SOEP v35, own calculations.

Table 4.16: Detailed RIF coefficient effects, 2012–2017.

	Percentile				
	10th	25th	50th	75th	90th
Estimated gap	−10,259.531*	−3,205.178***	164.578	4,402.967	6,210.026
	(5,504.79)	(873.69)	(1,583.84)	(5,535.00)	(20,086.96)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
Age	4,313.576	−634.938	−924.196	−12,783.623	−27,859.161
	(12,991.94)	(2,876.05)	(7,350.88)	(17,152.28)	(62,116.61)
	[42.0]	[−19.8]	[−561.6]	[−290.3]	[−448.6]
Female	−1,594.525	−461.799	−321.741	−193.142	−9,276.002
	(5,510.61)	(848.50)	(1,844.78)	(5,222.22)	(21,408.70)
	[−15.5]	[−14.4]	[−195.5]	[−4.4]	[−149.4]
Income	1,236.572	303.609	1,556.124	3,971.432	−49,858.454
	(9,819.35)	(747.68)	(2,672.63)	(8,057.82)	(64,418.49)
	[12.1]	[9.5]	[945.5]	[90.2]	[−802.9]
Risk attitude	−687.943	286.635	−167.193	−2,385.537	−13,974.957
	(5,211.21)	(623.91)	(1,551.84)	(5,538.56)	(36,417.97)
	[−6.7]	[8.9]	[−101.6]	[−54.2]	[−225.0]
Education	640.092	−1,248.968	−2,200.619	2,123.365	2,119.096
	(17,154.71)	(1,997.40)	(7,948.43)	(17,143.45)	(75,845.26)
	[6.2]	[−39.0]	[−1,337.1]	[48.2]	[34.1]
Wealth transfers	681.697	−96.514	134.361	1,077.388	−2,680.372
	(1,110.57)	(160.08)	(649.29)	(1,296.65)	(6,133.90)
	[6.6]	[−3.0]	[81.6]	[24.5]	[−43.2]
Remittances	−2,759.365	122.935	889.441	1,621.295	16,570.884
	(5,665.22)	(506.68)	(1,117.57)	(3,185.27)	(24,235.03)
	[−26.9]	[3.8]	[540.4]	[36.8]	[266.8]
Household size	−18,515.376	−844.003	1,125.460	6,313.950	−52,039.428
	(27,379.07)	(2,008.98)	(5,922.39)	(12,567.29)	(54,238.89)
	[−180.5]	[−26.3]	[683.8]	[143.4]	[−838.0]
Share of real estate	−8,415.513	−945.232	1,221.409	−118.006	−2,177.209
	(7,347.05)	(729.57)	(3,555.86)	(7,425.98)	(30,101.43)
	[−82.0]	[−29.5]	[742.1]	[−2.7]	[−35.1]
Share of other assets	80.246	−118.289	672.583	−861.149	−117.315
	(5,832.28)	(673.26)	(2,240.93)	(2,868.46)	(9,628.02)
	[0.8]	[−3.7]	[408.7]	[−19.6]	[−1.9]
Share of liabilities	1,658.279	157.385	1,325.851	891.815	3,298.502
	(4,855.11)	(664.17)	(6,915.80)	(4,124.05)	(12,420.24)
	[16.2]	[4.9]	[805.6]	[20.3]	[53.1]
Constant	21,579.200	2,578.626	−3,690.599	−1,396.982	123,473.081
	(28,594.56)	(3,757.68)	(23,055.77)	(25,337.27)	(185,486.60)
	[210.3]	[80.5]	[−2,242.5]	[−31.7]	[1,988.3]
Observations	9989	9989	9989	9989	9989

Note: Estimated gap defined as native savings less migrant savings. Based on panel sample 2012–2017 as defined in Section 4.2. All statistics weighted with longitudinal weights. Clustered bootstrap standard errors in parentheses. Percentage share of estimated gap in square brackets. * significant at 10%, ** significant at 5%, *** significant at 1%. Source: SOEP v35, own calculations.

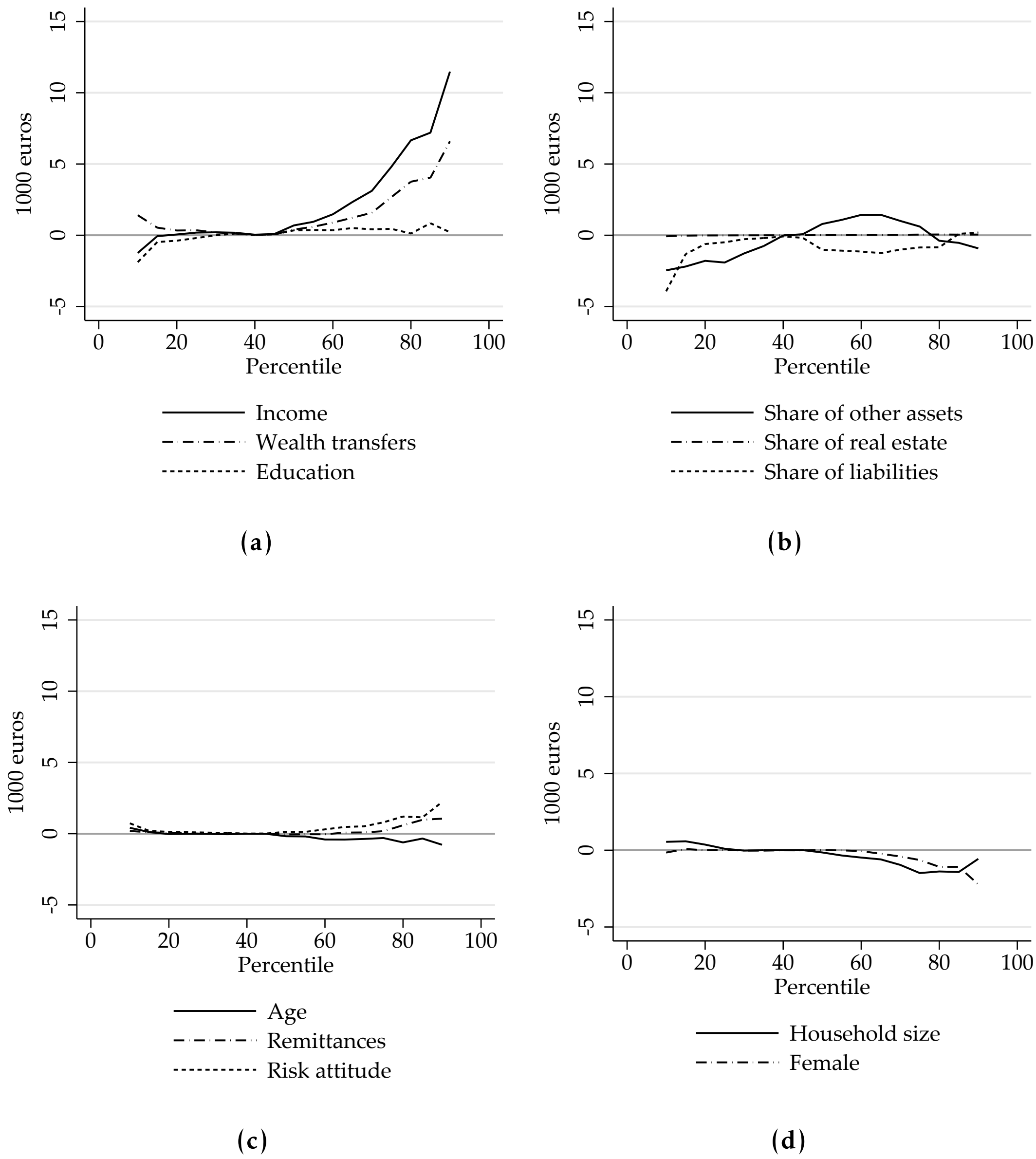


Figure 4.15: Detailed RIF composition effects, 2012–2017.

Note: Panel sample 2012–2017 as defined in Section 4.2. All calculations weighted with longitudinal weights. Source: SOEP v35, own calculations.

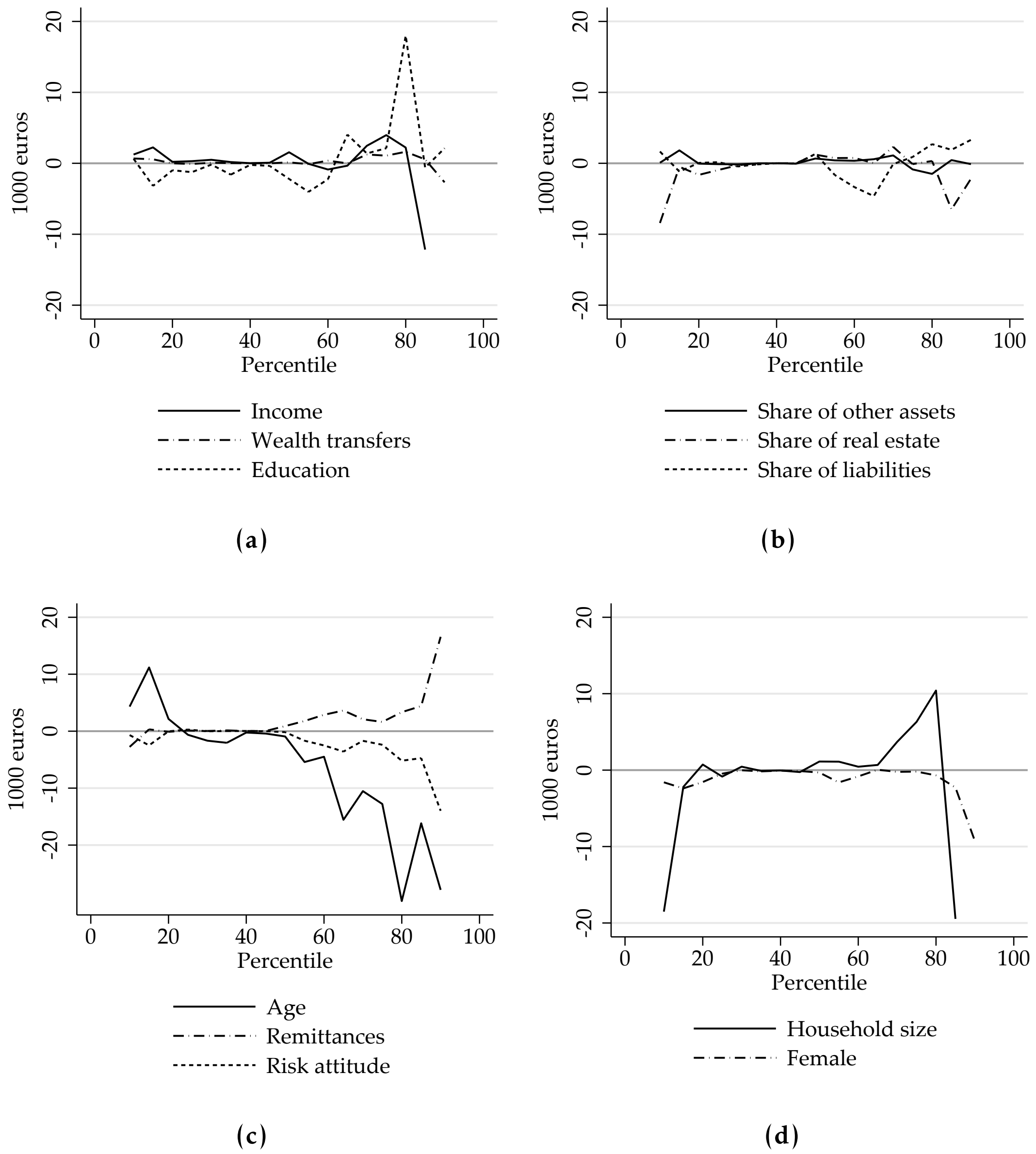


Figure 4.16: Detailed RIF coefficient effects, 2012–2017.

Note: Panel sample 2012–2017 as defined in Section 4.2. All calculations weighted with longitudinal weights. Source: SOEP v35, own calculations.

4.7.6 Robustness: Alternative Specifications for Income

Table 4.17: RIF decomposition results for different income specifications, 2002–2007.

	Percentile				
Income specification	10th	25th	50th	75th	90th
RIF composition effect:					
- Linear	−12,356.510***	−6,002.633***	996.167*	7,732.047***	22,348.156***
	(4,351.75)	(1,763.02)	(549.37)	(1,867.30)	(5,157.40)
	[−38.3]	[−92.7]	[185.0]	[75.5]	[82.1]
- Linear + quadratic	−12,067.413***	−6,008.167***	1,084.866*	7,924.346***	21,892.423***
	(4,345.25)	(1,774.67)	(576.07)	(1,878.15)	(5,134.10)
	[−37.4]	[−92.7]	[201.4]	[77.4]	[80.4]
- Linear + quadratic + cubic	−12,063.455***	−6,007.973***	1,083.780*	7,928.136***	21,913.653***
	(4,347.88)	(1,775.34)	(577.80)	(1,878.23)	(5,136.32)
	[−37.4]	[−92.7]	[201.9]	[77.4]	[80.5]
- Linear + logarithmic	−12,236.809***	−5,981.094***	1,041.745*	7,724.423***	21,729.805***
	(4,348.61)	(1,770.76)	(564.64)	(1,869.41)	(5,089.06)
	[−38.0]	[−92.6]	[194.2]	[75.8]	[80.0]
- Income of each year	−12,186.370***	−5,965.845***	1,042.375*	7,933.828***	22,572.277***
	(4,321.46)	(1,764.62)	(562.13)	(1,885.31)	(5,173.50)
	[−37.8]	[−92.1]	[194.0]	[77.5]	[82.9]
RIF coefficient effect:					
- Linear	−15,010.957**	−1,181.771	−6.901	3,149.550	5,510.475
	(6,188.07)	(1,447.97)	(141.10)	(2,623.10)	(5,920.33)
	[−46.5]	[−18.2]	[−1.3]	[30.8]	[20.2]
- Linear + quadratic	−15,024.340**	−1,181.069	−7.144	3,144.407	5,528.946
	(6,194.88)	(1,448.08)	(140.94)	(2,623.80)	(5,919.06)
	[−46.6]	[−18.2]	[−1.3]	[30.7]	[20.3]
- Linear + quadratic + cubic	−15,027.141**	−1,181.777	−8.756	3,139.880	5,510.898
	(6,188.59)	(1,447.34)	(140.24)	(2,629.79)	(5,972.45)
	[−46.6]	[−18.2]	[−1.6]	[30.7]	[20.2]
- Linear + logarithmic	−15,133.661**	−1,216.456	−3.335	3,354.023	5,756.520
	(6,248.03)	(1,451.57)	(147.28)	(2,648.29)	(6,010.38)
	[−47.0]	[−18.8]	[−0.6]	[32.9]	[21.2]
- Income of each year	−14,798.642**	−1,104.245	−6.003	3,292.110	5,809.991
	(6,188.22)	(1,449.92)	(145.77)	(2,692.76)	(5,914.25)
	[−45.9]	[−17.0]	[−1.1]	[32.2]	[21.3]
Specification error:					
- Linear	−3,974.776	957.288	−457.574	−959.622	−1,260.819
	(3,060.90)	(882.75)	(403.06)	(1,209.60)	(4,766.51)
	[−12.3]	[14.8]	[−85.0]	[−9.4]	[−4.6]
- Linear + quadratic	−4,263.872	962.822	−546.507	−1,151.924	−805.086
	(3,035.87)	(891.33)	(425.43)	(1,222.02)	(4,700.75)
	[−13.2]	[14.9]	[−101.5]	[−11.3]	[−3.0]
- Linear + quadratic + cubic	−4,267.830	962.628	−545.237	−1,155.713	−826.317
	(3,034.48)	(892.83)	(428.04)	(1,213.48)	(4,687.17)
	[−13.2]	[14.9]	[−101.6]	[−11.3]	[−3.0]
- Linear + logarithmic	−3,900.324	986.655	−508.375	−1,139.703	−729.586
	(3,030.41)	(891.03)	(415.73)	(1,210.28)	(4,625.13)
	[−12.1]	[15.3]	[−94.7]	[−11.2]	[−2.7]
- Income of each year	−4,144.915	920.500	−504.080	−1,161.404	−1,484.940
	(3,037.79)	(883.60)	(412.18)	(1,207.24)	(4,769.13)
	[−12.8]	[14.2]	[−93.8]	[−11.3]	[−5.5]
Income composition effect:					
- Linear	−2,373.685*	268.087	676.177**	3,735.757***	12,518.822***
	(1,377.96)	(321.47)	(266.28)	(723.58)	(2,769.98)
	[−7.4]	[4.1]	[125.6]	[36.5]	[46.0]
- Linear + quadratic	−1,493.312	251.234	946.287**	4,321.358***	11,130.997***

Continued on next page

	Percentile				
	10th	25th	50th	75th	90th
	(1,404.89)	(401.70)	(378.03)	(829.12)	(2,707.73)
	[−4.6]	[3.9]	[175.7]	[42.2]	[40.9]
- Linear + quadratic + cubic	−1,616.889	245.161	980.205**	4,203.030***	10,468.156***
	(1,443.65)	(398.87)	(390.75)	(808.99)	(2,587.84)
	[−5.0]	[3.8]	[182.6]	[41.1]	[38.4]
- Linear + logarithmic	−2,214.142	306.478	833.986**	3,850.399***	11,028.633***
	(1,509.43)	(386.67)	(333.28)	(745.84)	(2,616.80)
	[−6.9]	[4.7]	[155.4]	[37.8]	[40.6]
- Income of each year	−2,065.308	361.911	790.140**	4,224.267***	13,450.415***
	(1,374.03)	(360.50)	(308.75)	(800.57)	(2,947.39)
	[−6.4]	[5.6]	[147.1]	[41.3]	[49.4]
Income coefficient effect:					
- Linear	−3,689.068	−507.282	3.596	−1,190.002	4,505.964
	(8,291.65)	(1,552.73)	(362.41)	(2,932.39)	(11,310.61)
	[−11.4]	[−7.8]	[0.7]	[−11.6]	[16.5]
- Linear + quadratic	−15,308.417	−1,501.543	24.201	−3,935.718	−4,112.954
	(11,829.74)	(2,313.37)	(684.66)	(5,002.62)	(14,429.93)
	[−47.5]	[−23.2]	[4.5]	[−38.4]	[−15.1]
- Linear + quadratic + cubic	−19,522.193	−375.947	7.339	−5,899.929	−12,974.857
	(14,562.65)	(2,840.76)	(624.54)	(5,642.54)	(15,997.64)
	[−60.5]	[−5.8]	[1.4]	[−57.6]	[−47.7]
- Linear + logarithmic	−93,513.146	8,463.536	158.899	−34,790.582	−58,468.777
	(86,813.25)	(14,918.43)	(1,877.22)	(27,469.45)	(91,980.37)
	[−290.6]	[131.1]	[29.6]	[−341.3]	[−215.4]
- Income of each year	−2,567.084	−323.286	11.295	−2,071.038	4,472.337
	(8,106.05)	(1,665.26)	(434.18)	(3,458.00)	(12,283.09)
	[−8.0]	[−5.0]	[2.1]	[−20.2]	[16.4]
Income specification error:					
- Linear	−1,388.008	−195.673	2,132.782***	4,718.159***	15,477.029*
	(4,352.60)	(998.97)	(825.63)	(1,763.58)	(9,119.37)
	[−4.3]	[−3.0]	[396.1]	[46.1]	[56.8]
- Linear + quadratic	1,041.892	−116.516	3,951.753***	7,393.646***	10,905.963
	(5,749.59)	(1,661.88)	(1,529.02)	(2,390.20)	(9,782.71)
	[3.2]	[−1.8]	[733.7]	[72.2]	[40.1]
- Linear + quadratic + cubic	−316.084	189.810	4,837.014***	7,299.558**	5,599.856
	(7,539.69)	(2,342.63)	(1,819.21)	(3,051.09)	(11,031.80)
	[−1.0]	[2.9]	[901.0]	[71.3]	[20.6]
- Linear + logarithmic	13,264.257	4,553.818	19,404.011**	42,539.563**	−40,541.559
	(55,748.05)	(17,414.33)	(7,841.72)	(19,796.32)	(76,917.73)
	[41.2]	[70.5]	[3,616.4]	[417.3]	[−149.3]
- Income of each year	−2,347.443	−286.368	2,408.376**	5,512.291***	16,334.662*
	(4,399.16)	(1,076.99)	(936.60)	(1,877.59)	(9,708.39)
	[−7.3]	[−4.4]	[448.2]	[53.8]	[60.0]
Adjusted R^2 native RIF regression:					
- Linear	0.1407	0.1696	0.0946	0.0933	0.0778
- Linear + quadratic	0.1411	0.1696	0.0997	0.0946	0.0783
- Linear + quadratic + cubic	0.1411	0.1695	0.1003	0.0950	0.0794
- Linear + logarithmic	0.1407	0.1694	0.0972	0.0933	0.0786
- Income of each year					
Adjusted R^2 migrant RIF regression:					
- Linear	0.1402	0.1460	0.1103	0.1715	0.1015
- Linear + quadratic	0.1490	0.1459	0.1136	0.1767	0.1012
- Linear + quadratic + cubic	0.1485	0.1474	0.1133	0.1764	0.1007
- Linear + logarithmic	0.1425	0.1454	0.1102	0.1706	0.1008
- Income of each year					
Observations	11445	11445	11445	11445	11445

Note: Based on panel sample 2002–2007 as defined in Section 4.2. All statistics weighted with longitudinal weights. Clustered bootstrap standard errors in parentheses. Percentage share of estimated gap in square brackets. * significant at 10%, ** significant at 5%, *** significant at 1%. Source: SOEP v35, own calculations.

4.7.7 Robustness: Portfolio Changes

Table 4.18: RIF decomposition of savings between 2002 and 2007, controlling for homeownership changes.

	Percentile				
	10^{th}	25^{th}	50^{th}	75^{th}	90^{th}
Observed gap	−31,338.844***	−6,429.194***	528.198	10,235.361***	27,872.145***
	(6,766.12)	(1,868.09)	(342.73)	(2,333.02)	(5,898.64)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
RIF decomposition, native coefficients					
Estimated gap	−32,258.116***	−6,477.923***	537.505	10,237.927***	27,229.149***
	(6,402.27)	(1,851.70)	(350.26)	(2,409.09)	(5,979.63)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
Overall composition effect	−18,689.330***	−5,648.954***	537.779*	5,572.187***	17,006.199***
	(5,101.28)	(1,312.54)	(316.85)	(1,952.49)	(5,204.23)
	[57.9]	[87.2]	[100.1]	[54.4]	[62.5]
RIF composition effect	−13,585.833***	−6,473.971***	795.497	6,690.395***	19,537.831***
	(4,516.14)	(1,836.54)	(558.34)	(2,159.89)	(5,812.24)
	[42.1]	[99.9]	[148.0]	[65.3]	[71.8]
Specification error	−5,103.497*	825.017	−257.718	−1,118.208	−2,531.632
	(3,080.68)	(912.66)	(371.87)	(1,156.66)	(4,342.18)
	[15.8]	[−12.7]	[−47.9]	[−10.9]	[−9.3]
Overall coefficient effect	−13,568.786**	−828.969	−0.274	4,665.740*	10,222.949*
	(5,914.58)	(1,412.77)	(226.64)	(2,553.22)	(6,206.65)
	[42.1]	[12.8]	[−0.1]	[45.6]	[37.5]
RIF coefficient effect	−12,546.558**	−600.994	−8.152	4,380.944*	9,611.666
	(5,878.76)	(1,377.87)	(189.84)	(2,450.73)	(6,150.81)
	[38.9]	[9.3]	[−1.5]	[42.8]	[35.3]
Reweighting error	−1,022.228	−227.975	7.878	284.797	611.284
	(946.76)	(185.47)	(54.16)	(372.24)	(1,107.84)
	[3.2]	[3.5]	[1.5]	[2.8]	[2.2]
Observations	11445	11445	11445	11445	11445

Note: Observed gap defined as native savings minus migrants savings. Panel sample 2002–2007 as defined in Section 4.2. All calculations weighted with longitudinal weights. Clustered bootstrap standard errors in parentheses. Effect as percentage of estimated gap in square brackets. * significant at 10%, ** significant at 5%, *** significant at 1%. Source: SOEP v35, own calculations.

Table 4.19: Detailed RIF composition effects, 2002–2007, controlling for homeownership changes.

	Percentile				
	10^{th}	25^{th}	50^{th}	75^{th}	90^{th}
Estimated gap	−32,258.115***	−6,477.923***	537.505	10,237.927***	27,229.148***
	(6,402.27)	(1,851.70)	(350.26)	(2,409.09)	(5,979.63)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
Age	398.490	272.036	51.822	−113.674	−59.388
	(438.97)	(283.51)	(82.57)	(183.87)	(762.19)
	[1.2]	[4.2]	[9.6]	[−1.1]	[−0.2]
Female	−57.189	−63.594	−53.347	−124.610	−72.551
	(186.17)	(81.18)	(43.74)	(108.98)	(270.73)
	[−0.2]	[−1.0]	[−9.9]	[−1.2]	[−0.3]
Income	−3,114.373**	−14.750	550.505**	3,062.949***	10,708.305***
	(1,482.08)	(303.07)	(220.10)	(622.77)	(2,502.92)
	[−9.7]	[−0.2]	[102.4]	[29.9]	[39.3]
Risk attitude	−299.978	−224.528	14.685	121.435	173.523
	(441.38)	(167.99)	(42.54)	(185.46)	(582.27)
	[−0.9]	[−3.5]	[2.7]	[1.2]	[0.6]
Education	−322.668	−222.880	213.015	−95.402	1,304.967
	(897.76)	(516.75)	(157.88)	(579.05)	(1,799.10)
	[−1.0]	[−3.4]	[39.6]	[−0.9]	[4.8]
Wealth transfers	842.949*	482.489***	240.469**	1,091.179***	4,032.040***
	(507.62)	(175.18)	(104.21)	(330.62)	(1,291.88)
	[2.6]	[7.4]	[44.7]	[10.7]	[14.8]
Remittances	546.206	266.308	−29.459	473.150	1,446.037
	(690.85)	(263.81)	(80.28)	(364.11)	(1,053.47)
	[1.7]	[4.1]	[−5.5]	[4.6]	[5.3]
Household size	2,237.167	3.760	85.151	−147.947	−3,137.570
	(1,564.54)	(472.55)	(133.93)	(632.47)	(2,102.69)
	[6.9]	[0.1]	[15.8]	[−1.4]	[−11.5]
Share of real estate	−8,912.687***	−4,267.119***	−58.181	2,969.504***	6,969.272***
	(2,407.91)	(1,161.09)	(92.44)	(767.70)	(1,856.83)
	[−27.6]	[−65.9]	[−10.8]	[29.0]	[25.6]
Share of other assets	−513.685	−1,408.663**	180.141	1,044.653***	1,876.270*
	(1,499.96)	(648.24)	(121.17)	(387.66)	(1,123.95)
	[−1.6]	[−21.7]	[33.5]	[10.2]	[6.9]
Share of liabilities	−275.596	−102.609	−56.503	−97.047	−50.622
	(1,212.43)	(448.12)	(238.81)	(415.78)	(302.62)
	[−0.9]	[−1.6]	[−10.5]	[−0.9]	[−0.2]
Change into homeownership	24.508	−136.563	−133.519	−848.882	−2,424.109
	(123.75)	(171.68)	(178.61)	(1,070.34)	(2,778.66)
	[0.1]	[−2.1]	[−24.8]	[−8.3]	[−8.9]
Change out of homeownership	−4,138.978**	−1,057.856**	−209.283**	−644.913***	−1,228.342**
	(2,047.31)	(442.78)	(103.65)	(238.32)	(485.89)
	[−12.8]	[−16.3]	[−38.9]	[−6.3]	[−4.5]
Observations	11445	11445	11445	11445	11445

Note: Estimated gap defined as native savings less migrant savings. Based on panel sample 2002–2007 as defined in Section 4.2. All statistics weighted with longitudinal weights. Clustered bootstrap standard errors in parentheses. Percentage share of estimated gap in square brackets. * significant at 10%, ** significant at 5%, *** significant at 1%. Source: SOEP v35, own calculations.

Table 4.20: Detailed RIF coefficient effects, 2002–2007, controlling for homeownership changes.

	Percentile				
	10^{th}	25^{th}	50^{th}	75^{th}	90^{th}
Estimated gap	−32,258.115***	−6,477.923***	537.505	10,237.927***	27,229.148***
	(6,402.27)	(1,851.70)	(350.26)	(2,409.09)	(5,979.63)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
Age	8,081.666	−2,364.007	−44.289	12,976.518*	4,210.235
	(11,231.82)	(4,775.29)	(1,242.65)	(6,905.36)	(12,404.43)
	[25.1]	[−36.5]	[−8.2]	[126.7]	[15.5]
Female	−7,611.105	1,431.237	62.786	2,462.258	2,236.564
	(5,572.86)	(1,254.89)	(348.89)	(1,974.63)	(5,976.68)
	[−23.6]	[22.1]	[11.7]	[24.1]	[8.2]
Income	−7,715.446	−734.625	14.432	−2,085.261	1,649.422
	(8,322.83)	(1,453.95)	(489.37)	(2,654.89)	(10,086.58)
	[−23.9]	[−11.3]	[2.7]	[−20.4]	[6.1]
Risk attitude	4,489.372	289.747	−17.800	312.747	4,303.402
	(6,597.12)	(962.75)	(206.44)	(1,815.04)	(5,221.16)
	[13.9]	[4.5]	[−3.3]	[3.1]	[15.8]
Education	−9,439.695	−1,422.855	−485.969	−6,650.357	6,786.733
	(11,332.57)	(5,569.15)	(1,762.18)	(7,488.42)	(12,401.36)
	[−29.3]	[−22.0]	[−90.4]	[−65.0]	[24.9]
Wealth transfers	−1,718.625	−254.357	5.336	878.524	2,275.499
	(1,452.41)	(352.99)	(106.20)	(589.49)	(1,699.32)
	[−5.3]	[−3.9]	[1.0]	[8.6]	[8.4]
Remittances	−6,586.172	98.632	8.297	1,162.079	−857.228
	(5,888.99)	(871.88)	(201.45)	(1,344.24)	(5,114.42)
	[−20.4]	[1.5]	[1.5]	[11.4]	[−3.1]
Household size	3,181.059	5,546.883	96.303	−688.721	14,087.126
	(15,694.32)	(3,445.31)	(569.01)	(5,760.60)	(19,443.10)
	[9.9]	[85.6]	[17.9]	[−6.7]	[51.7]
Share of real estate	−11,197.837	−1,937.021	−4.432	55.359	3,760.633
	(10,593.94)	(1,793.94)	(190.58)	(2,355.15)	(4,592.81)
	[−34.7]	[−29.9]	[−0.8]	[0.5]	[13.8]
Share of other assets	346.310	−841.636	54.870	−1,157.537	−3,352.760
	(4,203.20)	(2,238.26)	(118.39)	(1,645.13)	(5,002.29)
	[1.1]	[−13.0]	[10.2]	[−11.3]	[−12.3]
Share of liabilities	3,999.006	897.158	57.004	−669.145	−4,067.136
	(2,995.41)	(679.07)	(872.33)	(1,502.27)	(3,195.74)
	[12.4]	[13.8]	[10.6]	[−6.5]	[−14.9]
Change into homeownership	701.213	−94.676	20.584	2,482.471	12,325.657*
	(1,238.24)	(406.47)	(496.07)	(1,830.89)	(6,613.29)
	[2.2]	[−1.5]	[3.8]	[24.2]	[45.3]
Change out of homeownership	−3,906.761**	−272.641	−6.547	−284.279	−1,203.381
	(1,589.63)	(193.70)	(78.04)	(221.71)	(817.26)
	[−12.1]	[−4.2]	[−1.2]	[−2.8]	[−4.4]
Constant	14,830.459	−942.832	231.274	−4,413.712	−32,543.102
	(25,589.58)	(6,099.65)	(1,849.77)	(9,255.27)	(31,810.65)
	[46.0]	[−14.6]	[43.0]	[−43.1]	[−119.5]
Observations	11445	11445	11445	11445	11445

Note: Estimated gap defined as native savings less migrant savings. Based on panel sample 2002–2007 as defined in Section 4.2. All statistics weighted with longitudinal weights. Clustered bootstrap standard errors in parentheses. Percentage share of estimated gap in square brackets. * significant at 10%, ** significant at 5%, *** significant at 1%. Source: SOEP v35, own calculations.

4.7.8 Results for Five Imputation Implicates

The SOEP data distribution contains five imputation implicates for the net wealth and asset variables. The main results use the first implicate, *a*. In this section, results for the other implicates *b* to *e* are shown. Overall, the estimates vary only slightly by implicate, underlining the robustness of the main results. The observed and estimated savings gaps vary to some extent. However, the savings gaps are statistically significant in all cases. The variation in savings gaps mainly affects the statistically insignificant estimates of the coefficient effect, while composition effects are very stable. These patterns are possibly attributable to the relatively small sample of migrants, which introduces statistical uncertainty into the estimation of quantiles of the migrant savings distribution and the migrant RIF regression coefficients. In contrast, the estimation of composition effects relies more heavily on the much larger sample of native individuals, resulting in more robust estimates.

Table 4.21: RIF decomposition of savings between 2002 and 2007 for five imputation implicates *a* to *e*.

	Percentile				
	10^{th}	25^{th}	50^{th}	75^{th}	90^{th}
Observed gap:					
a	-31338.844***	-6429.194***	528.220	10235.361***	27872.145***
	(6766.12)	(1868.09)	(342.73)	(2333.02)	(5898.64)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
b	-29555.387***	-6948.431***	512.150	8254.017***	26046.156***
	(6526.72)	(1551.85)	(318.14)	(2438.68)	(5194.63)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
c	-33376.143***	-7792.123***	721.147**	7732.021***	27810.945***
	(4099.50)	(1756.09)	(325.26)	(2658.34)	(5431.60)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
d	-31952.625***	-6971.608***	465.020*	9595.319***	28108.738***
	(5904.33)	(1895.10)	(280.13)	(2416.93)	(5948.54)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
e	-26374.430***	-6615.295***	464.825	7399.829***	23619.945***
	(6026.45)	(1779.43)	(313.90)	(2582.04)	(5925.20)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
Estimated gap:					
a	-32258.116***	-6477.923***	538.496	10237.927***	27229.149***
	(6402.27)	(1851.70)	(350.38)	(2409.09)	(5979.63)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
b	-29750.717***	-6934.968***	516.726	8210.834***	25945.755***
	(6360.90)	(1540.46)	(327.78)	(2506.28)	(5305.92)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
c	-33430.156***	-7780.031***	722.240**	7728.133***	27699.075***
	(4035.83)	(1731.17)	(333.86)	(2761.06)	(5521.19)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]

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	Percentile				
	10th	25th	50th	75th	90th
d	-32551.550***	-6984.203***	481.382*	9552.403***	28070.372***
	(5623.58)	(1862.86)	(287.59)	(2485.19)	(5937.78)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
e	-26380.344***	-6643.634***	485.909	6611.994**	23159.591***
	(5851.67)	(1748.17)	(322.49)	(2626.58)	(5954.98)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
Overall composition effect:					
a	-16331.285***	-5045.345***	538.594*	6772.424***	21087.337***
	(5137.45)	(1288.13)	(308.19)	(1628.40)	(4290.83)
	[50.6]	[77.9]	[100.0]	[66.2]	[77.4]
b	-13931.431***	-4242.976***	515.716*	7095.660***	19606.744***
	(4962.26)	(1227.83)	(292.04)	(1540.28)	(4279.75)
	[46.8]	[61.2]	[99.8]	[86.4]	[75.6]
c	-17156.758***	-4764.231***	723.558**	7235.328***	19805.976***
	(4655.43)	(1276.58)	(303.73)	(1640.71)	(4230.96)
	[51.3]	[61.2]	[100.2]	[93.6]	[71.5]
d	-12664.519***	-4578.393***	481.813*	7515.222***	19972.986***
	(4664.45)	(1275.26)	(276.89)	(1494.81)	(4768.10)
	[38.9]	[65.6]	[100.1]	[78.7]	[71.2]
e	-16308.609***	-4487.523***	485.699	7458.556***	23405.270***
	(4524.12)	(1332.49)	(304.88)	(1572.57)	(4539.52)
	[61.8]	[67.5]	[100.0]	[112.8]	[101.1]
Overall coefficient effect:					
a	-15926.831***	-1432.578	-0.098	3465.503	6141.812
	(6170.06)	(1478.93)	(148.55)	(2676.78)	(5921.43)
	[49.4]	[22.1]	[-0.0]	[33.8]	[22.6]
b	-15819.286***	-2691.992*	1.010	1115.174	6339.011
	(5841.13)	(1402.93)	(166.05)	(2630.43)	(5862.57)
	[53.2]	[38.8]	[0.2]	[13.6]	[24.4]
c	-16273.398***	-3015.801**	-1.318	492.804	7893.099
	(4689.27)	(1469.95)	(155.16)	(3036.58)	(5579.32)
	[48.7]	[38.8]	[-0.2]	[6.4]	[28.5]
d	-19887.031***	-2405.811	-0.431	2037.181	8097.387
	(5300.93)	(1653.41)	(67.96)	(2829.55)	(5881.79)
	[61.1]	[34.4]	[-0.1]	[21.3]	[28.8]
e	-10071.734**	-2156.110	0.210	-846.562	-245.678
	(4983.23)	(1531.79)	(97.82)	(2560.37)	(5907.44)
	[38.2]	[32.5]	[0.0]	[-12.8]	[-1.1]
RIF composition effect:					
a	-12356.510***	-6002.633***	996.167*	7732.047***	22348.156***
	(4351.75)	(1763.02)	(549.37)	(1867.30)	(5157.40)
	[38.3]	[92.7]	[185.0]	[75.5]	[82.1]
b	-11533.858***	-4696.706***	1241.480**	8370.493***	19218.349***
	(4261.67)	(1434.46)	(574.14)	(1823.83)	(4982.48)
	[38.8]	[67.7]	[240.3]	[101.9]	[74.1]

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	Percentile				
	10th	25th	50th	75th	90th
c	-12585.947***	-5509.318***	1089.622**	8129.705***	20180.172***
	(3401.16)	(1426.57)	(529.96)	(1768.15)	(5734.51)
	[37.6]	[70.8]	[150.9]	[105.2]	[72.9]
d	-11000.391***	-6174.705***	843.729*	8751.216***	24132.274***
	(3888.69)	(1539.04)	(487.14)	(1812.74)	(5936.70)
	[33.8]	[88.4]	[175.3]	[91.6]	[86.0]
e	-13494.008***	-5613.386***	847.784	8757.171***	21789.505***
	(3746.55)	(1591.60)	(549.00)	(1871.18)	(6377.36)
	[51.2]	[84.5]	[174.5]	[132.4]	[94.1]
Specification error:					
a	-3974.776	957.288	-457.574	-959.622	-1260.819
	(3060.90)	(882.75)	(403.06)	(1209.60)	(4766.51)
	[12.3]	[-14.8]	[-85.0]	[-9.4]	[-4.6]
b	-2397.574	453.730	-725.764	-1274.833	388.396
	(3057.52)	(717.40)	(474.04)	(1206.02)	(3963.97)
	[8.1]	[-6.5]	[-140.5]	[-15.5]	[1.5]
c	-4570.811	745.087	-366.065	-894.376	-374.196
	(2903.77)	(822.27)	(420.96)	(1230.13)	(4390.38)
	[13.7]	[-9.6]	[-50.7]	[-11.6]	[-1.4]
d	-1664.128	1596.312*	-361.915	-1235.994	-4159.288
	(3116.27)	(950.75)	(337.86)	(1178.78)	(4646.47)
	[5.1]	[-22.9]	[-75.2]	[-12.9]	[-14.8]
e	-2814.602	1125.863	-362.085	-1298.614	1615.764
	(2855.93)	(856.95)	(412.62)	(1187.16)	(4256.83)
	[10.7]	[-16.9]	[-74.5]	[-19.6]	[7.0]
RIF coefficient effect:					
a	-15010.957**	-1181.771	-6.901	3149.550	5510.475
	(6188.07)	(1447.97)	(141.10)	(2623.10)	(5920.33)
	[46.5]	[18.2]	[-1.3]	[30.8]	[20.2]
b	-14699.601**	-2396.776*	-3.768	818.286	5550.639
	(5709.69)	(1361.97)	(164.06)	(2577.59)	(5955.96)
	[49.4]	[34.6]	[-0.7]	[10.0]	[21.4]
c	-15409.707***	-2781.436*	-9.198	176.420	7276.499
	(4713.43)	(1436.99)	(132.76)	(2979.19)	(5639.89)
	[46.1]	[35.8]	[-1.3]	[2.3]	[26.3]
d	-19055.532***	-2174.693	-7.063	1666.543	7380.312
	(5301.30)	(1626.70)	(60.87)	(2787.06)	(6026.73)
	[58.5]	[31.1]	[-1.5]	[17.4]	[26.3]
e	-9476.098*	-1970.678	-8.275	-1203.577	-1082.292
	(4926.81)	(1505.65)	(87.18)	(2513.15)	(5968.01)
	[35.9]	[29.7]	[-1.7]	[-18.2]	[-4.7]
Reweighting error:					
a	-915.874	-250.807	6.803	315.953	631.337
	(942.51)	(199.04)	(17.32)	(206.45)	(609.31)
	[2.8]	[3.9]	[1.3]	[3.1]	[2.3]

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	Percentile				
	10th	25th	50th	75th	90th
b	-1119.685	-295.216	4.779	296.888	788.371
	(938.16)	(198.93)	(8.46)	(189.99)	(652.25)
	[3.8]	[4.3]	[0.9]	[3.6]	[3.0]
c	-863.691	-234.364	7.880	316.384	616.601
	(817.33)	(192.91)	(34.60)	(213.35)	(639.75)
	[2.6]	[3.0]	[1.1]	[4.1]	[2.2]
d	-831.500	-231.117	6.632	370.638*	717.074
	(865.50)	(190.32)	(11.04)	(206.50)	(548.64)
	[2.6]	[3.3]	[1.4]	[3.9]	[2.6]
e	-595.636	-185.432	8.485	357.015	836.614
	(1017.56)	(218.96)	(29.52)	(225.66)	(688.41)
	[2.3]	[2.8]	[1.7]	[5.4]	[3.6]
Observations	11445	11445	11445	11445	11445

Note: The table shows for each imputation implicate *a* to *e* the estimated RIF decomposition results at several percentiles. Example: The RIF composition effect at the 75th percentile in the imputed dataset *c* is 8,129.705 euros, explaining 105.2 percent of the estimated savings gap. Observed and estimated gap defined as native savings minus migrants savings. Panel sample 2002–2007 as defined in Section 4.2. All calculations weighted with longitudinal weights. Clustered bootstrap standard errors in parentheses. Effect as percentage of estimated gap in square brackets. * significant at 10%, ** significant at 5%, *** significant at 1%. Source: SOEP v35, own calculations.

Table 4.22: Detailed RIF composition effects for five imputation implicates *a* to *e*, 2002–2007.

	Percentile				
	10th	25th	50th	75th	90th
Estimated gap:					
a	-32258.116***	-6477.923***	538.496	10237.927***	27229.149***
	(6402.27)	(1851.70)	(350.38)	(2409.09)	(5979.63)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
b	-29750.717***	-6934.968***	516.726	8210.834***	25945.755***
	(6360.90)	(1540.46)	(327.78)	(2506.28)	(5305.92)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
c	-33430.156***	-7780.031***	722.240**	7728.133***	27699.075***
	(4035.83)	(1731.17)	(333.86)	(2761.06)	(5521.19)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
d	-32551.550***	-6984.203***	481.382*	9552.403***	28070.372***
	(5623.58)	(1862.86)	(287.59)	(2485.19)	(5937.78)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
e	-26380.344***	-6643.634***	485.909	6611.994**	23159.591***
	(5851.67)	(1748.17)	(322.49)	(2626.58)	(5954.98)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
Age:					
a	354.180	247.522	38.784	-184.477	-234.150
	(383.35)	(275.40)	(76.47)	(211.02)	(707.99)
	[1.1]	[3.8]	[7.2]	[-1.8]	[-0.9]

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	Percentile				
	10th	25th	50th	75th	90th
b	324.823	254.488	50.338	-229.779	-311.381
	(442.27)	(273.21)	(65.97)	(245.83)	(663.32)
	[1.1]	[3.7]	[9.7]	[-2.8]	[-1.2]
c	293.537	204.863	28.252	-152.972	-82.059
	(350.38)	(235.62)	(56.27)	(221.97)	(704.86)
	[0.9]	[2.6]	[3.9]	[-2.0]	[-0.3]
d	197.727	205.813	45.916	-153.901	-232.286
	(389.32)	(249.15)	(59.37)	(217.98)	(698.82)
	[0.6]	[2.9]	[9.5]	[-1.6]	[-0.8]
e	257.259	195.160	36.848	-174.000	-457.802
	(431.00)	(235.86)	(61.48)	(237.60)	(749.89)
	[1.0]	[2.9]	[7.6]	[-2.6]	[-2.0]
Female:					
a	5.211	-56.992	-58.989	-171.574	-217.046
	(189.18)	(80.97)	(47.85)	(134.56)	(314.46)
	[0.0]	[-0.9]	[-11.0]	[-1.7]	[-0.8]
b	63.523	-67.481	-67.449	-159.818	-178.746
	(183.13)	(70.72)	(54.88)	(129.43)	(295.42)
	[0.2]	[-1.0]	[-13.1]	[-1.9]	[-0.7]
c	-112.242	-114.526	-93.046	-131.023	-453.077
	(187.68)	(100.84)	(61.40)	(123.75)	(404.38)
	[-0.3]	[-1.5]	[-12.9]	[-1.7]	[-1.6]
d	-26.983	-52.263	-55.193	-104.160	-503.392
	(169.29)	(69.99)	(42.75)	(107.25)	(451.39)
	[-0.1]	[-0.7]	[-11.5]	[-1.1]	[-1.8]
e	-145.813	-84.751	-61.183	-139.787	-327.597
	(196.16)	(87.87)	(53.06)	(124.23)	(341.20)
	[-0.6]	[-1.3]	[-12.6]	[-2.1]	[-1.4]
Income:					
a	-2373.685*	268.087	676.177**	3735.757***	12518.822***
	(1377.96)	(321.47)	(266.28)	(723.58)	(2769.98)
	[-7.4]	[4.1]	[125.6]	[36.5]	[46.0]
b	-1865.005	435.985	717.295***	3809.619***	10470.671***
	(1171.10)	(273.71)	(275.24)	(785.64)	(2600.47)
	[-6.3]	[6.3]	[138.8]	[46.4]	[40.4]
c	-2041.995*	506.812*	826.079***	3726.809***	11326.118***
	(1066.66)	(276.39)	(233.50)	(754.37)	(3052.68)
	[-6.1]	[6.5]	[114.4]	[48.2]	[40.9]
d	-1722.645	299.331	607.644**	3595.300***	13586.699***
	(1068.36)	(266.44)	(269.68)	(735.61)	(3522.47)
	[-5.3]	[4.3]	[126.2]	[37.6]	[48.4]
e	-2616.964**	401.248	594.333**	3578.723***	11020.769***
	(1247.80)	(270.85)	(290.28)	(697.66)	(3229.58)
	[-9.9]	[6.0]	[122.3]	[54.1]	[47.6]
Risk attitude:					

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	Percentile				
	10th	25th	50th	75th	90th
a	-334.933	-230.499	15.421	131.865	208.792
	(454.64)	(169.36)	(43.91)	(187.60)	(592.40)
	[-1.0]	[-3.6]	[2.9]	[1.3]	[0.8]
b	-315.609	-109.372	48.054	315.833	332.491
	(419.53)	(131.36)	(43.27)	(208.78)	(588.50)
	[-1.1]	[-1.6]	[9.3]	[3.8]	[1.3]
c	-137.229	-171.397	48.975	302.239	754.898
	(382.27)	(140.58)	(48.61)	(217.10)	(652.49)
	[-0.4]	[-2.2]	[6.8]	[3.9]	[2.7]
d	-143.108	-237.380	28.190	131.619	277.699
	(397.28)	(167.80)	(41.17)	(205.34)	(607.85)
	[-0.4]	[-3.4]	[5.9]	[1.4]	[1.0]
e	-51.797	-46.803	64.507	421.839*	885.027
	(415.27)	(126.63)	(53.33)	(229.64)	(810.35)
	[-0.2]	[-0.7]	[13.3]	[6.4]	[3.8]
Education:					
a	-240.364	-123.640	292.196	383.384	2661.188
	(965.95)	(516.10)	(181.57)	(628.65)	(1936.96)
	[-0.7]	[-1.9]	[54.3]	[3.7]	[9.8]
b	-532.883	-247.999	266.962	282.457	2911.641**
	(1244.03)	(445.56)	(173.36)	(567.50)	(1325.70)
	[-1.8]	[-3.6]	[51.7]	[3.4]	[11.2]
c	-815.996	-247.010	218.124	705.346	1860.058
	(863.63)	(472.30)	(177.91)	(609.81)	(2127.63)
	[-2.4]	[-3.2]	[30.2]	[9.1]	[6.7]
d	-665.261	-459.844	256.647	770.020	4199.937***
	(915.69)	(445.00)	(179.22)	(602.62)	(1589.90)
	[-2.0]	[-6.6]	[53.3]	[8.1]	[15.0]
e	-906.565	-546.028	229.976	643.110	2981.872
	(906.05)	(518.25)	(187.20)	(572.37)	(2056.02)
	[-3.4]	[-8.2]	[47.3]	[9.7]	[12.9]
Wealth transfers:					
a	1142.603*	615.206***	308.364**	1471.562***	5075.917***
	(583.12)	(203.48)	(130.20)	(398.43)	(1461.58)
	[3.5]	[9.5]	[57.3]	[14.4]	[18.6]
b	1201.388**	499.916***	305.851**	1464.101***	4134.179***
	(606.70)	(189.69)	(125.38)	(395.49)	(1198.78)
	[4.0]	[7.2]	[59.2]	[17.8]	[15.9]
c	966.059**	377.530**	305.935***	1355.385***	4545.993***
	(482.62)	(162.11)	(105.62)	(366.31)	(1386.78)
	[2.9]	[4.9]	[42.4]	[17.5]	[16.4]
d	947.465*	503.292***	256.730**	1447.033***	4148.144***
	(535.37)	(183.67)	(110.41)	(392.13)	(1333.61)
	[2.9]	[7.2]	[53.3]	[15.1]	[14.8]
e	999.274*	496.632***	296.829**	1475.677***	4671.347***

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	Percentile				
	10^{th}	25^{th}	50^{th}	75^{th}	90^{th}
	(558.58)	(192.35)	(139.91)	(380.75)	(1427.03)
	[3.8]	[7.5]	[61.1]	[22.3]	[20.2]
Remittances:					
a	422.586	211.817	-56.581	317.715	1015.700
	(738.40)	(274.25)	(84.03)	(353.00)	(995.80)
	[1.3]	[3.3]	[-10.5]	[3.1]	[3.7]
b	638.538	326.998	77.481	598.911*	1146.215
	(800.30)	(228.06)	(78.10)	(347.80)	(859.46)
	[2.1]	[4.7]	[15.0]	[7.3]	[4.4]
c	336.554	241.426	33.973	575.165*	392.579
	(677.62)	(238.01)	(79.23)	(334.88)	(1046.56)
	[1.0]	[3.1]	[4.7]	[7.4]	[1.4]
d	641.698	289.845	8.120	499.030	1250.151
	(732.28)	(256.30)	(63.00)	(368.44)	(966.54)
	[2.0]	[4.2]	[1.7]	[5.2]	[4.5]
e	480.782	507.423*	16.561	578.279*	985.001
	(709.66)	(262.18)	(70.64)	(333.73)	(1023.82)
	[1.8]	[7.6]	[3.4]	[8.7]	[4.3]
Household size:					
a	1388.275	-325.758	-66.822	-974.138	-5371.650**
	(1568.32)	(476.23)	(140.02)	(669.70)	(2219.44)
	[4.3]	[-5.0]	[-12.4]	[-9.5]	[-19.7]
b	1069.946	-90.031	71.560	-279.539	-3830.451**
	(1540.40)	(432.08)	(130.05)	(571.12)	(1934.14)
	[3.6]	[-1.3]	[13.8]	[-3.4]	[-14.8]
c	110.142	-339.882	-78.835	-900.790	-3336.154
	(1329.29)	(416.24)	(140.45)	(636.33)	(2039.58)
	[0.3]	[-4.4]	[-10.9]	[-11.7]	[-12.0]
d	790.679	-478.956	-47.393	-662.502	-3889.847*
	(1336.08)	(464.45)	(124.75)	(662.18)	(2256.42)
	[2.4]	[-6.9]	[-9.8]	[-6.9]	[-13.9]
e	568.713	-303.583	24.795	-339.950	-3959.698*
	(1506.83)	(477.55)	(126.34)	(585.42)	(2235.50)
	[2.2]	[-4.6]	[5.1]	[-5.1]	[-17.1]
Share of real estate:					
a	-11693.062***	-5127.094***	-342.756**	1622.640***	3542.786***
	(3244.18)	(1391.17)	(150.12)	(513.80)	(1343.31)
	[-36.2]	[-79.1]	[-63.7]	[15.8]	[13.0]
b	-10763.405***	-4294.573***	-324.587**	1340.621***	2385.707**
	(3150.87)	(1174.38)	(147.81)	(488.03)	(1184.26)
	[-36.2]	[-61.9]	[-62.8]	[16.3]	[9.2]
c	-10304.458***	-4414.487***	-354.361***	1417.040***	2847.865**
	(2660.31)	(1134.14)	(130.06)	(481.94)	(1434.65)
	[-30.8]	[-56.7]	[-49.1]	[18.3]	[10.3]
d	-10505.143***	-4862.845***	-367.853**	1793.005***	2403.173*

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	Percentile				
	10th	25th	50th	75th	90th
	(3171.01)	(1191.45)	(159.27)	(511.96)	(1308.87)
	[-32.3]	[-69.6]	[-76.4]	[18.8]	[8.6]
e	-11091.095***	-4668.898***	-350.255**	1448.991***	2821.220**
	(2896.00)	(1203.86)	(170.91)	(480.87)	(1406.62)
	[-42.0]	[-70.3]	[-72.1]	[21.9]	[12.2]
Share of other assets:					
a	-763.559	-1384.238**	243.715*	1481.205***	3159.455**
	(1613.44)	(669.00)	(145.96)	(445.16)	(1393.53)
	[-2.4]	[-21.4]	[45.3]	[14.5]	[11.6]
b	-834.398	-1244.595**	203.311	1366.213***	2214.517*
	(1756.54)	(619.98)	(129.23)	(425.60)	(1322.49)
	[-2.8]	[-17.9]	[39.3]	[16.6]	[8.5]
c	-453.836	-1416.628**	265.576**	1359.903***	2483.261
	(1086.96)	(552.82)	(128.64)	(459.27)	(1575.61)
	[-1.4]	[-18.2]	[36.8]	[17.6]	[9.0]
d	-241.387	-1283.121**	167.638	1530.254***	2945.947**
	(1125.13)	(551.03)	(120.84)	(463.47)	(1457.80)
	[-0.7]	[-18.4]	[34.8]	[16.0]	[10.5]
e	-163.513	-1289.673**	154.841	1503.955***	3325.604*
	(1786.06)	(625.73)	(140.76)	(456.18)	(1856.19)
	[-0.6]	[-19.4]	[31.9]	[22.7]	[14.4]
Share of liabilities:					
a	-263.761	-97.044	-53.342	-81.892	-11.657
	(1177.11)	(427.87)	(226.55)	(356.08)	(211.05)
	[-0.8]	[-1.5]	[-9.9]	[-0.8]	[-0.0]
b	-520.775	-160.042	-107.336	-138.126	-56.494
	(1101.80)	(322.43)	(207.71)	(307.05)	(277.50)
	[-1.8]	[-2.3]	[-20.8]	[-1.7]	[-0.2]
c	-426.482	-136.018	-111.051	-127.397	-159.310
	(1085.51)	(345.04)	(237.03)	(307.93)	(537.30)
	[-1.3]	[-1.7]	[-15.4]	[-1.6]	[-0.6]
d	-273.432	-98.578	-56.717	-94.483	-53.951
	(1080.14)	(369.46)	(194.52)	(376.20)	(285.83)
	[-0.8]	[-1.4]	[-11.8]	[-1.0]	[-0.2]
e	-824.289	-274.114	-159.468	-239.666	-156.238
	(1193.44)	(396.91)	(229.80)	(345.18)	(405.71)
	[-3.1]	[-4.1]	[-32.8]	[-3.6]	[-0.7]
Observations	11445	11445	11445	11445	11445

Note: The table shows for each imputation implicate *a* to *e* the estimated RIF composition effect for different variables at several percentiles. Example: The composition effect of age at the 75th percentile in the imputed dataset *c* is −152.972 euros, explaining −2.0 percent of the estimated savings gap. The estimated gap defined as native savings less migrant savings. Based on panel sample 2002–2007 as defined in Section 4.2. All statistics weighted with longitudinal weights. Clustered bootstrap standard errors in parentheses. Percentage share of estimated gap in square brackets. * significant at 10%, ** significant at 5%, *** significant at 1%. Source: SOEP v35, own calculations.

Table 4.23: Detailed RIF coefficient effects for five imputation implicates *a* to *e*, 2002–2007.

	Percentile				
	10th	25th	50th	75th	90th
Estimated gap:					
a	-32258.116***	-6477.923***	538.496	10237.927***	27229.149***
	(6402.27)	(1851.70)	(350.38)	(2409.09)	(5979.63)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
b	-29750.717***	-6934.968***	516.726	8210.834***	25945.755***
	(6360.90)	(1540.46)	(327.78)	(2506.28)	(5305.92)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
c	-33430.156***	-7780.031***	722.240**	7728.133***	27699.075***
	(4035.83)	(1731.17)	(333.86)	(2761.06)	(5521.19)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
d	-32551.550***	-6984.203***	481.382*	9552.403***	28070.372***
	(5623.58)	(1862.86)	(287.59)	(2485.19)	(5937.78)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
e	-26380.344***	-6643.634***	485.909	6611.994**	23159.591***
	(5851.67)	(1748.17)	(322.49)	(2626.58)	(5954.98)
	[100.0]	[100.0]	[100.0]	[100.0]	[100.0]
Age:					
a	4526.831	-3945.333	-9.062	12518.551*	3472.156
	(11250.18)	(4841.20)	(803.84)	(7126.37)	(12159.22)
	[14.0]	[-60.9]	[-1.7]	[122.3]	[12.8]
b	1156.376	-2671.390	-46.635	12060.290	12568.199
	(10039.85)	(3844.44)	(1044.31)	(7796.28)	(11343.97)
	[3.9]	[-38.5]	[-9.0]	[146.9]	[48.4]
c	-1666.459	-5073.670	-5.954	17059.143*	2088.456
	(8344.26)	(3744.38)	(695.64)	(8812.57)	(13033.96)
	[-5.0]	[-65.2]	[-0.8]	[220.7]	[7.5]
d	2242.128	-725.942	-8.752	9645.558	6892.712
	(10258.93)	(3971.77)	(352.04)	(7526.49)	(10593.85)
	[6.9]	[-10.4]	[-1.8]	[101.0]	[24.6]
e	3721.980	-3431.558	-53.431	12403.931	6189.734
	(8030.34)	(3745.48)	(461.96)	(8178.75)	(11961.75)
	[14.1]	[-51.7]	[-11.0]	[187.6]	[26.7]
Female:					
a	-7725.350	1413.785	53.942	2605.098	6662.485
	(5690.23)	(1239.82)	(196.06)	(2098.25)	(5954.81)
	[-23.9]	[21.8]	[10.0]	[25.4]	[24.5]
b	-2983.071	1736.062	55.764	1885.383	3979.388
	(5459.33)	(1198.33)	(181.07)	(1899.91)	(7889.38)
	[-10.0]	[25.0]	[10.8]	[23.0]	[15.3]
c	-3451.068	1820.635*	123.452	2321.435	5670.006
	(3859.07)	(1083.64)	(305.23)	(2433.86)	(6019.11)
	[-10.3]	[23.4]	[17.1]	[30.0]	[20.5]
d	-1765.516	616.260	89.776	2917.203	7748.517
	(5643.74)	(1245.92)	(118.33)	(1933.00)	(5847.47)

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	Percentile				
	10th	25th	50th	75th	90th
	[-5.4]	[8.8]	[18.6]	[30.5]	[27.6]
e	98.390	1427.058	56.898	1439.301	5604.817
	(5498.01)	(1333.99)	(224.35)	(2253.41)	(5689.22)
	[0.4]	[21.5]	[11.7]	[21.8]	[24.2]
Income:					
a	-3689.068	-507.282	3.596	-1190.002	4505.964
	(8291.65)	(1552.73)	(362.41)	(2932.39)	(11310.61)
	[-11.4]	[-7.8]	[0.7]	[-11.6]	[16.5]
b	2375.308	-61.750	13.752	-1325.020	6825.321
	(7815.37)	(1279.74)	(301.89)	(2648.90)	(14324.22)
	[8.0]	[-0.9]	[2.7]	[-16.1]	[26.3]
c	-3035.956	-432.646	41.857	-1492.880	10568.504
	(6365.57)	(1493.58)	(447.88)	(2958.44)	(12313.24)
	[-9.1]	[-5.6]	[5.8]	[-19.3]	[38.2]
d	2996.997	-802.357	-2.217	-2455.306	53.775
	(9623.70)	(1530.79)	(201.66)	(3134.18)	(15416.35)
	[9.2]	[-11.5]	[-0.5]	[-25.7]	[0.2]
e	-2158.470	-622.339	23.078	-2780.301	7374.930
	(8095.31)	(1592.64)	(232.57)	(2997.34)	(11252.01)
	[-8.2]	[-9.4]	[4.7]	[-42.0]	[31.8]
Risk attitude:					
a	4532.686	143.880	-19.664	291.120	4219.571
	(6807.85)	(993.26)	(172.90)	(1791.45)	(5609.79)
	[14.1]	[2.2]	[-3.7]	[2.8]	[15.5]
b	3263.489	1429.447	54.669	2385.086	6696.597
	(5918.51)	(1110.44)	(56.76)	(1986.97)	(6798.43)
	[11.0]	[20.6]	[10.6]	[29.0]	[25.8]
c	2678.190	1283.187	-13.381	2213.316	-698.590
	(4271.12)	(1071.67)	(163.00)	(2479.53)	(5818.59)
	[8.0]	[16.5]	[-1.9]	[28.6]	[-2.5]
d	3747.072	1718.217	27.409	2414.310	3821.698
	(9378.85)	(1176.57)	(54.42)	(2099.09)	(5052.13)
	[11.5]	[24.6]	[5.7]	[25.3]	[13.6]
e	11151.108*	2715.146*	39.696	4289.732*	4676.340
	(5955.87)	(1502.21)	(162.18)	(2483.14)	(6316.81)
	[42.3]	[40.9]	[8.2]	[64.9]	[20.2]
Education:					
a	-6954.735	-271.795	-386.061	-4711.670	17919.415
	(10921.75)	(5543.35)	(1733.05)	(7701.06)	(12947.17)
	[-21.6]	[-4.2]	[-71.7]	[-46.0]	[65.8]
b	-15817.714*	-4839.911	-89.414	-8660.589	10574.307
	(9517.26)	(4382.46)	(2291.53)	(7689.54)	(12829.50)
	[-53.2]	[-69.8]	[-17.3]	[-105.5]	[40.8]
c	-3603.142	-1833.679	-464.827	-10578.990	18609.237
	(6934.31)	(4760.19)	(989.61)	(8558.38)	(13568.55)

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	Percentile				
	10th	25th	50th	75th	90th
	[-10.8]	[-23.6]	[-64.4]	[-136.9]	[67.2]
d	-13372.196	-5226.751	-562.418	-3699.287	17142.826
	(9628.49)	(5568.17)	(650.52)	(7151.42)	(11448.66)
	[-41.1]	[-74.8]	[-116.8]	[-38.7]	[61.1]
e	-9262.352	-2054.312	-409.009	-10513.127	16518.357
	(8093.12)	(4485.22)	(644.22)	(7487.09)	(11572.80)
	[-35.1]	[-30.9]	[-84.2]	[-159.0]	[71.3]
Wealth transfers:					
a	-959.047	-184.912	6.147	863.780	2034.730
	(1382.51)	(359.81)	(69.65)	(603.98)	(1768.59)
	[-3.0]	[-2.9]	[1.1]	[8.4]	[7.5]
b	-940.729	-142.559	4.209	489.992	2719.269
	(1247.67)	(353.50)	(73.53)	(544.25)	(1884.85)
	[-3.2]	[-2.1]	[0.8]	[6.0]	[10.5]
c	-976.982	-101.418	9.060	622.737	2674.914
	(961.09)	(265.06)	(64.54)	(719.35)	(1784.49)
	[-2.9]	[-1.3]	[1.3]	[8.1]	[9.7]
d	-866.458	-114.536	4.219	273.167	-464.172
	(1483.45)	(313.92)	(44.68)	(636.02)	(2300.83)
	[-2.7]	[-1.6]	[0.9]	[2.9]	[-1.7]
e	-2423.134*	-77.032	6.803	43.662	862.422
	(1274.28)	(345.76)	(61.27)	(720.99)	(1914.40)
	[-9.2]	[-1.2]	[1.4]	[0.7]	[3.7]
Remittances:					
a	-6857.132	-24.005	6.330	1584.363	-1444.752
	(6084.29)	(912.05)	(134.53)	(1480.01)	(5491.52)
	[-21.3]	[-0.4]	[1.2]	[15.5]	[-5.3]
b	-3822.581	-399.446	-8.848	305.261	5020.886
	(4981.73)	(926.21)	(206.46)	(1506.60)	(5529.72)
	[-12.8]	[-5.8]	[-1.7]	[3.7]	[19.4]
c	-3146.961	-1230.378	-19.048	-461.777	4776.229
	(4004.44)	(1001.83)	(89.46)	(1773.10)	(5151.20)
	[-9.4]	[-15.8]	[-2.6]	[-6.0]	[17.2]
d	-1461.359	-781.593	-7.364	639.833	4212.687
	(4730.80)	(1100.81)	(36.28)	(1446.98)	(5040.51)
	[-4.5]	[-11.2]	[-1.5]	[6.7]	[15.0]
e	-3931.342	-471.289	5.702	2319.513	2291.400
	(4126.44)	(1136.58)	(66.89)	(1904.42)	(5429.90)
	[-14.9]	[-7.1]	[1.2]	[35.1]	[9.9]
Household size:					
a	3157.033	5530.524	108.378	695.713	11145.155
	(15958.37)	(3542.12)	(350.49)	(6140.00)	(21849.26)
	[9.8]	[85.4]	[20.1]	[6.8]	[40.9]
b	-16436.892	277.872	-45.755	5163.351	25163.474
	(19057.55)	(3302.43)	(388.60)	(6584.36)	(24115.18)

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	Percentile				
	10th	25th	50th	75th	90th
	[-55.2]	[4.0]	[-8.9]	[62.9]	[97.0]
c	13742.065	1628.985	108.870	5135.905	47475.777**
	(11031.64)	(3561.87)	(353.08)	(8169.60)	(23815.59)
	[41.1]	[20.9]	[15.1]	[66.5]	[171.4]
d	-2637.388	1668.677	-11.607	3603.034	8689.192
	(26654.66)	(3588.81)	(208.43)	(6074.46)	(21801.54)
	[-8.1]	[23.9]	[-2.4]	[37.7]	[31.0]
e	-21803.104	-1091.889	62.426	3581.585	7210.709
	(19662.46)	(3671.53)	(308.35)	(7315.24)	(21272.19)
	[-82.6]	[-16.4]	[12.8]	[54.2]	[31.1]
Share of real estate:					
a	-16769.916	-3037.163	-32.506	-1625.094	-1931.727
	(11365.25)	(2030.26)	(132.14)	(1879.10)	(4081.76)
	[-52.0]	[-46.9]	[-6.0]	[-15.9]	[-7.1]
b	-13379.764	-4637.926**	-45.525	-2208.334	116.870
	(9312.34)	(1970.62)	(85.89)	(1901.18)	(5209.94)
	[-45.0]	[-66.9]	[-8.8]	[-26.9]	[0.5]
c	-22897.546***	-4710.396**	-21.013	-3673.010	759.955
	(8674.42)	(1951.31)	(153.27)	(2662.01)	(4065.14)
	[-68.5]	[-60.5]	[-2.9]	[-47.5]	[2.7]
d	-17233.605	-2992.659	6.039	-450.036	4548.158
	(13675.02)	(2088.71)	(55.13)	(1605.14)	(3748.43)
	[-52.9]	[-42.8]	[1.3]	[-4.7]	[16.2]
e	-6960.680	-3916.687**	-29.960	-4041.974	810.024
	(6923.85)	(1903.95)	(134.51)	(3060.08)	(4201.06)
	[-26.4]	[-59.0]	[-6.2]	[-61.1]	[3.5]
Share of other assets:					
a	-23.369	-1767.131	33.003	-1591.782	-3008.281
	(4117.17)	(2266.60)	(69.44)	(1659.58)	(4995.79)
	[-0.1]	[-27.3]	[6.1]	[-15.5]	[-11.0]
b	-1818.803	-2976.350	-21.792	-1941.598	-2111.650
	(3793.70)	(1833.73)	(73.87)	(1717.50)	(5302.46)
	[-6.1]	[-42.9]	[-4.2]	[-23.6]	[-8.1]
c	-1561.215	-2950.620	27.142	-3288.809	-1358.677
	(3414.89)	(2085.01)	(86.00)	(2347.44)	(5671.66)
	[-4.7]	[-37.9]	[3.8]	[-42.6]	[-4.9]
d	-833.616	-1338.818	44.933	-2627.357	-3039.095
	(5014.80)	(2615.58)	(58.35)	(1828.81)	(5432.93)
	[-2.6]	[-19.2]	[9.3]	[-27.5]	[-10.8]
e	4594.148	-1131.667	37.979	-2041.342	-3871.363
	(3754.50)	(2091.17)	(71.26)	(2096.61)	(5331.27)
	[17.4]	[-17.0]	[7.8]	[-30.9]	[-16.7]
Share of liabilities:					
a	4204.453	1052.421	51.329	-411.788	-5032.031
	(3047.82)	(663.24)	(561.18)	(1305.04)	(3349.81)

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	Percentile				
	10th	25th	50th	75th	90th
	[13.0]	[16.2]	[9.5]	[-4.0]	[-18.5]
b	1520.986	1080.925*	39.029	418.175	1196.662
	(2721.22)	(638.13)	(592.52)	(1234.56)	(3947.72)
	[5.1]	[15.6]	[7.6]	[5.1]	[4.6]
c	5534.896**	1849.005***	71.315	90.794	567.383
	(2345.68)	(599.45)	(507.27)	(1301.10)	(3201.63)
	[16.6]	[23.8]	[9.9]	[1.2]	[2.0]
d	3058.874	1337.081**	62.336	308.329	-1332.079
	(3911.70)	(679.10)	(231.96)	(1222.09)	(2940.46)
	[9.4]	[19.1]	[12.9]	[3.2]	[-4.7]
e	345.448	1215.360	47.429	-1271.493	-2284.591
	(2888.46)	(754.99)	(369.80)	(1400.64)	(3130.14)
	[1.3]	[18.3]	[9.8]	[-19.2]	[-9.9]
Constant:					
a	11546.658	415.241	177.667	-5878.739	-33032.209
	(25692.51)	(6209.69)	(1914.30)	(10720.33)	(33661.21)
	[35.8]	[6.4]	[33.0]	[-57.4]	[-121.3]
b	32183.793	8808.250*	86.776	-7753.709	-67198.683**
	(23010.96)	(5187.77)	(2379.22)	(10117.73)	(34023.50)
	[108.2]	[127.0]	[16.8]	[-94.4]	[-259.0]
c	2974.470	6969.559	133.328	-7771.443	-83856.696**
	(18700.30)	(6533.02)	(1185.41)	(11917.76)	(35891.33)
	[8.9]	[89.6]	[18.5]	[-100.6]	[-302.7]
d	7069.534	4467.730	350.584	-8902.904	-40893.908
	(28427.41)	(6384.48)	(565.56)	(10401.11)	(29197.63)
	[21.7]	[64.0]	[72.8]	[-93.2]	[-145.7]
e	17151.909	5468.531	204.113	-4633.066	-46465.072
	(22885.52)	(5915.76)	(913.80)	(12449.77)	(31592.02)
	[65.0]	[82.3]	[42.0]	[-70.1]	[-200.6]
Observations	11445	11445	11445	11445	11445

Note: The table shows for each imputation implicate *a* to *e* the estimated RIF coefficient effect for different variables at several percentiles. Example: The coefficient effect of age at the 75th percentile in the imputed dataset *c* is 17,059.143 euros, explaining 220.7 percent of the estimated savings gap. The estimated gap defined as native savings less migrant savings. Based on panel sample 2002–2007 as defined in Section 4.2. All statistics weighted with longitudinal weights. Clustered bootstrap standard errors in parentheses. Percentage share of estimated gap in square brackets. * significant at 10%, ** significant at 5%, *** significant at 1%. Source: SOEP v35, own calculations.

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Summary

This dissertation consists of four empirical chapters on inequality, with Chapters 1 and 2 focusing on regional inequality, and Chapters 3 and 4 examining the origins of inequality at the individual level.

The first chapter makes a methodological contribution to the literature on inequality across regions by providing the `fayherriot` command for the statistical software Stata. The command implements the Fay-Herriot model (Fay and Herriot, 1979), a small-area estimation technique (Rao and Molina, 2015) that improves the precision of region-level direct estimates using region-level covariates. The command implements the default model and encompasses additional options to a) produce out-of-sample predictions, b) adjust non-positive random effects variance estimates, and c) deal with the violation of model assumptions. An application of the command in the last part of the chapter shows that the statistical precision of regional income estimates can be considerably improved, allowing for a more robust examination of inequality between regions.

Similar to the first chapter, the second chapter is concerned with providing improved data for the analysis of regional differences. For this purpose, the chapter presents a novel regional database of tax revenues for the interwar period in Germany. The database contains annual income and payroll, corporate, wealth, and turnover tax revenues for 900 tax districts in the former German Empire over the period 1926 to 1938. Moreover, the database provides geocoded borders for each tax district and year, allowing researchers to flexibly link the tax data to other geocoded data sources. The use of the data is further facilitated by a detailed description of the interwar German tax system in the second part of the chapter. Comparing the tax data with historical regional GDP estimates, the chapter finds high correlations, suggesting that tax data are valid proxy for regional economic development and a useful data source for regional analyses.

The third chapter focuses on individual inequality and one of the largest shocks to private wealth in 20th century Germany: the destruction of the housing stock during the Second World War. By the end of the war, an estimated 20 percent of the West German housing stock had been destroyed, and the chapter examines the extent to which regional differences in destruction can explain differences in private wealth today. As the empirical basis, the analysis links a unique dataset on the levels of wartime destruction in 1,739 West German cities with recent micro data on household wealth provided by the German Socio-Economic Panel (SOEP). The results indicate that wealth is still significantly lower today among individuals born in cities or villages that were badly damaged. Similarly, the destruction of parents' cities or villages of birth has significant negative effects on the wealth of their descendants.

These detrimental effects are robust after controlling for a rich set of pre-war regional and city-level control variables. In a complementary mediation analysis, the chapter studies potential channels such as health, education, and work experience, through which the wartime destruction could have affected wealth accumulation.

The fourth chapter investigates wealth inequality between migrants and natives in Germany. In particular, the chapter examines the role of characteristics and behavior for the development of the large wealth gaps between the two groups. Based on data from the SOEP, the results of this chapter show that the native-migrant wealth gap is large and persistent throughout the 2002 to 2017 period. A subsequent decomposition analysis exploits the panel dimension of the data and shows that working-age migrants cannot significantly catch up with natives in terms of net wealth because they lack sufficient levels of income, inheritances, and inter-vivos gifts. The results also indicate that especially native individuals consume, transfer, or lose significant amounts of wealth over time, which reduces the pace at which the wealth inequality between migrants and natives increases.

Zusammenfassung

Diese Dissertation setzt sich aus vier empirischen Kapiteln über Ungleichheit zusammen, wobei die Kapitel 1 und 2 regionale Ungleichheiten behandeln, während die Kapitel 3 und 4 Ungleichheit auf individueller Ebene untersuchen.

Das erste Kapitel leistet einen methodischen Beitrag zur Literatur über regionale Ungleichheit, indem es den Befehl `fayherriot` für die Statistiksoftware Stata bereitstellt. Der Befehl implementiert das Fay-Herriot-Modell (Fay and Herriot, 1979), eine Small-Area-Methode (Rao and Molina, 2015), die die Genauigkeit direkter Schätzungen auf regionaler Ebene unter Verwendung von regionaler Kovariate verbessert. Der Befehl implementiert das Standardmodell und umfasst zusätzliche Optionen, um a) Out-of-Sample-Vorhersagen zu treffen, b) nichtpositive Schätzungen der Fehlertermvarianz zu korrigieren und c) mit weiteren Verletzung von Modellannahmen umzugehen. Eine Anwendung des Befehls im letzten Teil des Kapitels zeigt, dass das Fay-Herriot-Modell die statistische Genauigkeit von regionalen Einkommensschätzungen erheblich verbessern kann, was eine robustere Untersuchung der Ungleichheit zwischen Regionen ermöglicht.

Ähnlich wie das erste Kapitel hat das zweite Kapitel das Ziel, die Datengrundlage für die Analyse regionaler Unterschiede zu verbessern. Zu diesem Zweck wird in dem Kapitel eine neue regionale Datenbank mit Steuereinnahmen aus der Zwischenkriegszeit in Deutschland bereit- und vorgestellt. Die Datenbank enthält die jährlichen Steuereinnahmen aus der Einkommen- , Körperschaft-, Vermögen- und Umsatzsteuer sowie die des Lohnsteuerabzugs für die rund 900 Finanzämter im ehemaligen Deutschen Reich im Zeitraum von 1926 bis 1938. Darüber hinaus bietet die Datenbank geocodierte Grenzen für jedes Jahr und jeden Finanzamtsbezirk, so dass die Steuerdaten flexibel mit anderen geocodierten Datenquellen verknüpft werden können. Um die Datennutzung weiter zu erleichtern, ist im zweiten Teil des Kapitels eine detaillierte Beschreibung des deutschen Steuersystems der Zwischenkriegszeit enthalten. Beim Vergleich der Steuerdaten mit Schätzungen für das regionale, historische Bruttoinlandsprodukt werden hohe Korrelationen festgestellt, was darauf hindeutet, dass die Steuerdaten ein gültiger Proxy für die regionale Wirtschaftsentwicklung und eine nützliche Datenquelle für regionale Analysen sind.

Das dritte Kapitel behandelt Ungleichheiten zwischen Personen und analysiert einen der größten Schocks für das Privatvermögen in Deutschland im 20. Jahrhundert: die Zerstörung des Wohnungsbestands während des Zweiten Weltkriegs. Bei Kriegsende waren schätzungsweise 20 Prozent des westdeutschen Wohnungsbestands zerstört, und in diesem Kapitel wird untersucht, inwieweit regionale Unterschiede bei der Zerstörung Unterschiede im heutigen Privatvermögen erklären können. Als empirische Grundlage verknüpft die Analyse einen detaillierten Datensatz über das Ausmaß der Kriegszerstörungen in 1,739 westdeutschen Städten mit aktuellen Mikrodaten zum Vermögen privater Haushalte aus dem Sozio-oekonomischen Panel (SOEP). Die Ergebnisse zeigen, dass das Vermögen von Personen, die in stark zerstörten Städten oder Dörfern geboren wurden, auch heute noch deutlich geringer ist. Ebenso hat die Zerstörung der Geburtsorte der Eltern signifikante negative Auswirkungen auf das heutige Vermögen ihrer Nachkommen. Die geschätzten Effekte sind robust auch nachdem für eine Reihe von Variablen auf regionaler und städtischer Ebene aus der Vorkriegszeit kontrolliert wird. In einer ergänzenden Mediationsanalyse werden in diesem Kapitel mögliche Wirkungskanäle wie Gesundheit, Bildung und Berufserfahrung untersucht, über die die Kriegszerstörung die Vermögensbildung beeinflusst haben könnte.

Das vierte Kapitel untersucht die Vermögensungleichheit zwischen Zugewanderten und Einheimischen in Deutschland. Insbesondere untersucht das Kapitel die Bedeutung von Merkmalsunterschieden für die Entwicklung der Vermögensunterschiede zwischen den beiden Gruppen. Auf Grundlage von Daten des SOEP zeigen die Ergebnisse dieses Kapitels, dass das Vermögensgefälle zwischen Einheimischen und Zugewanderten sehr groß und über den gesamten Analysezeitraum von 2002 bis 2017 relativ stabil ist. Eine anschließende Dekompositionsanalyse nutzt die Paneldimension der Daten aus und zeigt, dass Zugewanderte im erwerbsfähigen Alter hinsichtlich des Nettovermögens über die Zeit nicht wesentlich zur einheimischen Bevölkerung aufschließen können, da sie nicht über das ausreichende Einkommen verfügen und nicht im gleichen Maße von Erbschaften oder Schenkungen profitieren. Die Ergebnisse deuten außerdem darauf hin, dass vor allem einheimische Personen im Laufe der Zeit signifikante Teile ihres Vermögens aufzehren, übertragen oder verlieren, wodurch sich die Geschwindigkeit verringert, mit der die Vermögensungleichheit zwischen Zugewanderten und Einheimischen zunimmt.