Regional Disparities in the 21st-Century USA

Three Essays in Empirical Regional Economics

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Content

1 INTRODUCTION

In recent decades, disparities within and across regions have grown distinctively in the US (Berube et al., 2021; Peach & Adkisson, 2020; Thiede et al., 2020). They concern different areas, among others, income, standard of living, infrastructure, Internet connectedness, and public goods and services provision (Brookings, n.d.). These disparities might affect the economic development and quality of life of the regions and their inhabitants. For instance, within-region income inequality might provide economic incentives and role models, fostering innovation and incentivizing economic activity (Partridge & Weinstein, 2013; Royuela et al., 2019). However, it might also undermine social capital, increase crime, create sociopolitical unrest and ultimately impede future economic development (Glaeser et al., 2009; Metz & Burdina, 2018; Partridge & Weinstein, 2013; Royuela et al., 2019). Differences in public goods and services provision provide learning opportunities for best practices but might lead to people voting with one's feet, creating internal migration and welfare magnets (Cebula & Clark, 2013; Tiebout, 1956).

Therefore, it is crucial to understand regional disparities' causes, drivers, and consequences. The present cumulative dissertation aims at contributing to this effort by studying three distinct aspects of regional economic disparities in three empirical papers. The first paper analyzes the relationship between income and within-city income inequality in US metropolitan areas. The second one assesses whether local reliance on the oil and gas sector contributes to income inequality. The third one evaluates whether the expansion of Medicaid, a public health insurance scheme, induced internal migration within the US.

All three papers focus on regional disparities: either on within-region income inequality (papers 1 and 2) or across-state differences in the provision of a public service (paper 3). This dissertation then studies either causes of these disparities: income growth and oil and gas reliance (papers 1 and 2), or a consequence: internal migration (paper 3). In every case, the considered disparities play a significant role in regional economic conditions and development. The remainder of this introduction chapter presents the three papers and their contributions to the literature in more detail.

Paper 1 (chapter 2) examines the income-inequality relationship within US metropolitan areas over 1980—2016. It has been published as CEPIE Working Paper No. 01/21 and later on in *Investigaciones Regionales - Journal of Regional Research*.[1]

The spatial proximity of different income levels renders inequality particularly prominent in cities (Partridge & Weinstein, 2013). Economic growth might then either increase or decrease income inequality, depending on the circumstances (Autor & Dorn, 2013; Kuznets, 1955; Partridge & Weinstein, 2013; Rigby & Breau, 2008).

Still, comparatively little is known about this relationship in metropolitan areas, mainly due to data limitations. The few empirical studies obtain diverging results depending on the employed method (Castells-Quintana et al., 2015; Glaeser et al., 2009; Rodríguez-Pose & Tselios, 2009).

My paper assesses the income-inequality relationship within US metropolitan areas using cross-section and panel regression techniques based on American Community Survey and US Census data (Manson et al., 2017; Ruggles et al., 2018; US Census Bureau, n.d.-a). This procedure provides a consistent background to compare against the results for different techniques and years. Furthermore, the paper expands the time horizon for city-level studies on the income-inequality relationship up until 2016.

The paper finds that the income-inequality relationship changes over time. A higher per capita income level was associated with a lower inequality level in earlier years, but this association vanished later. For the 1980—2000 panel, per capita income increases are associated with decreases in inequality. In contrast, an increase in per capita income is associated with an increase in inequality in the 2006—2016 panel. The obtained results hint at polarization resulting from technological change, substituting middle-skill routine tasks, being responsible for this change in sign.

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[1] Seifert, F. (2021). The income-inequality relationship within US metropolitan areas 1980—2016 (No. 01/21; CEPIE Working Paper). TU Dresden. https://nbn-resolving.org/urn:nbn:de:bsz:14-qucosa2-740877.
Seifert, F. (2022). The Income-Inequality Relationship within US Metropolitan Areas 1980-2016. *Investigaciones Regionales - Journal of Regional Research*. https://investigacionesregionales.org/es/article/the-income-inequality-relationship-within-us-metropolitan-areas-1980-2016/.

Paper 2 (chapter 3) is concerned with the distributional effects of oil-and-gas-reliant economic development at the US local level in recent years (2012—2019). This period coincidences with the last years of the fracking boom, the subsequent bust, and slight recovery.

Which groups benefit from increased oil and gas reliance due to fracking remains an open question. On the one hand, many jobs in this sector require comparatively few skills but pay relatively well, benefitting low-skill, low-income workers, hence potentially decreasing income inequality (Jacobsen, 2019; Kearney & Wilson, 2018; Marchand, 2020; Upton & Yu, 2021). On the other hand, fracking generates considerable royalty income to land- and resource owners, a highly concentrated group, hence potentially increasing inequality (J. P. Brown et al., 2019; Hardy & Kelsey, 2015).

Empirical studies on the impact of local oil and gas reliance on inequality are scarce. The existing papers focus either on wages or on royalties. The first strand finds wage inequality decreases related to the fracking boom (Gittings & Roach, 2020; Jacobsen, 2019), while the second uncovers income inequality increases in Pennsylvania for the same boom (J. P. Brown et al., 2019; Hardy & Kelsey, 2015). Both literature strands do not assess the bust and its aftermath nor estimate asymmetric effects.

My paper assesses the relationship between changes in oil and gas reliance and income inequality at the US local level employing panel fixed-effects regressions and American Community Survey data (Ruggles et al., 2021; US Census Bureau, n.d.-b). Unlike previous papers, it does not study wages or royalties only, but income, which encompasses all these income sources and provides overall effects. Furthermore, it uses the income shares of different income groups to gauge the relative winners and losers of the recent oil and gas evolutions. To the best of my knowledge, the paper is also the first to estimate related asymmetric effects, that is, separate effects for increases and decreases in oil and gas reliance. Moreover, the study period of 2012—2019 covers the end of the fracking boom as well as the subsequent bust and stabilization period. Taken together, the paper's approach enables a more precise and comprehensive evaluation of the impact of changes in the local oil and gas reliance on income inequality in the 21st century US.

The paper uncovers a highly asymmetric effect of changes in oil and gas reliance on income inequality at the local level. Reliance decreases increase inequality while reliance increases do not decrease it. Top incomes, especially royalty recipients, are the most resilient group to oil and gas reliance decreases. They might also gain over-proportionally relative to the other income groups during reliance increases. In contrast, the paper cannot identify any oil and gas gains for low-income workers. Instead, they experience relative income losses during both increases and decreases in oil and gas reliance. Thus, the oil and gas sector reinforces the polarization tendencies in the US. This pattern might also explain socio-political tensions surrounding fracking projects.

Paper 3 (chapter 4) assesses whether the provision of public health care insurance can act as a welfare magnet by examining the inter-state migration response to the 2014 Affordable Care Act Medicaid expansion to low-income, working-age adults. A later version has been published in the *Review of Regional Research* and a precursor version in the 2021 Barcelona Workshop on Regional and Urban Economics conference program.[2]

In the wake of the Affordable Care Act, some US states expanded Medicaid eligibility while others did not, potentially inducing migration across state borders to obtain Medicaid. Much of this migration would arise in border regions (McKinnish, 2005). This phenomenon can strain border regions considerably, even if state-level effects are negligible.

Already Goodman (2017) considers Medicaid-induced border migration in 2014 by restricting its sample accordingly. However, this substantially decreases the available number of observations and results in statistical power issues, making it impossible to identify any border migration effects. To overcome these issues, my paper uses the border-versus-interior-regions approach suggested by McKinnish (2005, 2007). Adopting this approach, Alm & Enami (2017) could identify border migration effects for the 2006 Massachusetts Medicaid expansion.

[2] Seifert, F. (2022). The Affordable Care Act Medicaid expansion and interstate migration in border regions of US States. *Review of Regional Research, 42*(1), 49–74. https://doi.org/10.1007/s10037-022-00165-2.
Seifert, F. (2021). *The Affordable Care Act Medicaid expansion and inter-state migration in border regions of US states*. 2021 Barcelona Workshop on Regional and Urban Economics (Internal migrations and cross-border commuting), Barcelona, Spain. http://www.ub.edu/aqr/workshop/2021/wp-content/uploads/2021/5Seifert.pdf

The contribution of my paper is twofold. First, it is the first one that applies the border-versus-interior-regions approach to the 2014 Medicaid expansion and evaluates Medicaid migration effects for five states at once (Arkansas, Illinois, Iowa, Maryland, and New Mexico). Second, the analysis extends until 2017, adding three more years to Goodman's (2017) observation period. This allows both to increase the number of observations and study slightly longer-term effects of the Medicaid expansion on migration.

The paper can only identify a statistically significant, positive Medicaid migration effect for Arkansas. The other states exhibit insignificant migration effects, which sometimes even turn negative, indicating that no Medicaid migration occurs. The differing results across states could stem from statistical power issues but might also result from state peculiarities. However, even for Arkansas, the Medicaid migration effect seems manageable. If all additional migrants to Arkansas' border regions take up Medicaid, the number of Medicaid beneficiaries in these regions increases by less than 5 %. This increase probably does not impose a meaningful fiscal externality on regional budgets.

All the papers of my dissertation are single-authored by me. For the second paper about fracking and inequality, Andreas Büttner helped with assembling the data sets.

2 THE INCOME-INEQUALITY RELATIONSHIP WITHIN US METROPOLITAN AREAS 1980—2016[3]

Economic growth might both increase and decrease income inequality, also at the city level. This paper examines the income-inequality relationship within US metropolitan areas and finds that it changes over time. A higher per capita income level was associated with a lower inequality level in earlier years, but this association vanished later. For the 1980—2000 panel, per capita income increases are associated with decreases in inequality. In contrast, an increase in per capita income is associated with an increase in inequality in the 2006—2016 panel. The obtained results hint at polarization resulting from technological change substituting middle-skill routine tasks.

JEL classification: D31, O18, R11

Keywords: Inequality, Income, Metropolitan Areas, United States

2.1 Introduction

The income-inequality relationship has been a question of debate since the seminal work by Kuznets proposing the Kuznets curve: inequality first increases and then decreases with increasing national income (Kuznets, 1955). However, the income-inequality relationship at the city level does not necessarily follow the national one. Some channels from the national level, such as credit market mechanisms and redistribution policies, do not translate directly to the city level (Glaeser et al., 2009; Royuela et al., 2019). The latter is characterized by more in- and out-migration and less political room for maneuver than countries. Other factors level out at the national level, such as segregation. At the same time, income inequality is most visible and prominent in cities due to the spatial proximity of

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[3] A revised version of this chapter has been published as Seifert, F. (2022). The Income-Inequality Relationship within US Metropolitan Areas 1980-2016. *Investigaciones Regionales - Journal of Regional Research*. https://investigacionesregionales.org/es/article/the-income-inequality-relationship-within-us-metropolitan-areas-1980-2016/. A precursor version of this chapter has been published as CEPIE Working Paper: Seifert, F. (2021). The income-inequality relationship within US metropolitan areas 1980—2016 (No. 01/21; CEPIE Working Paper). TU Dresden. https://nbn-resolving.org/urn:nbn:de:bsz:14-qucosa2-740877.

different income levels (Partridge & Weinstein, 2013). Still, comparatively little is known about the income-inequality relationship at the city level, mainly due to data limitations. To close this gap, this study is going to assess this relationship within US metropolitan statistical areas (MSAs) from 1980—2016.

Few studies have analyzed the income-inequality relationship at this scale. For US MSAs, a negative income-inequality relationship has been found for 1980 and 2000: higher income levels are associated with lower inequality levels in MSAs based on cross-section regressions (Glaeser et al., 2009). For European regions, determinants of inequality at the regional level have been analyzed using annual panels over the 1990s and 2000s. These studies find a positive income-inequality relationship: income increases are associated with inequality increases (Castells-Quintana et al., 2015; Rodríguez-Pose & Tselios, 2009).

To further assess these opposing results, the present paper employs both cross-section and fixed effects (FEs) panel regression analyses for one geographic unit (MSAs) over several decades (1980—2016). This procedure provides a consistent background to compare against the results for different techniques and years. The analyses are based on two distinct data sets. The first is an annual panel over 2006—2016 using data from the American Community Surveys (ACSs) (Ruggles et al., 2018; US Census Bureau, n.d.-a). The second is a decennial panel over 1980—2000 using US Census data (Manson et al., 2017; Ruggles et al., 2018). Thereby, this paper expands the time horizon for local-level studies on the income-inequality relationship up until 2016.

This paper finds that the income-inequality relationship changes over time. A higher per capita income level was associated with a lower within-MSA inequality level in the earlier years. However, this association stopped being statistically significant in 2000 and remains insignificant for all the following years. For the 1980—2000 panel, per capita income increases are accordingly associated with inequality decreases. In contrast, an increase in per capita income is associated with an increase in inequality in the 2006—2016 panel. The income-inequality relationship changed its direction. These results are robust to the use of various inequality measures.

This change in sign might be due to differences in MSA delineations and time dimensions across the two panels. However, it could also originate from qualitative changes in the income-inequality relationship over time, potentially reflecting globalization and specialization. Notably, this study finds hints for polarization in line with the Autor & Dorn (2013) hypothesis of technological change substituting middle-skill routine tasks. However, these explanations cannot be completely distinguished with the data sets at hand. Thus, further research is required.

The following section reviews in greater detail the literature on how income and inequality are linked at the city level. Section 2.3 describes the data sources used and provides the empirical framework. Section 2.4 presents the cross-section results on the income-inequality relationship, while section 2.5 details the panel ones. Sections 2.6 and 2.7 present robustness checks using alternative inequality and income measures. Section 2.8 discusses potential reasons for the change in sign of the income-inequality relationship, while section 2.9 concludes, and 2.10 presents the appendix.

2.2 City-Level Links between Income and Inequality

Increases in mean income might both increase and decrease inequality depending on the circumstances. The Kuznets curve theory hypothesizes that the income-inequality relationship follows an inverted-U-shaped curve: inequality first increases and then decreases with increasing income (Kuznets, 1955). The N-shape hypothesis augmented this theory, stating that after a certain point, inequality starts increasing again with income for highly-developed economies (Castells-Quintana et al., 2015; Conceição & Galbraith, 2001).

Trade and labor market phenomena such as specialization, technological change substituting middle-skill routine tasks, deunionization, and flexible labor market regulations might lead to a positive income-inequality relationship. They might induce economic growth and increase inequality (Autor & Dorn, 2013; Partridge & Weinstein, 2013; Rigby & Breau, 2008). On the contrary, theories about residential segregation and disamenities such as crime and sociopolitical unrest predict a negative association: inequality decreases with income. For instance, residential segregation is associated with lower economic growth and higher inequality (Florida & Mellander, 2015; Li et al., 2013). Crime and sociopolitical unrest hinder economic growth while leading to and reinforcing inequality, resulting in vicious circles (Glaeser et al., 2009; Partridge & Weinstein, 2013).

These theories consider implicitly a medium- to long-run perspective where agents can adjust to the new situation. No explicitly short-run theory about the income-inequality relationship exists to the best of the author's knowledge. However, the relationship between income and inequality might differ between the short, medium, and long run. Transmission channels differ in their manifestation rapidity, with purely economic factors typically realizing faster than sociopolitical ones (Halter et al., 2014).

An MSA's population size, education levels, and the sectoral structure of its economy influence within-MSA inequality as well (Glaeser et al., 2009). Studies on the city size-inequality relationship typically identify a positive relationship: larger cities are ceteris paribus more unequal (Baum-Snow & Pavan, 2012; Glaeser et al., 2009). Education proxies for differences in skills and the degree of specialization, which leads to dispersed incomes (Glaeser et al., 2009). Higher education levels are associated with higher levels of inequality (Glaeser et al., 2009; Perugini & Martino, 2008). Shifts in the economy's sectoral structure might influence inequality due to differences in the associated income structure (Bolton & Breau, 2012; Castells-Quintana et al., 2015). Deindustrialization increases inequality (Bolton & Breau, 2012; Partridge & Weinstein, 2013).[4]

Several studies on MSA-level determinants of inequality exist, but they only employ cross-section regression analyses. A higher median income level is related to a lower level of inequality for 1980 and 2000 (Glaeser et al., 2009). Similarly, a higher average income level is associated with lower income inequality in 2010 when wage inequality is controlled for (Florida & Mellander, 2016). Higher per capita income growth appears to lead to lower end-of-period inequality in 1990 (Bhatta, 2001). However, cross-sections only capture the situation at one point in time and hence incorporate all the past influences leading to differences across MSAs (Forbes, 2000; Partridge, 2005). In this sense, they have rather a long-term perspective. This perspective contrasts with panel studies that assess how changes in income levels result in inequality changes for a given MSA (Atems, 2013; Partridge, 2005). Panel studies have rather a short- to medium-term perspective. Therefore,

[4] The demographic and racial composition of a MSA might influence inequality levels as well. However, related variables have proved not statistically significant in the regressions. They have been omitted from the presented analysis for clarity.

cross-section and panel results are not directly comparable (Atems, 2013). This study will use both techniques, cross-section and panel analyses, to gain a complete picture of the income-inequality relationship at hand.

Some studies of European regions have analyzed the income-inequality relationship in annual panel frameworks. Per capita income changes appear to be positively related to inequality changes for European NUTS I and II regions over 1995—2000 based on FEs, random effects, and GMM techniques (Rodríguez-Pose & Tselios, 2009). A U-shaped relationship is found over the 1993—2011 period for NUTS I regions but only when using the GINI as the inequality measure (Castells-Quintana et al., 2015). The latter interprets this as inequality having increased more in regions with higher relative increases in income, hence a positive income-inequality relationship as well (Castells-Quintana et al., 2015). However, these results are not directly transferable to US MSAs due to the differing labor market and institutional context, influencing the income-inequality relationship. Furthermore, MSAs provide smaller and more homogeneous regions than the NUTS regions. The present study's sample size is also larger, with up to 399 MSAs available for the analysis.

This paper expands the time horizon for studies on the income-inequality relationship by using data spanning from 1980 to 2016, although with gaps and changes in between as detailed in the next section. This setup enables assessing whether this relationship changed over time.

2.3 Data Sources and Empirical Framework

The study unit of this paper is the MSA.[5] MSAs are suitable units for studying regional economic activity and income inequality, as they encompass both the city core and suburbs related through commuting (Madden, 2000). MSAs form a functional economic unit encompassing production and consumption activities (Madden, 2000). Although the concept of MSAs has changed little over time, their county composition does change. A major change in MSA delineations occurred in 2013. Data within the 1990 MSA delineations are

[5] An MSA is a geographic entity delineated by the Office of Management and Budget for use by US statistical agencies. MSAs consist of the county or counties associated with at least one urbanized area of at least 50,000 inhabitants plus adjacent counties having a high degree of social and economic integration with the core as measured through commuting ties (US Census Bureau, n.d.-c).

available for 1980, 1990, and 2000. Data within the 2013 MSA delineations are available from 2006 onward.

This study employs hence two distinct data sets. One with decennial data for 1980—2000 and one with annual data from 2006—2016. For the 2006—2016 data set, the data stem from the 1-year ACSs collected by the US Census Bureau. The data for all the main variables were retrieved from FactFinder (US Census Bureau, n.d.-a). This data includes the pretax household income GINI at the MSA level. All ACS income variables are for the past 12 months prior to the interview moment, which is not publicly disclosed (IPUMS-USA, n.d.-c; Peters, 2013; US Census Bureau, 2009). This paper converts all original income variables into 2010 US-$ using the conversion factors provided by the Integrated Public Use Microdata Series USA (IPUMS) to adjust for inflation (IPUMS-USA, n.d.-c). Table 2.1 presents descriptive statistics. The resulting panel data set consists of 399 MSAs and 11 years. It is unbalanced due to slight further delineation changes over the time period.

For the 1980—2000 data set, the data stem from US Census via NHGIS and IPUMS (Manson et al., 2017; Ruggles et al., 2018). NHGIS offers aggregated data at the MSA level for all main variables except the GINI. The latter is calculated from IPUMS, which offers household-level data. There are drawbacks to using IPUMS data to calculate the GINI compared to variables provided by NHGIS or FactFinder directly. First, MSA populations are incompletely identified in the IPUMS data sets (IPUMS-USA, n.d.-a). Second, data confidentiality issues in smaller MSAs reduce the sample size. Third, household income is bottom-coded, and the reported incomes rounded in all years (IPUMS-USA, n.d.-c).[6] The correlation between the 2010 FactFinder and IPUMS-calculated GINIs is nonetheless over 0.9 and statistically significant at the 1 % level. The resulting unbalanced panel data set for 1980—2000 consists of 260 MSAs and 3 years. Table 2.2 presents descriptive statistics.

[6] A negative income is possible because both the Census and the ACSs include self-employment income from own businesses, that is, net income after business expenses. Furthermore, they include income from an estate or trust, interest, and dividends, which can be negative as well (IPUMS-USA, n.d.-c).

Table 2.1: Descriptive Statistics 2006—2016 Data Set

	Obs.	Mean	St. Dev.			Min	Max
			overall	between	within		
Gini	4,069	0.450	0.027	0.023	0.015	0.355	0.561
per capita income	4,069	24,738	4,423	4,223	1,175	12,572	51,661
mean household income	4,069	63,444	11,527	11,193	2,938	42,026	139,718
median household income	4,069	47,992	8,796	8,529	2,453	29,416	99,965

The statistics are for all observations of all MSAs over the entire 2006—2016 pooled together. The within standard deviation is within MSAs. *Source: FactFinder as well as own calculations*

Table 2.2: Descriptive Statistics 1980—2000 Data Set

	Obs.	Mean	St. Dev.			Min	Max
			overall	between	within		
Gini	735	0.416	0.033	0.024	0.024	0.333	0.532
per capita income	735	23,906	4,379	3,742	2,425	11,664	42,928
median household income	735	51,413	8,406	7,847	3,087	29,385	97,304

The statistics are for all observations of all MSAs over the entire 1980—2000 pooled together. The within standard deviation is within MSAs. *Source: NHGIS and IPUMS as well as own calculations*

This paper estimates the income-inequality relationship in cross-sections and panel frameworks using MSA and time FEs. The latter approach controls for time- and MSA-invariant variables. It also allows studying dynamics of change within short time series (Rodríguez-Pose & Tselios, 2009). However, FEs might lead to less variation than in cross-sectional studies as only within variation is considered (Royuela et al., 2019). This effect might be especially relevant for the 2006—2016 panel analysis, as inequality is believed to change only slowly over time (Glaeser et al., 2009; Royuela et al., 2019).

This paper regresses inequality on mean income in the same year. The empirical model is as follows:

$$g_{it} = \alpha + \beta y_{it} + \gamma \boldsymbol{X}_{it} + \mu_i + \tau_t + \varepsilon_{it} \qquad \text{(Equation 2.1)}$$

where g_{it} is a measure of inequality for MSA *i* at time *t*, y_{it} is an income measure (in logs), $\boldsymbol{X}_{it}$ is a vector of control variables, μ_i and τ_t are respectively MSA and time FEs, and ε_{it} is the error term. Standard errors are clustered at the MSA level. The cross-sections exclude the MSA and time FEs and are only estimated for a given *t*.

Controls for population, education, and sector employment shares are included to avoid confounding factors. They have been shown to influence within-city inequality, as previously discussed.[7]

Reverse causality between income and inequality constitutes an issue in these regressions. Income influences inequality, but inequality, in turn, affects income and income growth. Convincing instruments for income have not yet been proposed in this context. Therefore, the obtained coefficients have to be interpreted as associations rather than causal effects of income on inequality.

2.4 Cross-Section Results

This section presents cross-section results using both data sets. These results constitute a starting point to assess the income-inequality relationship across time. Table 2.3 presents the results. The first three columns report regression results for 2016, 2010, and 2006. These regressions use the 2013 MSA delineations. The data stems from the ACSs via FactFinder. The last three columns report regression results for 2000, 1990, and 1980. These regressions use the 1990 MSA delineations. The data stem from the Census via NHGIS and IPUMS.

For 2000, 2006, 2010, and 2016, the income coefficient is not statistically significant even at the 10 % level. Per capita income levels appear not to influence inequality levels in these years: neither positively nor negatively. The income coefficient is statistically significant at the 1 % level and negative in 1980 and 1990. Higher per capita income levels appear to be associated with reduced inequality levels in these years. A 1 % increase in per capita income involves, ceteris paribus, a decrease in the GINI by 0.0004 (1980) respectively 0.0005 points (1990) for a given MSA. This decrement is equivalent to a decrease by about 0.1 %

[7] Quadratic terms for the income variables were also included in the regressions to test for quadratic relationships. Their coefficients are not always statistically significant. If they are, the MSAs are concentrated on one side of the curve. Furthermore, they also exhibit the switch in sign of the income-inequality relationship across panels. (Results have been omitted for conciseness of the presentation but are available upon request.)

at the mean of the GINI. These negative coefficients correspond to the previous findings in the literature for MSAs.

Possible reasons for the divergence in results include differences in the database, the MSA delineations' changes, and qualitative changes in the income-inequality relationship over time. They are discussed more in detail in section 2.8.

Table 2.3: Cross-Section Results Regressing Inequality on Income

	(1) 2016 gini	(2) 2010 gini	(3) 2006 gini	(4) 2000 gini	(5) 1990 gini	(6) 1980 gini
ln(per capita income)	**-0.003**	**0.001**	**0.016**	**-0.032**	**-0.048*****	**-0.038*****
	(0.016)	**(0.014)**	**(0.015)**	**(0.022)**	**(0.018)**	**(0.014)**
ln(population)	0.000	-0.001	-0.001	0.001	0.002	0.001
	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)
baplus	0.001***	0.002***	0.002***	0.077**	0.101***	0.050**
	(0.000)	(0.000)	(0.000)	(0.036)	(0.038)	(0.023)
hsplus	-0.003***	-0.003***	-0.003***	-0.223***	-0.236***	-0.170***
	(0.000)	(0.000)	(0.000)	(0.063)	(0.049)	(0.028)
sagr	-0.002***	-0.002**	-0.002***	-0.249***	-0.140**	-0.097***
	(0.001)	(0.001)	(0.001)	(0.088)	(0.069)	(0.035)
sman	-0.001***	-0.001***	-0.001***	-0.126***	-0.070***	-0.129***
	(0.000)	(0.000)	(0.000)	(0.025)	(0.023)	(0.013)
constant	0.711***	0.676***	0.545***	0.969***	1.082***	0.916***
	(0.124)	(0.125)	(0.126)	(0.178)	(0.140)	(0.114)
MSAs	382	366	359	251	245	239
R^2	0.232	0.311	0.305	0.310	0.338	0.496

The first three columns report results for 2016, 2010, and 2006 respectively. They use 2013 MSA delineations and ACS data from FactFinder. The last three columns report results for 2000, 1990, and 1980 respectively. They use 1990 MSA delineations and Census data from NHGIS and IPUMS. baplus is the population share with a bachelor's degree or higher (in percent). hsplus is the population share with a high school diploma or higher (in percent). sagr is the share of persons 16 years and over employed in agriculture (in percent). sman is the share of persons 16 years and over employed in the manufacturing sector (in percent). Robust standard errors are in parentheses. * p<0.1, ** p<0.05, *** p<0.01; *Source: FactFinder, NHGIS and IPUMS as well as own calculations*

The control variables' coefficients are typically of the expected signs. However, population has surprisingly a statistically insignificant coefficient. Thus, the MSA size does not seem to influence the inequality level in the considered context. The coefficient of the share having a bachelor's degree or higher is statistically significant at the 5 % level and positive. Conversely, the coefficient of the share having a high school diploma or higher is statistically significantly negative. Thus, a better-educated population tends to be associated with higher inequality. These results correspond to the predictions and the findings obtained by Glaeser et al. (2009). The coefficients of both the share employed in agriculture and the share employed in manufacturing are statistically significant and negative. These results indicate that an economic structure based on these sectors is associated with less inequality than a service-based economy. The coefficient sizes of all the control variables are tiny.

The control variables do not drive the results as similar results are obtained when excluding them from the regression (available upon request). The negative income-inequality relationship persists for 1980 and 1990 when the GINI is only regressed on per capita income. The absolute coefficient size even increases slightly. The income coefficient is, in this case, also statistically significantly negative in 2000. For 2006, 2010, and 2016, the income coefficients remain not statistically significant as previously.

2.5 Panel Results

This section presents panel results using both data sets. They permit evaluating the impact of changes in per capita income on inequality and provide a comparison point to the cross-section results. Besides, they reduce the issue of unobserved heterogeneity in time-invariant MSA characteristics compared to cross-sections.

Table 2.4 presents the results. The first two columns show the annual 2006—2016 panel results. Column one uses *per capita* income while column two employs *mean household* income. The third column shows the decennial 1980—2000 panel results employing *per capita* income.[8]

[8] Mean household income is not available for 1980 and 1990. Its cross-section results for the remaining years are very similar to the per capita income ones (available upon request).

Table 2.4: Panel Results Regressing Inequality on Income

	2006—2016		1980—2000
	(1)	(2)	(3)
	gini	gini	gini
ln(per capita income)	**0.149*****		**-0.072*****
	(0.009)		**(0.017)**
ln(mean household income)		**0.135*****	
		(0.010)	
ln(population)	-0.020***	-0.023***	-0.013**
	(0.004)	(0.005)	(0.006)
baplus	-0.001**	-0.000*	0.150**
	(0.000)	(0.000)	(0.059)
hsplus	-0.001***	-0.001***	-0.043
	(0.000)	(0.000)	(0.035)
sagr	-0.001	-0.000	-0.000
	(0.000)	(0.000)	(0.072)
sman	-0.001***	-0.000**	-0.084***
	(0.000)	(0.000)	(0.027)
constant	-0.674***	-0.628***	1.302***
	(0.107)	(0.126)	(0.161)
MSA & Time FE	yes	yes	yes
N	4069	4069	735
MSAs	399	399	260
T	11	11	3
within-R^2	0.288	0.267	0.849

The first two columns report the 2006—2016 annual panel results, while the third column reports the 1980-2000 decennial panel results. baplus is the population share with a bachelor's degree or higher (in percent). hsplus is the population share with a high school diploma or higher (in percent). sagr is the share of persons 16 years and over employed in agriculture (in percent). sman is the share of persons 16 years and over employed in the manufacturing sector (in percent). Standard errors clustered at the MSA level in parentheses; * p<0.1, ** p<0.05, *** p<0.01; *Source: FactFinder resp. NHGIS and IPUMS as well as own calculations*

For the 2006—2016 panel, the income coefficient is statistically significant at the 1 % level and positive in both regressions. Increases in mean income appear to be associated with increases in inequality. A 1 % increase in per capita (mean household) income involves, ceteris paribus, an increase in the GINI by 0.0015 (0.0014) points for a given MSA. This increment is equivalent to an increase by about 0.3 % at the mean of the GINI. These results correspond to the ones obtained for European regions in annual panels over the 1990s and 2000s (Castells-Quintana et al., 2015; Rodríguez-Pose & Tselios, 2009).

For the 1980—2000 panel, the income coefficient is statistically significantly negative. Over these years, an increase in mean income seems to have decreased inequality. The absolute size of the income coefficient is smaller than previously. A 1 % increase in per capita income involves, ceteris paribus, a decrease in the GINI by 0.0007 points for a given MSA. This decrement is equivalent to a decrease by about 0.2 % at the mean of the GINI. However, the within-R^2 increases considerably from 0.29 before to now 0.85.

This divergence in the obtained income-inequality relationships might be due to similar reasons as the divergence in cross-section results for these data sets: differences in the database, the changes in the MSA delineations, and qualitative changes in the income-inequality relationship over time. Besides, the 2006—2016 panel is a yearly one, whereas the 1980—2000 panel is a decennial one. The 2006—2016 panel has observations from 11 time periods, whereas the 1980—2000 one only has three. Section 2.8 discusses these reasons more in detail.

The control variables' coefficients also change compared to the cross-section regressions. Population now exhibits a statistically significant negative coefficient. Thus, increases in MSA size seem to decrease inequality, whereas the population level per se does not affect an MSA's inequality level. The coefficient of the share having a bachelor's degree or higher is statistically significantly negative in the 2006—2016 panel but remains statistically significantly positive in the 1980—2000 panel. The coefficient of the share having a high school diploma or higher remains statistically significantly negative in the 2006—2016 panel but is not significant in the 1980—2000 one, providing for mixed results. The coefficient of the share employed in agriculture is not statistically significant in both panels, whereas the share employed in manufacturing remains statistically significantly negative in both panels. This coefficient indicates that deindustrialization is indeed associated with increasing inequality. The coefficient sizes of all the control variables remain tiny.

The obtained results are again robust to excluding all control variables from the regression (results available upon request). The positive income-inequality relationship in the 2006—2016 panel and the negative one in the 1980—2000 panel persist.

2.6 Employing Alternative Inequality Measures

The obtained opposing results for the two data sets might stem from a peculiarity of the GINI. Therefore, the previous regressions have been repeated with several other inequality measures to test the results' robustness. The robustness check sections only present results for the panel regressions as they most clearly exhibit the pattern of switching signs. Furthermore, they can be considered the more reliable results as they abstract from MSA-specific unobservable characteristics, which might bias the cross-section results.[9]

The calculated alternative inequality measures for within-MSA inequality are as follows:

- the GE(0) (Generalized Entropy index with a=0, that is, the mean log deviation),[10]
- the 90/10, 90/50, and 50/10 percentile ratios, and
- the s1, the income share of the top 1 % incomes in an MSA.

The GE(0) is an overall inequality measure as the GINI, providing a direct comparison point. The 90/10 percentile ratio is also an overall measure, but it excludes the extreme values at the top and the bottom of the income distribution. The 90/50 percentile ratio measures the inequality within top incomes, while the 50/10 percentile ratio measures the inequality within bottom incomes. The s1 indicates the evolution of the very top incomes compared to the rest.

The alternative inequality measures are calculated for both data sets from IPUMS as it offers household-level data. This procedure reduces the number of observations in the 2006—2016 data set to 2856 (from 4069 before) and in the 1980—2000 data set to 700 (735 before). The alternative inequality measures replace the GINI as the dependent variable in the regressions. Table 2.5 presents the 2006—2016 panel results, and table 2.6 the 1980—2000 panel ones.

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[9] Robustness checks have also been run for the cross-sections with similar results, indicating that their results are overall robust as well (appendix tables 2.9-2.13).

[10] Regressions have also been run for the GE(2) (Generalized Entropy index with a=2, that is, half the squared coefficient of variation). The obtained results are very similar to the GE(0) ones. The results have been omitted due to space considerations but are available upon request.

For the 2006—2016 panel, GE(0) shows a similar result to the GINI one: a statistically significant and positive income coefficient. The income coefficient is also statistically significantly positive for s1, while it is not statistically significant in the regressions with the percentile ratios. For the 1980—2000 panel, all income coefficients are statistically significant and negative as with the GINI except for the 50/10 percentile ratio and s1. In the latter cases, the coefficient is not statistically significant.

Table 2.5: Alternative Inequality Measures in the 2006—2016 Panel

	(1) gini	(2) ge0	(3) p90p10	(4) p90p50	(5) p50p10	(6) s1
ln(per capita income)	0.126***	0.037**	-1.097	0.027	-0.480	0.052***
	(0.011)	(0.016)	(0.961)	(0.100)	(0.317)	(0.007)
Controls	yes	yes	yes	yes	yes	yes
MSA & Time FEs	yes	yes	yes	yes	yes	yes
Constant	yes	yes	yes	yes	yes	yes
N	2856	2856	2856	2856	2856	2856
MSAs	293	293	293	293	293	293
T	11	11	11	11	11	11
within-R^2	0.289	0.248	0.062	0.141	0.028	0.090

Standard errors clustered at the MSA level in parentheses; * p<0.1, ** p<0.05, *** p<0.01; *Source: FactFinder and IPUMS as well as own calculations*

Table 2.6: Alternative Inequality Measures in the 1980—2000 Panel

	(1) gini	(2) ge0	(3) p90p10	(4) p90p50	(5) p50p10	(6) s1
ln(per capita income)	-0.073***	-0.102***	-3.811***	-0.744***	-0.277	0.012
	(0.018)	(0.023)	(0.927)	(0.113)	(0.284)	(0.014)
Controls	yes	yes	yes	yes	yes	yes
MSA & Time FEs	yes	yes	yes	yes	yes	yes
Constant	yes	yes	yes	yes	yes	yes
N	700	700	700	700	700	700
MSAs	254	254	254	254	254	254
T	3	3	3	3	3	3
within-R^2	0.857	0.852	0.349	0.770	0.140	0.756

Standard errors clustered at the MSA level in parentheses; * p<0.1, ** p<0.05, *** p<0.01; *Source: FactFinder and IPUMS as well as own calculations*

Overall, the regressions with alternative inequality measures confirm the results obtained with the GINI. The oppositional signs of the two panels' income coefficients appear again for the GE(0). The other measures exhibit mixed results. The latter corresponds to the expectations as they only consider parts of the income distribution.

The use of these alternative inequality measures also allows distinguishing between two hypotheses, which have been discussed for the rising inequality in the US: a rise in the top income share and polarization (Autor et al., 2006; Essletzbichler, 2015; Piketty & Saez, 2003). Per capita income has, on average, increased over the study period. Thus, both channels would result in a positive income coefficient for s1 and the 90/50 percentile ratio. Polarization would additionally lead to a negative coefficient for the 50/10 percentile ratio, while the 90/10 ratio should remain relatively unchanged. Notably, the 2006—2016 panel should exhibit this pattern as it captures the time of technological change substituting middle-skill routine tasks, leading to polarization.

The obtained results hint towards polarization but cannot substantiate this hypothesis unambiguously. The income coefficient for s1 is positive and significant in the new panel compared to being insignificant, albeit already positive, in the old panel. This result indicates that the per capita income increases disproportionally benefited the very top incomes. Concurrently, the 90/50 percentile ratio turned insignificantly positive from being significantly negative before. Thus, increasing top incomes played a role in the increasing inequality and switching signs of the income-inequality relationship across the panels. In addition, the 90/10 exhibits an insignificant coefficient in the newer panel, while being significantly negative before, consistent with polarization. However, the coefficient of the 50/10 percentile ratio is not significant but negative in both panels, which questions an income redistribution from the middle to the bottom incomes as suggested by the polarization hypothesis.

2.7 Employing Median Income

This section evaluates whether controlling for the gap between mean and median income also results in the changing signs in the income-inequality relationship across the two panels[11]. This exercise can also enlighten further whether technological change leading to polarization drives this change in sign.

Only including median income into the regressions would not produce meaningful results as its relationship with inequality is statistically predetermined to be negative, unlike mean income's. MSAs exhibit right-skewed income distributions of their inhabitants' incomes. An increase in the median income of a right-skewed income distribution decreases inequality by reducing the gap to the higher mean income. An increase in the mean income might lead in this context to higher inequality but not necessarily so, depending on which income group drives the increase.[12]

An increase in the difference between mean and median income as well as in the ratio of mean to median income should increase inequality in a right-skewed distribution. Thus, a positive coefficient is expected.

In the case of polarization, there should be less income mass around the middle of the income distribution. Thus, both per capita and median income increases should increase inequality for a given gap or ratio between the two. Conversely, if more income accumulates around the middle, income increases reduce inequality for a given gap or ratio between per capita and median income. If only the top incomes increase with rising income, then this would be captured by the per capita-median gap, and the single income measures' coefficients should not be significant on their own.

The gap, respectively, the ratio between per capita and median income have been added to the regressions to assess the polarization hypothesis. Table 2.7 presents the obtained results for the 2006—2016 panel and table 2.8 for the 1980—2000 one.

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[11] Appendix tables 2.14-2.17 report the cross-section results.

[12] This is confirmed empirically for both the cross-section and the panel analyses. On its own, median income always exhibits statistically significant negative coefficients. When both income types are included, the coefficients are as expected: always positive for per capita and always negative for median income. Results are available upon request.

Table 2.7: Mean and Median Income in the 2006—2016 Panel

	(1) gini	(2) gini	(3) gini	(4) gini
ln(per capita income)	0.049***		0.033***	
	(0.006)		(0.006)	
ln(median household income)		0.049***		0.031***
		(0.006)		(0.006)
difference	0.250***	0.299***		
	(0.007)	(0.008)		
ratio			2.851***	3.200***
			(0.075)	(0.086)
Controls	yes	yes	yes	yes
MSA & Time FEs	yes	yes	yes	yes
Constant	yes	yes	yes	yes
N	4069	4069	4069	4069
MSAs	399	399	399	399
within-R^2	0.618	0.618	0.618	0.618

Difference is the difference between per capita and median household income. Ratio is the ratio between per capita and median household income. Standard errors clustered at the MSA level in parentheses; * p<0.1, ** p<0.05, *** p<0.01; *Source: FactFinder as well as own calculations*

Table 2.8: Mean and Median Income in the 1980—2000 Panel

	(1) gini	(2) gini	(3) gini	(4) gini
ln(per capita income)	-0.070***		-0.081***	
	(0.016)		(0.016)	
ln(median household income)		-0.070***		-0.075***
		(0.016)		(0.015)
difference	0.133***	0.062**		
	(0.022)	(0.025)		
ratio			1.534***	0.655**
			(0.252)	(0.274)
Controls	yes	yes	yes	yes
MSA & Time FEs	yes	yes	yes	yes
Constant	yes	yes	yes	yes
N	735	735	735	735
MSAs	260	260	260	260
within-R^2	0.862	0.862	0.862	0.862

Difference is the difference between per capita and median household income. Ratio is the ratio between per capita and median household income. Standard errors clustered at the MSA level in parentheses; * p<0.1, ** p<0.05, *** p<0.01; *Source: NHGIS and IPUMS as well as own calculations*

Columns 1 and 2 include the difference between per capita and median household income. This difference exhibits statistically significant positive coefficients in both panels, as expected. Conditional on this difference, the per capita and median household income coefficients are statistically significantly positive in the 2006—2016 panel. In contrast, they are statistically significant and negative in the 1980—2000 panel.

Columns 3 and 4 include the ratio between per capita and median household income. This ratio also exhibits statistically significant positive coefficients in both panels, as expected. Conditional on this ratio, the income coefficients are again statistically significantly positive in the 2006—2016 panel and negative in the 1980—2000 panel.

These opposing signs of the income coefficients confirm the opposing signs in the baseline panel analyses. The mean income coefficient was previously also positive in the 2006—2016 panel and negative in the 1980—2000 panel.

The observed pattern strongly hints at polarization occurring in the 21st century. In contrast, middle incomes appear to have disproportionally benefited from income increases before, as demonstrated by the 1980—2000 panel results.

2.8 Reasons for the Change in the Income-Inequality Relationship

There are four possible reasons why the income-inequality relationship changes its sign across panels: differences in the database, changes in the MSA delineations, the different time gaps in the panels, and qualitative changes in the relationship.

First, changes in the underlying data and its aggregation between FactFinder and IPUMS might lead to differing results. The 1980—2000 panel is based on Census data, while the 2006—2016 one uses the ACS. However, both data products are produced by the US Census Bureau according to similar standards. Furthermore, the 2006—2016 results persist when using IPUMS-calculated inequality measures as shown in the alternative inequality measures regressions. Thus, the differences in the databases cannot account for the changing sign of the income-inequality relationship.

Second, MSA delineation changes result in different MSAs being considered across the two data sets. These changes lead to a clear difference in the number of MSAs available:

260 in the 1980—2000 data set versus 399 in the 2006—2016 one. The increase in sample size due to the number of MSAs alone is hence considerable. However, 260 MSAs are a large enough number of observations for regression analyses. Furthermore, the panel and cross-section results remain unchanged when restricting the 20006—2016 sample to only those MSAs that have already existed in 2000 (results available upon request). Besides, one can calculate both the GINI and mean household income from IPUMS for 2000 and 2010 for both MSA delineations. If one then regresses the GINI on the income, the obtained results are qualitatively the same regarding significance levels and signs (appendix table 2.18). Thus, delineation and sample size changes might play a role in the diverging results, but they appear unlikely to be the opposing results' sole cause.

Third, the time gaps and time dimensions of the panels differ. The 2006—2016 panel is an annual one with observations for 11 different years. The 1980—2000 panel is a decennial one with observations for only three years. Both might result in statistical issues. There might not be enough within-variation in the former for proper estimation, while the number of observations per MSA might be too small in the latter. The 10-year gap between observations in the latter results in a more medium-run perspective than the short-run one of the annual panel. Transmission channels differ in their manifestation rapidity, as discussed in section 2.2. Purely economic factors typically realize faster than sociopolitical ones (Halter et al., 2014). The former include trade and labor market phenomena, which also result in a positive income-inequality relationship. The latter comprise segregation, crime, and sociopolitical contrast and hence exactly those factors leading to a negative income-inequality relationship. Annual panel studies for European regions found likewise positive income-inequality relationships for 1994—2001 (Rodríguez-Pose & Tselios, 2009), respectively 1993—2011 (Castells-Quintana et al., 2015).

The 2006—2016 panel can be transformed into one with 5-year gaps and observations for three years (2006, 2011, and 2016). This approaches the time gap between observations to the one of the 1980—2000 panel and results in the same number of observation years (three). When regressing the GINI on income and the usual controls in this panel, the income coefficient remains statistically significant and positive for both per capita and mean household income. However, its size diminishes by about one-third (appendix table 2.19). A similar reduction is observed when basing the 5-year panel on the already existing

MSAs over 1980—2000. Thus, there appears to be something special about the 2006—2016 time period rather than the time gap between observations and the number of observed years resulting in the positive income-inequality relationship. However, the 10-year gaps cannot be simulated due to the 2006—2016 panel's limited time dimension.

Forth, the income-inequality relationship might have changed qualitatively over the years, especially between 2000 and 2006, according to the panel results. [13] The cross-sections also reflect this change. The negative income-inequality association stops already in 1990 and does not exist anymore for 2000 and further years. This timing corresponds to the sharp rise in inequality generally observed in the US in the 1980s and beyond (Piketty & Saez, 2003). This increase in inequality is also observed in the MSA-level data employed in the present study. Apparently, not only inequality increased, but its relationship with income changed as well. The changed sign of the income-inequality relationship also hints at economic growth having become less inclusive over the years.[14]

The influence of factors resulting in a negative income-inequality relationship might have decreased over time while the influence of those leading to a positive relationship increased.

Factors resulting in a negative income-inequality relationship include residential segregation, crime, and sociopolitical unrest, as detailed in section 2.2. Crime rates have indeed declined for several offenses since the 1980s (Asher, 2017), but residential segregation increased during the considered period (Bischoff & Reardon, 2014). Thus, the evidence for a decline in the ''negative'' factors is mixed.

Factors leading to a positive income-inequality relationship include specialization, technological change substituting middle-skill routine tasks, trade, deunionization, and flexible labor market regulations.

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[13] The European panel studies finding a positive income-inequality relationship analyzed the 1990s and 2000s (Castells-Quintana et al., 2015; Rodríguez-Pose & Tselios, 2009).

[14] The economic crisis of 2008 might also have influenced the income-inequality relationship. However, the change is already visible in the 2000 cross-section, where the income coefficient is insignificant for the first time. Furthermore, the positive income-inequality association also appears in a 2012—2016 panel, starting after the crisis years.

Trade and specialization have increased since the 1980s due to globalization and technological change substituting middle-skill routine tasks (Autor et al., 2006; Autor & Dorn, 2013; Rigby & Breau, 2008). Unionization rates declined over the last decades (Hu & Hanink, 2018). All these developments would strengthen a positive income-inequality relationship. Combined, they might have led to the observed change in the sign of the income-inequality relationship if the importance of these positive factors was stronger relative to the negative factors, especially residential segregation.

Given the available data, it is impossible to distinguish data-related issues neatly from qualitative changes in the income-inequality relationship. Thus, one cannot exclude that the differences in the data and the analysis setup are responsible for the observed change in sign of the relationship. This would require a longer annual panel over at least 20 years to evaluate results for panels of different lengths based on a single, consistent data set. Consequently, further research is required on this topic.

2.9 Conclusion

This paper analyzed the income-inequality relationship within MSAs using two data sets: a decennial one over 1980—2000 based on the Census and an annual one over 2006—2016 based on the ACS. These data sets enable studying the income-inequality relationship within MSAs over a more extended period than previously possible and employing both cross-section and panel regression techniques.

A higher per capita income level was still associated with a lower within-MSA inequality level in the earlier years. However, this association stopped being statistically significant in 2000 and remained so until 2016. For the 1980—2000 panel, per capita income increases are accordingly associated with inequality decreases. In the 2006—2016 panel, per capita income increases are associated with inequality increases. The income-inequality relationship changed its direction over time.

The main explanations for this change in sign consist of MSA delineation changes and different time dimensions in the panels as well as qualitative changes in the income-inequality relationship. The latter are probably due to polarization resulting from technological change substituting middle-skill routine tasks in line with Autor & Dorn (2013). However, these explanations cannot be completely distinguished with the data sets at hand.

Therefore, further research is required to solve this puzzle. On the one hand, studies using a more extended annual panel are needed to evaluate the income-inequality relationship in panels with different time dimensions and time gaps. On the other hand, more research on the transmission channels of the income-inequality relationship at the MSA levels might enlighten upon the influence of specific factors on this relationship in different periods.

2.10 Appendix

Table 2.9: Cross-Section Results regressing GE(0) on Income

	(1) 2016 ge0	(2) 2010 ge0	(3) 2006 ge0	(4) 2000 ge0	(5) 1990 ge0	(6) 1980 ge0
ln(per capita income)	-0.023 (0.031)	-0.035 (0.036)	-0.052 (0.036)	-0.071* (0.038)	-0.116*** (0.032)	-0.057*** (0.018)
Controls	yes	yes	yes	yes	yes	yes
Constant	yes	yes	yes	yes	yes	yes
MSAs	260	261	259	251	225	229
R^2	0.233	0.352	0.270	0.280	0.385	0.518

The first three columns report results for 2016, 2010, and 2006 respectively. They use 2013 MSA delineations and ACS data from FactFinder. The last three columns report results for 2000, 1990, and 1980 respectively. They use 1990 MSA delineations and Census data from NHGIS and IPUMS. The usual controls are included. Robust standard errors are in parentheses. * p<0.1, ** p<0.05, *** p<0.01; *Source: FactFinder, NHGIS and IPUMS as well as own calculations*

Table 2.10: Cross-Section Results regressing the 90/10 Percentile Ratio on Income

	(1) 2016 p90p10	(2) 2010 p90p10	(3) 2006 p90p10	(4) 2000 p90p10	(5) 1990 p90p10	(6) 1980 p90p10
ln(per capita income)	-7.982 (5.427)	-4.235** (2.091)	-4.711*** (1.787)	-3.089* (1.611)	-4.768*** (1.256)	-1.355 (0.824)
Controls	yes	yes	yes	yes	yes	yes
Constant	yes	yes	yes	yes	yes	yes
MSAs	260	261	259	251	225	229
R^2	0.115	0.323	0.286	0.246	0.351	0.400

The first three columns report results for 2016, 2010, and 2006 respectively. They use 2013 MSA delineations and ACS data from FactFinder. The last three columns report results for 2000, 1990, and 1980 respectively. They use 1990 MSA delineations and Census data from NHGIS and IPUMS. The usual controls are included. Robust standard errors are in parentheses. * p<0.1, ** p<0.05, *** p<0.01; *Source: FactFinder, NHGIS and IPUMS as well as own calculations*

Table 2.11: Cross-Section Results regressing the 90/50 Percentile Ratio on Income

	(1) 2016 p90p50	(2) 2010 p90p50	(3) 2006 p90p50	(4) 2000 p90p50	(5) 1990 p90p50	(6) 1980 p90p50
ln(per capita income)	0.075	-0.085	-0.144	0.002	-0.568^{***}	-0.358^{***}
	(0.167)	(0.187)	(0.183)	(0.214)	(0.121)	(0.080)
Controls	yes	yes	yes	yes	yes	yes
Constant	yes	yes	yes	yes	yes	yes
MSAs	260	261	259	251	225	229
R^2	0.245	0.322	0.330	0.376	0.537	0.574

The first three columns report results for 2016, 2010, and 2006 respectively. They use 2013 MSA delineations and ACS data from FactFinder. The last three columns report results for 2000, 1990, and 1980 respectively. They use 1990 MSA delineations and Census data from NHGIS and IPUMS. The usual controls are included. Robust standard errors are in parentheses. * p<0.1, ** p<0.05, *** p<0.01; *Source: FactFinder, NHGIS and IPUMS as well as own calculations*

Table 2.12: Cross-Section Results regressing the 50/10 Percentile Ratio on Income

	(1) 2016 p50p10	(2) 2010 p50p10	(3) 2006 p50p10	(4) 2000 p50p10	(5) 1990 p50p10	(6) 1980 p50p10
ln(per capita income)	-2.690	-1.223^{**}	-1.424^{***}	-1.000^{**}	-0.981^{***}	0.066
	(1.648)	(0.543)	(0.462)	(0.406)	(0.353)	(0.315)
Controls	yes	yes	yes	yes	yes	yes
Constant	yes	yes	yes	yes	yes	yes
MSAs	260	261	259	251	225	229
R^2	0.107	0.241	0.228	0.145	0.216	0.195

The first three columns report results for 2016, 2010, and 2006 respectively. They use 2013 MSA delineations and ACS data from FactFinder. The last three columns report results for 2000, 1990, and 1980 respectively. They use 1990 MSA delineations and Census data from NHGIS and IPUMS. The usual controls are included. Robust standard errors are in parentheses. * p<0.1, ** p<0.05, *** p<0.01; *Source: FactFinder, NHGIS and IPUMS as well as own calculations*

Table 2.13: Cross-Section Results regressing the Top 1 % Income Share on Income

	(1) 2016 s1	(2) 2010 s1	(3) 2006 s1	(4) 2000 s1	(5) 1990 s1	(6) 1980 s1
ln(per capita income)	-0.013*	0.008	-0.000	-0.026***	-0.013***	0.051***
	(0.007)	(0.006)	(0.007)	(0.004)	(0.005)	(0.010)
Controls	yes	yes	yes	yes	yes	yes
Constant	yes	yes	yes	yes	yes	yes
MSAs	260	261	259	251	225	229
R^2	0.144	0.100	0.076	0.442	0.274	0.442

The first three columns report results for 2016, 2010, and 2006 respectively. They use 2013 MSA delineations and ACS data from FactFinder. The last three columns report results for 2000, 1990, and 1980 respectively. They use 1990 MSA delineations and Census data from NHGIS and IPUMS. The regressions include the usual controls. Robust standard errors are in parentheses. * p<0.1, ** p<0.05, *** p<0.01; *Source: FactFinder, NHGIS and IPUMS as well as own calculations*

Table 2.14: Cross-Section Results for Per Capita and Median Income Difference (I)

	(1) 2016 gini	(2) 2010 gini	(3) 2006 gini	(4) 2000 gini	(5) 1990 gini	(6) 1980 gini
ln(per capita income)	-0.032***	-0.039***	-0.028**	-0.067***	-0.079***	-0.064***
	(0.011)	(0.010)	(0.011)	(0.012)	(0.010)	(0.010)
difference	0.203***	0.194***	0.204***	0.226***	0.179***	0.139***
	(0.013)	(0.011)	(0.013)	(0.016)	(0.015)	(0.014)
Controls	yes	yes	yes	yes	yes	yes
Constant	yes	yes	yes	yes	yes	yes
MSAs	382	366	359	251	245	239
R^2	0.654	0.682	0.679	0.688	0.550	0.658

The first three columns report results for 2016, 2010, and 2006 respectively. They use 2013 MSA delineations and ACS data from FactFinder. The last three columns report results for 2000, 1990, and 1980 respectively. They use 1990 MSA delineations and Census data from NHGIS and IPUMS. Difference is the difference between per capita and median household income. The regressions include the usual controls. Robust standard errors are in parentheses. * p<0.1, ** p<0.05, *** p<0.01; *Source: FactFinder, NHGIS and IPUMS as well as own calculations*

Table 2.15: Cross-Section Results for Per Capita and Median Income Difference (II)

	(1) 2016 gini	(2) 2010 gini	(3) 2006 gini	(4) 2000 gini	(5) 1990 gini	(6) 1980 gini
ln(median household income)	-0.032***	-0.039***	-0.028**	-0.067***	-0.079***	-0.064***
	(0.011)	(0.010)	(0.011)	(0.012)	(0.010)	(0.010)
difference	0.171***	0.155***	0.175***	0.159***	0.101***	0.075***
	(0.017)	(0.016)	(0.017)	(0.019)	(0.013)	(0.015)
Controls	yes	yes	yes	yes	yes	yes
Constant	yes	yes	yes	yes	yes	yes
MSAs	382	366	359	251	245	239
R^2	0.654	0.682	0.679	0.688	0.550	0.658

The first three columns report results for 2016, 2010, and 2006 respectively. They use 2013 MSA delineations and ACS data from FactFinder. The last three columns report results for 2000, 1990, and 1980 respectively. They use 1990 MSA delineations and Census data from NHGIS and IPUMS. Difference is the difference between per capita and median household income. The regressions include the usual controls. Robust standard errors are in parentheses. * p<0.1, ** p<0.05, *** p<0.01; *Source: FactFinder, NHGIS and IPUMS as well as own calculations*

Table 2.16: Cross-Section Results for Per Capita and Median Income Ratio (I)

	(1) 2016 gini	(2) 2010 gini	(3) 2006 gini	(4) 2000 gini	(5) 1990 gini	(6) 1980 gini
ln(per capita income)	-0.045***	-0.052***	-0.042***	-0.083***	-0.092***	-0.076***
	(0.011)	(0.011)	(0.011)	(0.012)	(0.010)	(0.011)
ratio	2.339***	2.227***	2.341***	2.638***	2.089***	1.624***
	(0.146)	(0.124)	(0.147)	(0.186)	(0.178)	(0.169)
Controls	yes	yes	yes	yes	yes	yes
Constant	yes	yes	yes	yes	yes	yes
MSAs	382	366	359	251	245	239
R^2	0.655	0.682	0.680	0.692	0.551	0.658

The first three columns report results for 2016, 2010, and 2006 respectively. They use 2013 MSA delineations and ACS data from FactFinder. The last three columns report results for 2000, 1990, and 1980 respectively. They use 1990 MSA delineations and Census data from NHGIS and IPUMS. Ratio is the ratio between per capita and median household income. The regressions include the usual controls. Robust standard errors are in parentheses. * p<0.1, ** p<0.05, *** p<0.01; *Source: FactFinder, NHGIS and IPUMS as well as own calculations*

Table 2.17: Cross-Section Results for Per Capita and Median Income Ratio (II)

	(1) 2016 gini	(2) 2010 gini	(3) 2006 gini	(4) 2000 gini	(5) 1990 gini	(6) 1980 gini
ln(median household income)	-0.043***	-0.049***	-0.039***	-0.077***	-0.085***	-0.070***
	(0.010)	(0.010)	(0.011)	(0.011)	(0.010)	(0.010)
ratio	1.849***	1.663***	1.888***	1.741***	1.095***	0.805***
	(0.181)	(0.166)	(0.185)	(0.204)	(0.140)	(0.164)
Controls	yes	yes	yes	yes	yes	yes
Constant	yes	yes	yes	yes	yes	yes
MSAs	382	366	359	251	245	239
R^2	0.655	0.682	0.680	0.691	0.550	0.658

The first three columns report results for 2016, 2010, and 2006 respectively. They use 2013 MSA delineations and ACS data from FactFinder. The last three columns report results for 2000, 1990, and 1980 respectively. They use 1990 MSA delineations and Census data from NHGIS and IPUMS. Ratio is the ratio between per capita and median household income. The regressions include the usual controls. Robust standard errors are in parentheses. * p<0.1, ** p<0.05, *** p<0.01; *Source: FactFinder, NHGIS and IPUMS as well as own calculations*

Table 2.18: Cross-Section Results for 2000 and 2010 with Different PUMA Delineations

	(4) 2010 old IPUMS	(5) 2010 new IPUMS	(6) 2010 aggregated	(1) 2000 aggregated	(2) 2000 old IPUMS	(3) 2000 new IPUMS
	gini	gini	gini	gini	gini	gini
ln(mean household income)	0.015	0.015	0.014	-0.054***	-0.000	-0.019
	(0.012)	(0.011)	(0.010)	(0.009)	(0.000)	(0.013)
Controls	no	no	no	no	no	no
Constant	yes	yes	yes	yes	yes	yes
MSAs	283	261	366	251	283	258
R^2	0.011	0.010	0.007	0.115	0.000	0.015

The table reports results for regressing the GINI on log mean household income without any control variables included. The first three columns report results for 2010. The regression of the first column uses the 1990 MSA delineations together with mean household income calculated from IPUMS micro data. The regression of the second column also calculates from IPUMS but uses the 2013 MSA delineations. The regression of the third column then uses the aggregated FactFinder data for mean household income and the 2013 MSA delineations as in the main regressions. The last three columns report results for 2000. The regression of the fourth column uses the aggregated Census NHGIS data for mean household income and the 1990 MSA delineations as in the main regressions. The regression of the fifth column also uses the 1990 MSA delineations but calculates mean household income from IPUMS. The regression of the third column then uses the 2013 MSA delineations while calculating the mean household income from IPIMS. Robust standard errors are in parentheses. * p<0.1, ** p<0.05, *** p<0.01; *Source: FactFinder, NHGIS and IPUMS as well as own calculations*

Table 2.19: Results for a 5-Year-period Panel 2006—2016

	(1)	(2)
	gini	gini
ln(per capita income)	0.099^{***}	
	(0.013)	
ln(mean household income)		0.094^{***}
		(0.014)
Controls	yes	yes
MSA & Time FE	yes	yes
Constant	yes	yes
N	1106	1106
MSAs	397	397
T	3	3
within-R^2	0.330	0.322

The table reports regression results for a 5-year-period subpanel of the 2006—2016 one. Thus, it includes observations from 2006, 2011, and 2016 only. The regressions include the usual controls. Standard errors clustered at the MSA level in parentheses; * p<0.1, ** p<0.05, *** p<0.01; *Source: FactFinder as well as own calculations*

3 THE ASYMMETRIC EFFECT OF US LOCAL OIL AND GAS RELIANCE ON INCOME INEQUALITY IN THE FRACKING ERA

Oil and gas production might affect the incomes of all sorts of individuals, from low-skill workers to royalty receiving land- and resource owners, leaving its overall effect on income inequality uncertain. Therefore, the present paper studies the effect of changes in local-level oil and gas sector reliance on income inequality in the US. Employing panel fixed effects regressions over 2012—2019, it uncovers a highly asymmetric effect of oil and gas reliance. Inequality increases during oil and gas reliance decreases, while reliance increases do not decrease inequality. Bottom incomes experience relative income losses during both increases and decreases in oil and gas reliance, while top-most incomes experience relative income gains. Thus, the oil and gas sector reinforces ongoing income polarization tendencies, potentially explaining socio-political tensions surrounding related projects.

JEL classification: D31, Q32, Q33, R11

Keywords: inequality, fracking, oil boom, local effects, USA

3.1 Introduction

Fracking, a method to extract oil and gas from rocks by a pressurized liquid, became economically viable at the beginning of the 21st century. This technical development allowed oil and gas exploitation outside of conventional reservoirs, creating an oil and gas boom in the US. This boom increased employment, wages, royalties, and per capita income (Allcott & Keniston, 2018; Feyrer et al., 2017; Gittings & Roach, 2020; Hardy & Kelsey, 2015; Jacobsen, 2019; Marchand & Weber, 2018; Weber, 2012). However, fracking is also controversial due to associated increases in noise, pollution, and traffic (K. J. Black et al., 2021; US Environmental Protection Agency, 2013). Several US states have banned fracking while others promote it (J. P. Brown et al., 2019).

Which groups benefit from fracking remains an open question. On the one hand, many jobs in this sector require comparatively few skills but pay relatively well, benefitting low-skill, low-income workers (Jacobsen, 2019; Kearney & Wilson, 2018; Marchand, 2020; Upton & Yu, 2021). On the other hand, fracking generates considerable royalty income to land- and resource owners, a highly concentrated group (J. P. Brown et al., 2019; Hardy & Kelsey, 2015). Depending on who benefits more, income inequality would decrease or increase in concerned areas with increasing reliance on the oil and gas sector.

The local income distributional effects of oil and gas reliance are relevant for several reasons, even in the short run. An unequal distribution of the immediate fracking costs and benefits might explain some of the socio-political tension around fracking projects and some groups' (un)willingness to take them up (Ulrich-Schad et al., 2020). Inequality increases also increase crime (Glaeser et al., 2009; Metz & Burdina, 2018). Furthermore, policymakers might want to pursue income-smoothing and redistributive policies if oil and gas income fluctuations are considered too extreme or divergent from an inclusive economic growth path. Short-run inequality effects of changes in oil and gas reliance can accumulate, especially if they are asymmetric and hence potentially not offset over time. If this leads to high levels of inequality, it undermines social capital and hinders future economic growth, especially in rural areas (Fallah & Partridge, 2006).

Therefore, the present paper assesses the relationship between changes in the reliance on the oil and gas sector and income inequality at the local level in the US over 2012—2019. Unlike previous papers, it does not study wages or royalties only, but overall income, which encompasses all income sources and provides overall effects. Furthermore, it uses the income shares of different income groups to gauge the relative winners and losers of the recent oil and gas evolutions. To the best of my knowledge, the present paper is also the first to estimate related asymmetric effects, that is, separate effects for increases and decreases in oil and gas reliance. Moreover, the study period of 2012—2019 covers the end of the fracking boom as well as the subsequent bust and stabilization period (appendix figure 3.3), in contrast to most fracking papers, which only study the boom. Taken together, the present paper's approach enables a more precise and comprehensive evaluation of the impact of changes in the local oil and gas reliance on income inequality in the 21st century US.

The present paper's panel regressions estimate the symmetric and asymmetric effects of changes in local oil and gas reliance, measured by the related employment share, on local income inequality accounting for area and year fixed effects (FEs). Local income inequality is measured by the pre-tax Gini and the income share of each income quartile. The data stems from the American Community Surveys (ACSs) (Ruggles et al., 2021; US Census Bureau, n.d.-b). A robustness check runs symmetric instrumental variable (IV) regressions. Their instrument consists of the interaction of the surface percentage of an area over a shale basin and either the US-level resource sector employment share, the annual average US gas price, or the world market oil price.

The present paper uncovers a highly asymmetric effect of changes in oil and gas reliance on income inequality at the local level. Oil and gas reliance and inequality are negatively related, especially when reliance decreases. Reliance decreases increase inequality while reliance increases do not decrease it. The effect size is small: a one-percentage-point decrease in the resource sector employment share increases the Gini by 0.003 points. However, the effect's significance is remarkable given this sector's small size in the overall economy, even in highly oil-and-gas-reliant areas.

The income shares of the income quartiles also exhibit highly asymmetric effects of changes in oil and gas reliance. Top incomes, especially royalty recipients, are the most resilient group to oil and gas reliance decreases. They might also gain over-proportionally relative to the other income groups during reliance increases. In contrast, the present paper cannot identify any over-proportional oil and gas gains for low-income workers. Instead, they experience relative income losses during both increases and decreases in oil and gas reliance, together with the bottom-most incomes. Thus, the oil and gas sector reinforces the polarization tendencies in the US. This pattern might also explain socio-political tensions surrounding fracking projects.

The remainder of the introduction section gives a brief overview of the related literature. The following section presents the empirical strategy and data employed. Section 3.3 presents the main results. Section 3.4 discusses IV estimation results, while section 3.5 performs further robustness checks. Finally, section 3.6 concludes, and 3.7 presents the appendix.

Changes in oil and gas reliance might affect the local income distribution differently depending on the underlying channels at work. Resource booms, including oil and gas ones, can be seen as a particular form of labor demand shocks, and the resource industry generally pays more than other local industries (Marchand & Weber, 2018). If the new resource workers were previously located towards the bottom of the income distribution, inequality would decrease (Fleming & Measham, 2015). The latter appears likely as many resource-based jobs require comparatively few skills, consisting of routine manual tasks, but pay relatively well (Fleming & Measham, 2015; Marchand, 2020; Upton & Yu, 2021). Thus, the relationship between changes in oil and gas reliance and inequality would be negative: inequality decreasing with increasing reliance. This relationship corresponds to the first hypothesis tested in this paper: low-income workers over-proportionally benefit from the oil and gas sector compared to other groups (hypothesis 1).

On the other hand, fracking provides royalty incomes to land- and resource owners. These owners represent a tiny portion of the population, resulting in royalty incomes being highly concentrated at the top of the income distribution (J. P. Brown et al., 2019; Hardy & Kelsey, 2015; Jong & Craig, 2020). Thus, the relationship between changes in oil and gas reliance and inequality would be positive: inequality increases with increasing reliance. This relationship corresponds to the second hypothesis tested in this paper: royalty recipients over-proportionally benefit from the oil and gas sector compared to other groups (hypothesis 2).

Income drainage from the oil-and-gas-reliant region is a non-negligible factor in this context. Much of the economic benefits of fracking have accrued to non-residents because of commuting and absentee land-, resource and company owners (Feyrer et al., 2017; Gittings & Roach, 2020; Kelsey et al., 2016; Kim & Johnson, 2020). The incomes of commuters are not incorporated into the local inequality measures based on residence. The same applies to royalties and profits occurring to owners residing outside the oil and gas area. This income drainage dilutes the oil and gas reliance effect on local income inequality, rendering it inexistent in the extreme.

Empirical studies on this effect are scarce. However, an oil and gas boom slightly increased income inequality within oil-and-gas-reliant areas in Western Canada in 2005 compared to unconcerned areas and the pre-boom level in 1995 (Marchand, 2015).

Most often, the empirical studies focus on wages only. This literature strand finds no or a negative effect of increased oil and gas production due to the fracking boom on wage inequality: increased production reduces wage inequality (Gittings & Roach, 2020; Jacobsen, 2019). These findings align with hypothesis 1 of the oil and gas sector particularly benefitting low-income workers. However, these studies only analyze the fracking boom, stopping at the latest in 2014. Regarding conventional oil over 1969—2000, increasing oil prices (booms) do not affect the earnings distribution in oil-reliant areas: boom gains are shared equally (Basso, 2017). However, oil price decreases (busts) disproportionately depress the lower end of the earnings distribution, increasing inequality (Basso, 2017). Thus, an asymmetric effect of changes in oil prices emerges, which might also apply to oil and gas reliance. This pattern translates to a third hypothesis tested in this paper: changes in oil and gas reliance asymmetrically affect the income distribution (hypothesis 3).

A few empirical studies of the fracking boom effect on royalty incomes exist, especially for Pennsylvania. Based on tax returns, royalty incomes increased more than overall income in fracking areas over 2007—2010, overriding any wage gains (Hardy & Kelsey, 2015). As land and resource ownership is highly concentrated, royalties from fracking are heavily concentrated among a small percentage of the population: the top income group (J. P. Brown et al., 2019; Hardy & Kelsey, 2015). Consequently, income inequality increases when oil and gas reliance increases with fracking, in line with hypothesis 2. However, this small strand of literature does not perform any regression analysis, potentially leading to omitted variable bias. Furthermore, these papers again only study the fracking boom.

3.2 Empirical Strategy and Data

This paper estimates the effect of changes in oil and gas reliance on changes in inequality via panel frameworks employing area and year FEs over 2012—2019. This approach allows studying dynamics of change within short time series and analyzing asymmetric effects (Allison, 2019; Rodríguez-Pose & Tselios, 2009).

The present paper regresses inequality on oil and gas reliance in the same year. The symmetric FEs regression model is as follows:

$$g_{it} = \alpha + \beta reliance_{it} + \gamma \boldsymbol{X}_{it} + \mu_i + \tau_t + \varepsilon_{it} \quad \text{(Equation 3.1)}$$

where g_{it} is the inequality measure: either the Gini or the income share[15] of a specific income group of area *i* at time *t*, $reliance_{it}$ is the area-level oil and gas reliance measured by the related employment share, $\boldsymbol{X}_{it}$ is a vector of control variables detailed below, μ_i and τ_t are respectively area and year FEs, and ε_{it} is the error term. Standard errors are clustered at the state level as states can decide upon fracking regulations and set institutional frameworks (Negro, 2012; Winters et al., 2020).

Asymmetric effects are estimated by FEs regressions of the dependent variable on the cumulative sums of the independent variables' positive and negative changes, following Allison (2019). Thus, the effect of increases respectively decreases in oil and gas reliance is estimated by adjusting the symmetric FEs-model accordingly. This approach is not the same as separating the sample into boom and bust period subsamples according to the world oil price.[16] Increases in oil and gas reliance are more frequent during booms, but they can also occur during busts, depending on local developments.

The following area-level control variables have been added to the regressions as they influence local-level inequality: per capita income, population size, demographic structure (share of the population aged 25 years and younger, respectively 65 years and older), educational attainment (share of the population over 25 years with at least a high school diploma, respectively, a bachelor's degree), and racial composition (ethnic diversity index) (Baum-Snow & Pavan, 2013; Castells-Quintana et al., 2015; Florida & Mellander, 2016;

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[15] Note that the increase of one income share necessarily implies the decrease of at least one other income share. Therefore, income shares do not measure absolute, but relative, income gains and losses of the considered income group.

[16] When running the regressions on boom (2012—2014) and bust (2015—2019) subsamples, the statistical significance of the estimated coefficients is reduced, probably due to the small number of years included into each subsample. Still, the full sample results are confirmed for both periods as the coefficients keep their respective signs.

Glaeser et al., 2009; Peters, 2012; Seifert, 2021). The area FEs control for initial and persistent differences in inequality levels across areas (Fallah & Partridge, 2006; Glaeser et al., 2009), while the year FEs control for changes in inequality over time which are common across all areas.

Reverse causality might arise between oil and gas reliance and inequality. Higher inequality might imply lesser political influence of lower-income individuals and more for higher-income ones. These groups might be particularly in favor or against fracking, depending on the perceived benefits, resulting in the (non) implementation of oil and gas projects. The resulting reverse causality potentially threatens the proposed causal identification strategy. Therefore, symmetric[17] regressions instrumenting oil and gas reliance have been run (cf. section 3.4). These regressions confirm the obtained conventional results, indicating the latters' robustness. Thus, the reverse causality channel appears negligible in the context of the present study.

The socioeconomic data employed in the present paper stems from the 1-year ACSs collected by the US Census Bureau (US Census Bureau, n.d.-b). The ACS is a survey of a 1 % sample of the US population. It is representative of any place with a population larger than 65,000 (US Census Bureau, 2018).

The smallest geographic unit in the ACS with annual data for all areas are Public Use Microdata Areas (PUMAs). A PUMA is a statistical entity consisting of one or more counties or census tracts combining a population of at least 100,000 (IPUMS-USA, n.d.-b). No PUMA crosses state boundaries. The PUMA delineations changed between 2011 and 2012 to ensure further compliance with the PUMA definition cited above (IPUMS-USA, n.d.-b). This study uses only the newer PUMA delineations to ensure consistency and covers the period 2012—2019. Furthermore, it limits itself to the contiguous US states plus Washington DC. The total number of PUMAs in the sample is 2,332. The resulting panel data set is slightly unbalanced due to sporadically missing observations.

[17] Asymmetric IV regressions are unfortunately not possible due to the IV construction modalities.

Table 3.1: Descriptive Statistics 2012—2019

Variable	Observations	Mean	Std. Dev.	Min	Max
Household-income Gini	18,238	0.448	0.043	0.298	0.671
Individual-income Gini	18,238	0.568	0.041	0.425	0.728
Household-income GE(2)	18,238	0.478	0.148	0.160	1.872
Q1's (bottom quartile's) income share (%)	18,238	1.02	0.703	-0.42	6.41
Q2's (2^{nd} quartile's) income share (%)	18,238	10.72	2.105	1.16	20.03
Q3's (3^{rd} quartile's) income share (%)	18,238	25.67	2.355	14.89	36.52
Q4's (top quartile's) income share (%)	18,238	64.94	3.940	50.61	81.76
Resource sector employment share (%)	18,238	0.574	1.596	0	21.27
ln(per capita income)	18,238	10.34	0.329	9.19	11.79
ln(population)	18,238	11.81	0.197	11.39	12.61
Share under 25 years (%)	18,238	32.45	4.864	11.01	61.10
Share over 64 years (%)	18,238	15.09	4.567	3.00	54.80
Share high school diploma and higher (%)	18,238	87.33	7.678	36.80	99.10
Share bachelor's degree and higher (%)	18,238	30.99	14.57	2.60	88.90
Ethnic diversity index	18,238	0.367	0.178	0.033	0.788

The reported values are for pooling the data over the whole study period (2012—2019). The sample is restricted to the 48 contiguous US states plus Washington DC. GE(2) is the Generalized Entropy Index with a=2, that is, half the square coefficient of variation. Per capita income is in 2019 US-$. Individual and household income can be negative in the ACS, for instance, in the case of business losses. If this applies to many individuals or implies large amounts, the bottom quartile's income share can also be negative.

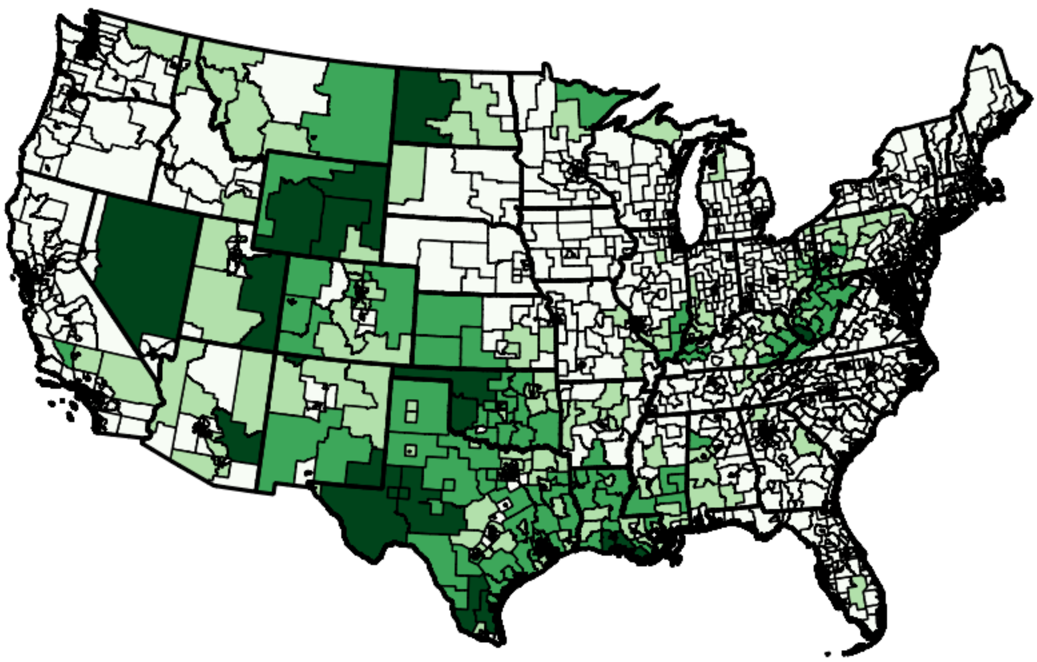
The ACSs provide the pre-tax household-income Gini at the PUMA level. It also supplies data on employment by industry, population, age, educational attainment, ethnic composition, and per capita income in the past 12 months at the PUMA level, as well as income at the household and individual level. The ACS income definition includes wages, income from welfare, social security, a business, and investments (income from an estate or trust, interest, dividends, royalties, and rents received), as well as retirement income (IPUMS-USA, n.d.-c). It can be negative, for instance, in the case of business losses. All incomes are converted into 2019 US-$ to adjust for inflation (IPUMS-USA, n.d.-c). Table 3.1 presents pooled descriptive statistics over 2012—2019 for all the main variables.

The income shares and alternative inequality measures have been constructed from the Integrated Public Use Microdata Series USA (IPUMS) ACS microdata (Ruggles et al., 2021). The income shares are based on individual, not on household, income as this set-up corresponds more closely to the to-be-tested hypotheses. They are based on the pre-tax income of working-age (16-64 years) individuals.[18] A corresponding individual-income Gini has also been constructed. In addition, the household-income GE(2), the Generalized Entropy Index with a=2, that is, half the square coefficient of variation, has been calculated from the IPUMS data as a further inequality measure.

Oil and gas reliance is measured by the share of individuals employed in the resource sector compared to the civilian employed population 16 years and over in a given PUMA. The employment share captures better than oil and gas production the reliance of a local area on oil and gas compared to its overall economic structure. The share is very low, with a mean of 0.6 %. Even in oil-and-gas-reliant PUMAs with a resource sector employment share above 3 %, the latter's average amounts to 6 % only. Figure 3.1 shows a map indicating each PUMA's resource sector employment share. PUMAs with a high share are spatially correlated but dispersed over many states.

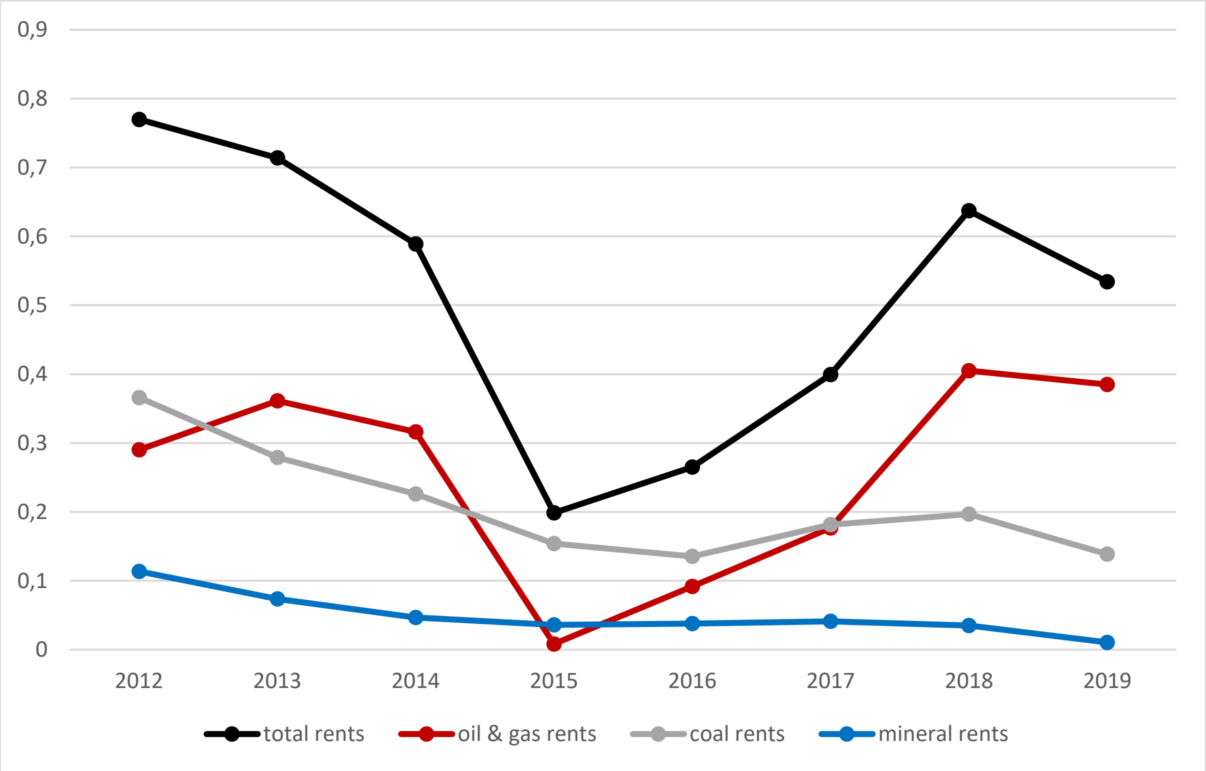
[18] Section 3.5 presents results employing household income shares based on the whole PUMA population instead.

Figure 3.1: Map of PUMA-Level Oil and Gas Reliance in 2014



The map shows the 2014 PUMA-level resource sector employment share for all contiguous US states. White PUMAs have a resource sector employment share < 1 %, those in light green between 1 and 3 %, those in mid-dark green between 3 and 10 %, and those in dark green > 10 %, with the maximum being 21 %. *Source: own elaboration based on ACS data and NHGIS shapefiles* (Manson et al., 2020; US Census Bureau, n.d.-b)

Figure 3.2: US Resource Sector Rents as Percentage of GDP 2012—2019



Source: own elaboration based on World Bank data (World Bank Group, 2021)

The employed reliance measure not only includes employment in oil and gas but also in mining and quarrying. Nevertheless, the oil and gas sector is the primary driver of changes in the resource sector employment in the considered period, rendering it a suitable measure of oil and gas reliance changes. Oil and gas rents make up 45 % of the US resource sector rent over 2012—2019 (World Bank Group, 2021). The remainder falls upon coal and mineral rents. However, the oil and gas sector drives the evolution of resource rents as a percentage of the Gross Domestic Product (GDP) (figure 3.2). Changes in the oil rent exhibit a correlation of 0.96 with changes in the overall resource sector rent over 2012—2019. In contrast, this correlation is only 0.76 for coal and 0.40 for mineral rents. Thus, the oil and gas sector is the primary driver of the changes in the resource sector.[19]

3.3 Symmetric and Asymmetric Effects of Oil and Gas Reliance on Inequality

This paper estimates the effect of changes in local oil and gas reliance, measured by the PUMA resource sector employment share, on changes in inequality, conditional on several control variables. Inequality is measured by the household- and individual-income Ginis and the GE(2). Furthermore, the income shares of the income quartiles serve as the dependent variable. The latter set-up evaluates which income groups particularly benefit from changes in oil and gas reliance.

The income shares of the four quartiles correspond to the following groups:

- Q1: the bottom 25 % incomes: the lower class, often outside of the labor market or only in precarious jobs[20], collectively owning on average 1 % of the total PUMA income (table 3.1);
- Q2: between 25 and 50 %: the working class corresponding to the low-skill, low-income workers of hypothesis 1, collectively owning 10 % of the total income;

[19] Regarding the relevance of fracking within the oil and gas sector: 51 % of the US crude oil production and 67 % of its natural gas production stemmed from fracking in 2015 (US Energy Information Administration, 2016a, 2016b).

[20] 16.6 % of people age 18 to 64 participated in government assistance programs in 2015 (US Census Bureau, 2015). Students also often belong to this group.

- Q3: between 50 and 75 %: the middle class, collectively owning 25 % of the total income; and
- Q4: the top 25 % incomes: the upper class, including the royalty recipients of hypothesis 2, collectively owning 64 % of the total income.

Royalty recipients often belong to Q4 as royalty income is highly concentrated among local residents due to highly concentrated land and resource ownership. For instance, only 8.8 % of tax returns filed by residents of Pennsylvanian Marcellus Shale counties reported receiving rents, royalties, patents, and copyrights income (Hardy & Kelsey, 2015). Similarly, the top 10 % of landowners in these counties own about 80 % of the locally owned land area, while the bottom 50 % collectively own only about 2 % (Hardy & Kelsey, 2015). Accordingly, royalty increases due to fracking are distinctively the largest in the highest income group while negligible in the others (J. P. Brown et al., 2019).

Q4's incomes do not exclusively consist of royalties. However, royalties should be Q4's most sensitive income component to changes in local oil and gas reliance at the place of residence. Oil and gas company profits do not matter much locally as their recipients typically do not reside in the extraction areas. Managers' salaries are neither earned there as the companies' headquarters are generally located in metropolitan areas, not at extraction sites (Tsvetkova & Partridge, 2016). In contrast, the share of land- and resource owners living in the extraction county is appreciable, ranging between 12 and 74 % depending on the considered state (Fitzgerald, 2014).

Royalty recipients particularly gain from oil and gas reliance, according to hypothesis 2. If this hypothesis is correct, the resource sector employment share coefficient is statistically significantly positive when Q4's income share or the Gini are the dependent variable. Meanwhile, hypothesis 1 states that low-skill workers benefit over-proportionally from oil and gas reliance. If this hypothesis is correct, the resource sector employment share coefficient is statistically significantly positive when Q2's income share is the dependent variable while it is significantly negative with the Gini.

Table 3.2 presents the regression results. The first line of coefficients presents the symmetric ones for the resource sector employment share. Appendix table 3.4 reports symmetric coefficients for all the included variables.

Table 3.2: The Effect of Changes in Oil and Gas Reliance on Inequality and Income Shares

	(1) Household Gini	(2) Household GE(2)	(3) Individual Gini	(4) Q1	(5) Q2	(6) Q3	(7) Q4
resource sector employment share	-0.002***	-0.008**	-0.002*	0.013	0.047	0.075*	-0.121*
	(0.000)	(0.003)	(0.001)	(0.009)	(0.038)	(0.039)	(0.072)
↑ resource sector employment share	0.001	-0.005	0.001*	-0.045***	-0.120***	0.013	0.124*
	(0.000)	(0.003)	(0.001)	(0.009)	(0.029)	(0.055)	(0.065)
↓ resource sector employment share	-0.003***	-0.008**	-0.002***	0.022***	0.078**	0.089**	-0.165***
	(0.000)	(0.003)	(0.001)	(0.006)	(0.032)	(0.035)	(0.061)
Controls	yes	yes	yes	yes	yes	yes	yes
PUMA FEs	yes	yes	yes	yes	yes	yes	yes
year FEs	yes	yes	yes	yes	yes	yes	yes
N	18,238	18,238	18,238	18,238	18,238	18,238	18,238
PUMAs	2,332	2,332	2,332	2,332	2,332	2,332	2,332
States	49	49	49	49	49	49	49
T	8	8	8	8	8	8	8
within-R^2 symmetric	0.207	0.088	0.109	0.115	0.130	0.050	0.085
within-R^2 asymmetric	0.213	0.089	0.116	0.121	0.136	0.052	0.090
p-value F-test: symmetry	**0.000**	**0.023**	**0.000**	**0.000**	**0.000**	**0.039**	**0.000**

The household Gini is the pre-tax household-income Gini, while the individual Gini is the pre-tax individual-income Gini for working-age (16-64 years) individuals. Household GE(2) is the household-income Generalized Entropy Index with a=2, that is, half the square coefficient of variation. Qx is the income share of the x^{th} income quartile in percent. One is the bottom-most income group, whereas four is the top one. The first line of coefficients presents symmetric ones, whereas the following two present the asymmetric ones. Controls include per capita income, population, demographic structure, educational attainment, and ethnic diversity at the PUMA level. The sample is restricted to the 48 contiguous US states plus Washington DC. Standard errors clustered at the state level are in parentheses. The null hypothesis of the F-test are symmetric resource sector employment share coefficients. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

For the Ginis and the GE(2), the symmetric resource sector employment share coefficient is negative and statistically significant, although at varying levels. These coefficients indicate that an increase in oil and gas reliance decreases inequality ceteris paribus. Conversely, a decrease in reliance leads ceteris paribus to an increase in inequality. A one-percentage-point increase in the resource sector employment share decreases the household-income Gini by 0.002 points. At the Gini mean of 0.448, this corresponds to a decrease by 0.5 %. Thus, the effect of changes in oil and gas reliance on inequality is tiny in absolute size. Still, this effect is significant despite the oil and gas sector's small size in the overall economy.

For the income shares, the symmetric resource sector employment share coefficient is positive for Q1, Q2, and Q3, and negative for the top-income share Q4. However, only Q3's and Q4's coefficients are statistically significant, and then only at the 10 % level. Thus, the income shares of the two bottom income groups appear to be unaffected by oil and gas reliance. The one of the middle class might increase when oil and gas reliance increases, while the top income share might decrease ceteris paribus. Thus, the observed inequality decrease seems to stem from relative income changes in the upper part of the income distribution only. The obtained symmetric results reject that royalty recipients particularly benefit from oil and gas reliance, contradicting hypothesis 2. They are inconclusive regarding hypothesis 1 about low-income workers over-proportionally benefiting from oil and gas reliance.

The effect of changes in oil and gas reliance on inequality might be asymmetric, that is, differ between increases and decreases in reliance as suggested by hypothesis 3. In the considered 2012—2019 sample, the resource sector employment share increased compared to the previous year for 37 % of the year-PUMA observations. Decreases account for 39 % of the sample, while the share remained constant in 24 % of the cases (mostly when no resource extraction occurs, hence an unchanging share of zero). The largest decrease in the resource sector employment share between two subsequent years amounts to -6.6 percentage points and the largest increase to 8.7.

The second and third lines of coefficients in table 3.2 show the asymmetric case. Line two presents the coefficients for increasing and line three the ones for decreasing resource sector employment shares. If the effect of changes in oil and gas reliance on inequality is symmetric, then the coefficients of increases and decreases in the resource sector employment share are of the same sign and size. If the effect is asymmetric, the coefficients' sizes are statistically significantly different. One can test the symmetry of two coefficients via an F-test with the null hypothesis of identical coefficients for increases and decreases. The last line of table 3.2 reports the test results.

All the obtained resource sector employment share coefficients are highly asymmetric as all F-tests' p-values are below 0.005. These results highlight the importance of estimating asymmetric effects. For both Ginis and the GE(2), the resource sector employment share increases coefficient is positive but not statistically significant at the 5 % level, while negative and significant for decreases. The strong negative effect for decreases explains the overall negative effect of changes in oil and gas reliance on inequality. A decrease in the resource sector employment share by one percentage point increases the household-income Gini by 0.003 points ceteris paribus. In contrast, an increase in reliance does not necessarily imply a change in inequality. The inequality increase during decreasing oil and gas reliance is not compensated by inequality decreases during reliance increases.

Increasing oil and gas reliance particularly affects the bottom income groups Q1 and Q2. Their coefficients are negative and significant at the 1 % level. A one-percentage-point increase in the resource sector employment share decreases Q1's income share by 0.05 percentage points (its average being 1 %) and Q2's by 0.12 (average 10.7 %) ceteris paribus. In contrast, Q3 and Q4 exhibit positive coefficients but only Q4's is statistically significant at the 10 % level. Thus, increases in oil and gas reliance either do not affect or lead to an increase in the income shares of the top income groups.

As income shares are considered, Q1's and Q2's negative effects do not imply that they have absolute income losses during increases in oil and gas reliance. However, their income increases are smaller than those of the other income groups. This effect might be due to migration to oil and gas boom areas which attenuates potential income gains for workers (Wilson, 2020). The obtained results contradict hypothesis 1, stating that low-income workers particularly benefit from an oil and gas boom. They might hint at Q4's royalty recipients over-proportionally benefitting, potentially confirming hypothesis 2.

Decreasing oil and gas reliance affects all income groups. All have positive coefficients except Q4, whose coefficient is negative. The coefficients are all statistically significant at the 5 % level. Thus, decreases in oil and gas reliance decrease ceteris paribus the income shares of all income groups except the top-most one, who relatively gain. This pattern explains the overall inequality increases during decreases in reliance.

With oil and gas production decreasing, wages immediately decrease with reduced working hours or even stop being paid when people are laid off, especially in the highly flexible US labor market. However, workers might not move immediately to more prospering areas due to hopes of a fast recovery or migration costs. Consequently, the shock is felt locally as migration cannot level out these effects in contrast to periods of increasing oil and gas reliance.

The top incomes' relative income increases might be thanks to royalties not ceasing immediately when oil and gas production stops. This phenomenon especially occurs if the stop is expected to be temporary, due to a decrease in oil prices, and not due to the depletion of an oil and gas field. Royalty income generated from leases typically consists of two parts: a share of the gross production revenue and a primary term (Fitzgerald, 2014). Even if no production occurs, the primary term income flows for up to three years (Fitzgerald, 2014). Rapidly decreasing company profits do not matter locally at the extraction site due to absentee owners. Thus, the relative income gains of Q4 might not be absolute income gains but due to the even more substantial decline in other groups' incomes during oil and gas reliance decreases. Royalty recipients benefit, maybe not over-proportionally during reliance increases, but distinctively during oil and gas reliance decreases, at least in the short run.

Hypothesis 2 is hence confirmed: a relative advantage for royalty recipients exists, particularly during reliance decreases. Hypothesis 1 regarding particularly strong income gains for low-income workers cannot be confirmed as Q2's income share declines during both increases and decreases in oil and gas reliance. This result contrasts with the wage literature finding proportional or even over-proportional earning gains for low-income workers in resource areas during the fracking boom (Gittings & Roach, 2020; Jacobsen, 2019). However, these wage gains are outmatched by royalty gains and disappear during periods of decreasing oil and gas reliance, not considered in the aforementioned papers.

The bottom income groups do not appear to benefit at all from oil and gas reliance compared to the upper ones. Q1 and Q2 experience relative income losses during both increases and decreases in oil and gas reliance. This pattern is potentially worrisome as these are already the worst-off income groups. Especially low-skill workers are already struggling compared to higher-skilled ones due to the ongoing deindustrialization eliminating their previous, relatively well-paying jobs (Autor & Dorn, 2013). The oil and gas sector cannot reverse this trend: neither by creating jobs nor by providing workers a better alternative during its decline. The presence of the oil and gas sector renders things relatively worse for the bottom incomes in any case. The income share of the middle class (Q3) appears to go more or less with the flow of the oil and gas reliance cycle, even though the effect is stronger during reliance decreases. This pattern indicates that the middle class misses suitable alternatives to the oil and gas sector. The top incomes achieve relative income gains during both reliance increases and decreases, comparatively benefitting from the oil and gas sector in any case. In sum, this pattern leads to increased income polarization ceteris paribus in oil-and-gas-reliant areas against a backdrop of already rising income polarization in the US due to skill-biased technological change substituting routine tasks (Autor & Dorn, 2013; Seifert, 2021).

The results also demonstrate the importance of estimating asymmetric effects to adequately assess the oil and gas reliance-inequality relationship and the hypotheses at hand. In the symmetric case, the Q1 and Q2 resource sector employment share coefficients were not statistically significant. These estimates obscure that Q1 and Q2 incur relative income losses during both increases and decreases in oil and gas reliance, strongly contradicting hypothesis 1. Q4's negative symmetric coefficient similarly hides the fact that the income share of this group might de facto increase with increasing reliance. The asymmetric results refute the symmetric results' rejection of hypothesis 2 about royalty recipients benefitting over-proportionally. Hypothesis 3 postulating asymmetric effects of changes in oil and gas reliance can be confirmed. The underlying causes of these asymmetric effects still need to be investigated further.

In sum, a complex picture emerges of the distributional effects of local changes in oil and gas reliance. The oil and gas sector, albeit small, has a significant impact. This impact is felt locally: there is no complete diversion of benefits and losses to non-residents. However, the impact is highly uneven. Fewer benefits accrue to lower incomes during increases in oil and gas reliance while they bear the brunt of the reliance decreases. Conversely, top incomes benefit over-proportionally. This pattern might explain socio-political tensions surrounding fracking projects.

3.4 IV Estimation Results

Reverse causality between oil and gas reliance and inequality might plague the previous conventional FEs estimations. Higher inequality might imply lesser political influence of lower-income individuals and more for higher-income ones. These groups might be particularly in favor or against fracking, depending on the perceived benefits, resulting in the (non) implementation of oil and gas projects. Therefore, this section presents results from Two-Stage Least Squares (2SLS) IV FEs regressions.

The constructed instruments for oil and gas reliance consist of two parts. First, a PUMA's surface share over a shale basin provides the baseline levels, broadly following the fracking literature (Bartik et al., 2019; Fetzer, 2014; Feyrer et al., 2017; Tsvetkova & Partridge, 2016)[21]. Second, this variable is interacted with either the national-level US resource sector employment share, the US gas or the world market oil price[22], providing the variation over time needed for FEs estimations. None of the three instruments outperforms the others regarding relevance. Therefore, results for all three of them are reported.

The required geological data stems from the US Energy Information Administration (EIA) and the IPUMS National Historical Geographic Information System (NHGIS) (Manson et al., 2020; US Energy Information Administration, 2019). The annual average US gas and world market oil price data also stem from the EIA (US Energy Information Administration,

[21] A basin is a geological concept that refers to a region where geological forces have caused the rock layers to form a rough bowl shape, with the center then filled in by layers of sediment. If one of the layers is a shale layer, the basin is a shale basin (Bartik et al., 2019).
[22] Oil is a fungible commodity whose price is determined in a global marketplace. In contrast, natural gas access to the global market is limited by the absence of liquefied natural gas export facilities in the US. As a result, the US natural gas market is largely insulated from world markets (Fitzgerald & Rucker, 2016).

2021a, 2021b). The prices are inflation-adjusted to 2019 US-$. They evolute relatively parallel with the US resource sector employment share from the ACS (appendix figure 3.4). Appendix table 3.5 presents pooled descriptive statistics over 2012—2019 for the IVs.

Several prerequisites need to be fulfilled to estimate valid IV regressions: relevance and exogeneity of the instrument as well as compliance with the exclusion restriction. First, the instrument must be relevant, significantly influencing the instrumented variable (PUMA-level resource sector employment share). Shale basins are a prerequisite for fracking as the shale contains the oil and gas. These basins often overlap with conventional oil and gas extraction sites as shale is also the source rock for oil reservoirs (Swenson et al., 2012). Oil and gas constitute a large part of the US resource sector, as discussed in section 3.2. Changes in the US resource sector employment share mirror general employment tendencies in this sector, also encountered at the local level. Changes in the US and world market prices are closely linked to local oil and gas production changes as the local production adjusts to these prices. Overall, the instruments should hence be relevant. Their relevance has also been tested empirically via F-tests of the first stage IV coefficient's significance.

Second, the instrument must be exogenous: no reverse causality between the dependent variable (inequality) and the instrument. Basins are a geological feature existing long before humankind, rendering them exogenous to inequality. The PUMA-level oil and gas sector is not large enough to influence the evolution of the US one nor a fortiori world market prices. Even the PUMA with the highest absolute number of resource-sector workers represents only 1.8 % of the US aggregate of these workers. Therefore, PUMA-level inequality should not influence the national-level or worldwide parts of the instruments. Thus, the exogeneity of the instrument is given.

Third, the exclusion restriction must hold: the instrument should only influence the dependent variable (inequality) via the instrumented variable (PUMA-level resource sector employment share). This condition implies two things. First, the instrument should not influence the dependent variable directly. The basins do not cause local income inequality directly, and they also do not measure the geological diversity of an area. Thus, their influence on local income inequality must pass through the oil and gas sector. The time-varying part of the instrument does neither directly cause local income inequality changes. Changes in the US resource sector employment share, gas prices, and the world market oil price only directly induce changes in the local-level oil and gas production and employment, then translating to changes in local-level inequality. Second, there should not exist any other variable meddling between the instrument and the dependent variable besides the instrumented one. However, the existence of basins might influence not only the local oil and gas reliance but also the local per capita income. The latter might feedback on local income inequality, in addition to the direct, immediate effect of the oil and gas reliance. Similarly, changes in the US resource sector employment share, gas prices, and the world market oil price are also related to changes in local income levels. However, per capita income is controlled for in all regressions, nullifying this issue.

Table 3.3: IV Results for the Effect of Changes in Oil and Gas Reliance on Inequality

	(1) FEs Household Gini	(2) IV FEs Household Gini	(3) IV FEs Household Gini	(4) IV FEs Household Gini
resource sector employment share	**-0.002***** **(0.000)**	**-0.013***** **(0.004)**	**-0.014***** **(0.005)**	**-0.009*** **(0.005)**
Controls	yes	yes	yes	yes
PUMA FEs	yes	yes	yes	yes
year FEs	yes	yes	yes	yes
N	18,238	18,235	18,235	18,235
PUMAs	2,332	2,329	2,329	2,329
States	49	49	49	49
T	8	8	8	8
Within-R^2	0.207	0.149	0.133	0.184
Instrument		share basin*US resource sector employment share	share basin*US gas price	share basin*world oil price
1st stage F-statistic		9.28	9.42	9.80

The household Gini is the pre-tax household-income Gini. Controls include per capita income, population, demographic structure, educational attainment, and ethnic diversity at the PUMA level. The sample is restricted to the 48 contiguous US states plus Washington DC. Standard errors clustered at the state level are in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01

Table 3.3 presents the results for the household-income Gini. The first column reports the conventional FEs result, while the following three columns report the IV FEs results employing the previously discussed instruments. Appendix table 3.6 reports the first stage results of the IV regressions. Appendix table 3.7 presents the IV results for the individual-income Gini and the GE(2), while appendix table 3.8 shows the income shares results.

The last lines of these tables present the first stage F-statistic of testing the instrument's coefficient against being equal to zero. According to a rule-of-thumb, this F-statistic should be larger than ten for the instrument to be relevant (Staiger & Stock, 1997). All the employed instruments produce F-statistics slightly below ten, ranging from 9.28 to 9.80. These statistics indicate relatively weak instrumental variables, potentially leading to biased coefficients. However, the estimated coefficients and their significance levels remain unchanged when employing weak instrument techniques such as Limited Information Maximum Likelihood or General Method of Moments regressions (results available upon request). Furthermore, a similar picture regarding the effect of changes in oil and gas reliance on inequality emerges in the reduced form regressions (results available upon request). Thus, the employed instruments appear acceptable.

The IV results resemble the conventional symmetric FEs ones regarding coefficient signs. The coefficients of both Ginis, GE(2), and Q4 remain negative, while the ones of Q1 and Q2 stay positive. Only Q3's coefficients turn in two out of three cases negative but are always statistically insignificant. Overall, the IV regressions confirm the conventional symmetric FEs results.

The absolute coefficient sizes are larger in all the IV regressions than in the conventional FEs ones. For the household-income Gini, for instance, they range between 0.009 and 0.014 compared to 0.002 before. At the most, this would imply that a one-percentage-point increase in the PUMA resource sector employment share decreases the Gini by 0.014 points, that is, by 3 % at its mean. Thus, the conventional FEs coefficients can be considered a lower bound and the IV ones an upper bound of the effect.

IV regressions are not possible for the asymmetric regressions. The latter would require two completely distinct instruments for increases and decreases in oil and gas reliance.[23] An alternative consists in asymmetric reduced form regressions for each of the three instruments. This procedure assumes the same changes for each PUMA as the time-varying instrument component is at the US or world level. However, even during substantial busts not all places are equally affected. Consequently, this is rather a boom-versus-bust analysis than an asymmetric one. Overall, the coefficients' signs remain as in the conventional FEs estimations, or the coefficients are not statistically significant (results available upon request). Thus, the asymmetric reduced form results do not contradict the conventional FEs ones but cannot act as an alternative asymmetric regression either.

3.5 Robustness Checks

The present paper runs several checks to evaluate the robustness of the obtained results. This section tests for the influence of the degree of oil and gas reliance, urbanization, and industrialization. Furthermore, it employs household income shares.

PUMAs without any oil and gas production might distort the results as their resource sector employment share is unchangingly zero. Therefore, the sample has been restricted to those PUMAs with some part of their territory located over a shale basin. These are about 45 % of all PUMAs. Appendix table 3.9 presents the results of this exercise. The obtained pattern is almost the same as in the main configuration, the full sample, for both symmetric and asymmetric effects. Thus, the previous interpretations and conclusions are confirmed.

Another possibility is to restrict the sample to only those PUMAs with a resource sector employment share above zero in all years. However, this reduces the sample size further to less than a third of the full sample. Overall, a similar pattern as for the main configuration emerges (appendix table 3.10). However, the GE(2) effects cannot be classified as asymmetric anymore. Instead, they are symmetrically negative. Increases in reliance are

[23] Running FEs estimations on subsamples of oil and gas reliance increases and decreases does not result in the same asymmetric analysis as in the main regressions due to large gaps between observations.

now ceteris paribus associated with significant decreases in GE(2), while the effect remains insignificant for both Ginis. The income share coefficients often decrease in statistical significance, but their respective signs remain as previously. In sum, the obtained results still confirm the previous results and conclusions.

The effects of changes in oil and gas reliance might also vary across states. A way to check for these differences is to run the regressions by state. However, only one state (Texas) has an adequate number of oil-and-gas-reliant and non-reliant PUMAs. Texas has 212 PUMAs, of which 60 have a resource sector employment share above 3 % in 2014. Its subsample is almost exclusively made up of cases of increases and decreases in the resource sector employment share, each accounting for about half of the subsample. Appendix table 3.11 presents the results where the standard errors are now clustered at the PUMA level.

The estimated symmetric and asymmetric effects for Texas sometimes differ from the main ones but still support the latter's conclusions. The most striking difference are the negative coefficients for Q1 and Q2 in all cases. Q2's coefficients are not asymmetric anymore, although the oil and gas reliance decrease one is not statistically significant. Thus, hypothesis 1 is still rejected: low-income workers in Texas do neither benefit over-proportionally from oil and gas reliance. Q4 still experiences relative income gains during reliance increases as the correspondent coefficient is positive and statistically significant. The coefficient for decreases is now insignificantly positive. Nevertheless, the effect remains asymmetric. Thus, royalty recipients in Texas still benefit over-proportionally from oil and gas reliance, even though these gains now rather stem from reliance increases, not decreases. These results confirm again hypothesis 2. In sum, the Texan results still support the conclusions for the US as a whole while indicating some Texan peculiarities. The latter might be linked to Texas having a long oil and gas production tradition, even before the onset of the fracking revolution (Michaels, 2011).

The effect of changes in the local oil and gas reliance might differ between urban and rural areas. The actual oil and gas extraction predominantly occurs in rural areas, although fracking might occur in more (sub)urban geographies (Mayer et al., 2020; Tsvetkova & Partridge, 2016). In contrast, the oil and gas companies' headquarters are typically located in urban areas (Tsvetkova & Partridge, 2016). The specialized workers typically reside in urban areas and commute long-distance (Tsvetkova & Partridge, 2016; Wang, 2020). These individuals count as being employed in the resource sector of their place of residence PUMA. Furthermore, not distinguishing between urban and rural areas might pick up diverging inequality evolutions between them, which are not incorporated in the control variables. These trends could bias the obtained estimates. Therefore, regressions on various urban and rural subsamples have been run. Appendix table 3.12 reports the results for the household-income GINI (remaining results available upon request).

The symmetric and asymmetric results remain robust in the various rural and urban subsamples. The overall pattern remains, especially regarding the signs of the resource sector employment share coefficients, while the significance levels sometimes differ from the full sample ones. Thus, no urban/rural specificities can be detected in the effect of changes in oil and gas reliance on inequality and income shares.

The sectoral structure of the economy, besides the resource sector, might also influence the reliance-inequality relationship. Therefore, the employment shares in the manufacturing and agricultural sectors have been included as additional control variables into the regressions. All the resource sector employment share coefficients remain unaffected by their inclusion (results available upon request).

The employed income shares are based on individual income from working-age individuals, not on household income of the whole PUMA population. While individual income shares fit better with the hypotheses this paper aims to study, using them comes at a cost. The confidentiality protections in the IPUMS are more restrictive for individual than for household data, resulting in more missing observations, especially for more extreme and unusual income values. This feature reduces the accuracy and representativity of individual-income-based measures. Furthermore, household income shares provide a background for the results obtained with the household-income Gini and GE(2). They reflect the overall societal conditions in a PUMA as they include all of its inhabitants. They also match better with the control variables as the latter are measured for the whole PUMA population. Therefore, the following discusses results for income shares based on the household income distribution for the whole PUMA population, calculated from the IPUMS micro-level ACS data.

Household income shares also exhibit highly asymmetric effects (appendix table 3.13). Only Q3 now displays symmetric coefficients. Regarding increases in oil and gas reliance, the results sometimes differ from the individual-income ones. The resource sector employment share coefficient is now not statistically significant positive for Q2, while it turns significantly positive for Q3. For Q1, this coefficient remains statistically significant and negative, while Q4's coefficient is not significantly negative. Regarding decreases in reliance, similar results to the individual-income ones are obtained. The resource sector employment share coefficients for all income shares remain of the same sign as previously and stay statistically significant. The bottom incomes bear the brunt of the bust while the top incomes are again less affected and experience relative income gains. Overall, hypothesis 1 regarding over-proportional income gains for low-income workers is again rejected as this group suffers disproportionally during decreases in oil and gas reliance. Royalty recipients again relatively benefit due to being less affected by reliance decreases, confirming hypothesis 2.

3.6 Conclusion

This paper studied how changes in oil and gas reliance influence within-PUMA income inequality during the US fracking era 2012—2019. It runs FEs regressions based on ACS data (Ruggles et al., 2021; US Census Bureau, n.d.-b). Unlike previous studies, the present one focuses on income and income groups, estimates asymmetric effects, and covers a period including both boom and bust.

The obtained oil and gas reliance coefficients are highly asymmetric and differ across income groups. Oil and gas reliance and inequality are negatively related, especially during reliance decreases. Reliance decreases increase inequality while reliance increases do not decrease it ceteris paribus. Top incomes, especially royalty recipients, are the most resilient group to oil and gas reliance decreases, at least in the short run. They might also gain over-proportionally relative to the other income groups during reliance increases. In contrast, the present paper could not identify any oil and gas gains for low-income workers. Instead, they experience relative income losses during both increases and decreases in oil and gas reliance, together with the bottom-most incomes. These results show that only assessing wages and booms as well as estimating symmetric effects is insufficient to gauge the question at hand.

In sum, a complex picture emerges of the income distribution effects of local changes in oil and gas reliance. The oil and gas sector, albeit small, has a significant impact. However, it cannot replace the lost manufacturing jobs of low-skill workers. Instead, it reinforces the polarization tendency due to skill-biased technological change. This pattern might explain socio-political tensions surrounding fracking projects. Consequently, policymakers should pay particular attention to cushioning the effect of both the establishment and the close-down of the oil and gas sector for low-skill workers.

The present paper can only constitute a starting point for discussing the local inequality consequences of the US fracking revolution. Future research is needed to assess longer time horizons and investigate further the underlying channels behind the asymmetric effect of changes in oil and gas reliance on inequality.

3.7 Appendix

Table 3.4: The Symmetric Effects of Changes in Oil and Gas Reliance on Inequality and Income Shares: All Coefficients

	(1) Household Gini	(2) Household GE(2)	(3) Individual Gini	(4) Q1	(5) Q2	(6) Q3	(7) Q4
resource sector employment share	**-0.002***** **(0.000)**	**-0.008**** **(0.003)**	**-0.002*** **(0.001)**	**0.013** **(0.009)**	**0.047** **(0.038)**	**0.075*** **(0.039)**	**-0.121*** **(0.072)**
ln(per capita income)	0.174*** (0.009)	0.538*** (0.039)	0.051*** (0.007)	0.810*** (0.087)	-0.089 (0.338)	-6.132*** (0.461)	5.023*** (0.673)
ln(population)	-0.043*** (0.011)	-0.102*** (0.032)	-0.005 (0.011)	0.074 (0.164)	0.626 (0.582)	-0.610 (0.460)	-0.346 (0.934)
share under 25 years	0.003*** (0.000)	0.009*** (0.001)	0.003*** (0.000)	-0.031*** (0.004)	-0.137*** (0.011)	-0.134*** (0.012)	0.276*** (0.020)
share over 64 years	0.002*** (0.000)	0.005*** (0.001)	0.001*** (0.000)	-0.012*** (0.004)	-0.083*** (0.013)	-0.064*** (0.015)	0.120*** (0.023)
share high school diploma and higher	-0.001*** (0.000)	-0.003*** (0.001)	-0.001*** (0.000)	0.005** (0.002)	0.059*** (0.008)	0.089*** (0.011)	-0.132*** (0.009)
share bachelor's degree and higher	-0.001*** (0.000)	-0.005*** (0.001)	-0.000** (0.000)	-0.001 (0.002)	-0.014** (0.006)	0.031*** (0.007)	-0.028** (0.011)
ethnic diversity index	0.013*** (0.004)	0.030 (0.025)	0.004 (0.005)	0.202** (0.089)	-0.234 (0.290)	-1.043** (0.460)	0.760 (0.561)
constant	-0.822*** (0.143)	-3.842*** (0.585)	0.111 (0.139)	-7.591*** (2.061)	5.029 (7.292)	93.020*** (7.057)	18.723 (12.188)
PUMA FEs	yes	yes	yes	yes	yes	yes	yes
year FEs	yes	yes	yes	yes	yes	yes	yes
N	18,238	18,238	18,238	18,238	18,238	18,238	18,238
PUMAs	2,332	2,332	2,332	2,332	2,332	2,332	2,332
States	49	49	49	49	49	49	49
T	8	8	8	8	8	8	8
within-R^2	0.207	0.088	0.109	0.115	0.130	0.050	0.085

The household Gini is the pre-tax household-income Gini, while the individual Gini is the pre-tax individual-income Gini for working-age (16-64 years) individuals. Household GE(2) is the household-income Generalized Entropy Index with a=2, that is, half the square coefficient of variation. Qx is the income share of the x^{th} income quartile in percent. One is the bottom-most income group, whereas four is the top one. The sample is restricted to the 48 contiguous US states plus Washington DC. Standard errors clustered at the state level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3.5: IV Descriptive Statistics 2012—2019

Variable	Observations	Mean	Std. Dev.	Min	Max
Basin dummy	18,238	0.449	0.497	0	1
Share PUMA surface over a basin (%)	18,238	37.99	46.38	0	100
US resource sector employment share (%)	18,238	0.555	0.073	0.468	0.656
US gas price	18,238	4.72	0.755	3.81	6.17
World oil price	18,238	73.48	24.51	46.08	107.4

The reported values are for pooling the data over the whole study period (2012—2019). The sample is restricted to the 48 contiguous US states plus Washington DC. The US gas and the world oil price are in 2019 US-$.

Table 3.6: First Stage Results from 2SLS IV FEs Regressions

	(1) resource sector employment share	(2) resource sector employment share	(3) resource sector employment share
share basin*US resource sector employment share	0.016^{***}		
	(0.005)		
share basin*US gas price		0.001^{***}	
		(0.000)	
share basin*world oil price			0.000^{***}
			(0.000)
Controls	yes	yes	yes
PUMA FEs	yes	yes	yes
year FEs	yes	yes	yes
N	18,235	18,235	18,235
PUMAs	2,329	2,329	2,329
States	49	49	49
T	8	8	8
within-R^2	0.062	0.058	0.056
F-statistic	9.28	9.42	9.80

Controls include per capita income, population, demographic structure, educational attainment, and ethnic diversity at the PUMA level. The sample is restricted to the 48 contiguous US states plus Washington DC. Standard errors clustered at the state level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3.7: Individual-Income Gini and GE(2) IV Results

	(1) IV FEs Individual Gini	(2) IV FEs Individ-ual Gini	(3) IV FEs Individ-ual Gini	(4) IV FEs Household GE(2)	(5) IV FEs House-hold GE(2)	(6) IV FEs House-hold GE(2)
resource sector em-ployment share	-0.010** (0.004)	-0.011*** (0.004)	-0.010* (0.005)	-0.023 (0.019)	-0.029 (0.021)	-0.020 (0.020)
Controls	yes	yes	yes	yes	yes	yes
PUMA FEs	yes	yes	yes	yes	yes	yes
year FEs	yes	yes	yes	yes	yes	yes
N	18,235	18,235	18,235	18,235	18,235	18,235
PUMAs	2,329	2,329	2,329	2,329	2,329	2,329
States	49	49	49	49	49	49
T	8	8	8	8	8	8
within-R^2	0.076	0.067	0.080	0.083	0.079	0.085
Instrument	share ba-sin*US re-source sec-tor employ-ment share	share ba-sin*US gas price	share basin* world oil price	share ba-sin*US re-source sec-tor employ-ment share	share ba-sin*US gas price	share basin* world oil price
1st stage F-statistic	9.28	9.42	9.80	9.28	9.42	9.80

The Gini is the pre-tax individual-income Gini for working-age (16-64 years) individuals. Household GE(2) is the household-income Generalized Entropy Index with a=2, that is, half the square coefficient of variation. Controls include per capita income, population, demographic structure, educational attainment, and ethnic diversity at the PUMA level. The sample is restricted to the 48 contiguous US states plus Washington DC. Standard errors clustered at the state level are in parentheses.
* p < 0.1, ** p < 0.05, *** p < 0.01

Table 3.8: Income Shares IV Results

	(1) IV FEs Q1	(2) IV FEs Q2	(3) IV FEs Q3	(4) IV FEs Q4	(5) IV FEs Q1	(6) IV FEs Q2	(7) IV FEs Q3	(8) IV FEs Q4	(9) IV FEs Q1	(10) IV FEs Q2	(11) IV FEs Q3	(12) IV FEs Q4
resource sector employment share	0.175**	0.607**	-0.034	-0.761*	0.170*	0.554*	0.053	-0.951**	0.187	0.454	-0.215	-0.771
	(0.088)	(0.253)	(0.266)	(0.390)	(0.096)	(0.296)	(0.264)	(0.408)	(0.118)	(0.343)	(0.337)	(0.504)
Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
PUMA FEs	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
year FEs	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
N	18,235	18,235	18,235	18,235	18,235	18,235	18,235	18,235	18,235	18,235	18,235	18,235
PUMAs	2,329	2,329	2,329	2,329	2,329	2,329	2,329	2,329	2,329	2,329	2,329	2,329
States	49	49	49	49	49	49	49	49	49	49	49	49
T	8	8	8	8	8	8	8	8	8	8	8	8
within-R^2	0.082	0.089	0.049	0.068	0.084	0.096	0.050	0.056	0.077	0.108	0.044	0.067
Instrument	share basin*US resource sector employment share				share basin*US gas price				share basin*world oil price			
1st stage F-statistic	9.28	9.28	9.28	9.28	9.42	9.42	9.42	9.42	9.80	9.80	9.80	9.80

Qx is the income share of the x^{th} income quartile in percent. One is the bottom-most income group, whereas four is the top one. Controls include per capita income, population, demographic structure, educational attainment, and ethnic diversity at the PUMA level. The sample is restricted to the 48 contiguous US states plus Washington DC. Standard errors clustered at the state level are in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01

Table 3.9: PUMAs over a Basin Results

	(1) Household Gini	(2) Household GE(2)	(3) Individual Gini	(4) Q1	(5) Q2	(6) Q3	(7) Q4
resource sector employment share	-0.002***	-0.007*	-0.001	0.011	0.047	0.056	-0.098
	(0.001)	(0.004)	(0.001)	(0.011)	(0.049)	(0.035)	(0.077)
↑ resource sector employment share	0.001	-0.004*	0.002***	-0.042***	-0.103***	-0.057*	0.156***
	(0.000)	(0.002)	(0.000)	(0.008)	(0.025)	(0.033)	(0.036)
↓ resource sector employment share	-0.003***	-0.008**	-0.002**	0.020**	0.079*	0.082**	-0.147**
	(0.000)	(0.003)	(0.001)	(0.008)	(0.044)	(0.031)	(0.063)
Controls	yes	yes	yes	yes	yes	yes	yes
PUMA FEs	yes	yes	yes	yes	yes	yes	yes
year FEs	yes	yes	yes	yes	yes	yes	yes
N	8,195	8,195	8,195	8,195	8,195	8,195	8,195
PUMAs	1,061	1,061	1,061	1,061	1,061	1,061	1,061
States	34	34	34	34	34	34	34
T	8	8	8	8	8	8	8
within-R^2 symmetric	0.235	0.091	0.099	0.083	0.112	0.054	0.080
within-R^2 asymmetric	0.248	0.093	0.109	0.094	0.121	0.058	0.088
p-value F-test: symmetry	**0.000**	**0.091**	**0.000**	**0.000**	**0.000**	**0.000**	**0.000**

This table reports results when only PUMAs over a shale basin are included into the regression. These PUMAs are located in 34 states. The household Gini is the pre-tax household-income Gini, while the individual Gini is the pre-tax individual-income Gini for working-age (16-64 years) individuals. Household GE(2) is the household-income Generalized Entropy Index with a=2, that is, half the square coefficient of variation. Qx is the income share of the x^{th} income quartile in percent. One is the bottom-most income group, whereas four is the top one. The first line of coefficients presents symmetric ones, whereas the following two present the asymmetric ones. Controls include per capita income, population, demographic structure, educational attainment, and ethnic diversity at the PUMA level. Standard errors clustered at the state level are in parentheses. The null hypothesis of the F-test are symmetric resource sector employment share coefficients. * p < 0.1, ** p < 0.05, *** p < 0.01

Table 3.10: PUMAs with a Resource Sector Employment Share >0 % Results

	(1) Household Gini	(2) Household GE(2)	(3) Individual Gini	(4) Q1	(5) Q2	(6) Q3	(7) Q4
resource sector employment share	-0.002***	-0.007**	-0.001	0.001	0.021	0.056	-0.067
	(0.000)	(0.003)	(0.001)	(0.009)	(0.045)	(0.038)	(0.076)
↑ resource sector employment share	0.000	-0.005***	0.001	-0.036***	-0.078***	-0.010	0.086
	(0.000)	(0.002)	(0.001)	(0.007)	(0.028)	(0.042)	(0.053)
↓ resource sector employment share	-0.002***	-0.008**	-0.001*	0.011	0.047	0.071**	-0.105
	(0.000)	(0.003)	(0.001)	(0.009)	(0.045)	(0.033)	(0.074)
Controls	yes	yes	yes	yes	yes	yes	yes
PUMA FEs	yes	yes	yes	yes	yes	yes	yes
year FEs	yes	yes	yes	yes	yes	yes	yes
N	5,533	5,533	5,533	5,533	5,533	5,533	5,533
PUMAs	720	720	720	720	720	720	720
States	44	44	44	44	44	44	44
T	8	8	8	8	8	8	8
within-R^2 symmetric	0.241	0.098	0.082	0.057	0.083	0.064	0.069
within-R^2 asymmetric	0.251	0.100	0.090	0.065	0.092	0.066	0.075
p-value F-test: symmetry	**0.000**	0.170	**0.000**	**0.000**	**0.001**	**0.003**	**0.000**

The table reports regression results for a sample including all PUMAs with a resource sector employment share >0 % in all years. This regression sample includes 44 US states. The household Gini is the pre-tax household-income Gini, while the individual Gini is the pre-tax individual-income Gini for working-age (16-64 years) individuals. Household GE(2) is the household-income Generalized Entropy Index with a=2, that is, half the square coefficient of variation. Qx is the income share of the x^{th} income quartile in percent. One is the bottom-most income group, whereas four is the top one. The first line of coefficients presents symmetric ones, whereas the following two present the asymmetric ones. Controls include per capita income, population, demographic structure, educational attainment, and ethnic diversity at the PUMA level. Standard errors clustered at the state level are in parentheses. The null hypothesis of the F-test are symmetric resource sector employment share coefficients. * p < 0.1, ** p < 0.05, *** p < 0.01

Table 3.11: Texas Results

	(1) Household Gini	(2) Household GE(2)	(3) Individual Gini	(4) Q1	(5) Q2	(6) Q3	(7) Q4
resource sector employment share	-0.001**	-0.002	0.001	-0.018**	-0.065**	0.002	0.068
	(0.001)	(0.003)	(0.001)	(0.009)	(0.031)	(0.047)	(0.056)
↑ resource sector employment share	0.000	-0.002	0.002**	-0.035***	-0.103***	-0.059	0.156**
	(0.001)	(0.004)	(0.001)	(0.012)	(0.036)	(0.053)	(0.070)
↓ resource sector employment share	-0.002***	-0.004	0.000	-0.010	-0.054	0.037	0.010
	(0.001)	(0.003)	(0.001)	(0.010)	(0.036)	(0.055)	(0.065)
Controls	yes	yes	yes	yes	yes	yes	yes
PUMA FEs	yes	yes	yes	yes	yes	yes	yes
year FEs	yes	yes	yes	yes	yes	yes	yes
N	1,633	1,633	1,633	1,633	1,633	1,633	1,633
PUMAs	210	210	210	210	210	210	210
T	8	8	8	8	8	8	8
within-R^2 symmetric	0.180	0.080	0.084	0.063	0.063	0.084	0.071
within-R^2 asymmetric	0.200	0.089	0.089	0.068	0.067	0.091	0.074
p-value F-test: symmetry	**0.000**	0.754	**0.026**	**0.036**	0.203	0.100	**0.044**

The table reports regression results for all Texan PUMAs with available data. Standard errors clustered at the PUMA level are in parentheses. The household Gini is the pre-tax household-income Gini, while the individual Gini is the pre-tax individual-income Gini for working-age (16-64 years) individuals. Household GE(2) is the household-income Generalized Entropy Index with a=2, that is, half the square coefficient of variation. Qx is the income share of the x^{th} income quartile in percent. One is the bottom-most income group, whereas four is the top one. The first line of coefficients presents symmetric ones, whereas the following two present the asymmetric ones. Controls include per capita income, population, demographic structure, educational attainment, and ethnic diversity at the PUMA level. The null hypothesis of the F-test are symmetric resource sector employment share coefficients. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3.12: Urban and Rural Subsamples Household-Income Gini Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	urban				**full sample**	**rural**			
PUMA:	in an MSA central city	at least partly in an MSA central city	population density >150 persons/km²	completely in an MSA		not completely in an MSA central city	not in an MSA central city	population density <150 persons/km²	not completely in an MSA
dependent variable	Household Gini	Household Gini	Household Gini	Household Gini	Household Gini	Household Gini	Household Gini	Household Gini	Household Gini
resource sector employment share	-0.003***	-0.003***	-0.002***	-0.003***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002**
	(0.001)	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
↑ resource sector employment share	-0.002	0.001	0.001	0.000	0.001	0.000	-0.000	-0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
↓ resource sector employment share	-0.005***	-0.004***	-0.003***	-0.004***	-0.003***	-0.003***	-0.002***	-0.003***	-0.002***
	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
PUMA FEs	yes	yes	yes	yes	yes	yes	yes	yes	yes
year FEs	yes	yes	yes	yes	yes	yes	yes	yes	yes
N	2,411	8,992	11,082	14,655	18,238	15,827	9,246	7,156	3,583
PUMAs	304	1,136	1,402	1,862	2,332	2,028	1,196	930	470
States	38	48	45	48	49	48	48	48	45
T	8	8	8	8	8	8	8	8	8
within-R^2 symmetric	0.160	0.177	0.173	0.188	0.207	0.226	0.246	0.279	0.315
within-R^2 asymmetric	0.165	0.188	0.179	0.195	0.213	0.233	0.252	0.285	0.321
p-value F-test: symmetry	0.116	**0.000**	**0.000**	**0.000**	**0.000**	**0.000**	**0.000**	**0.000**	**0.000**

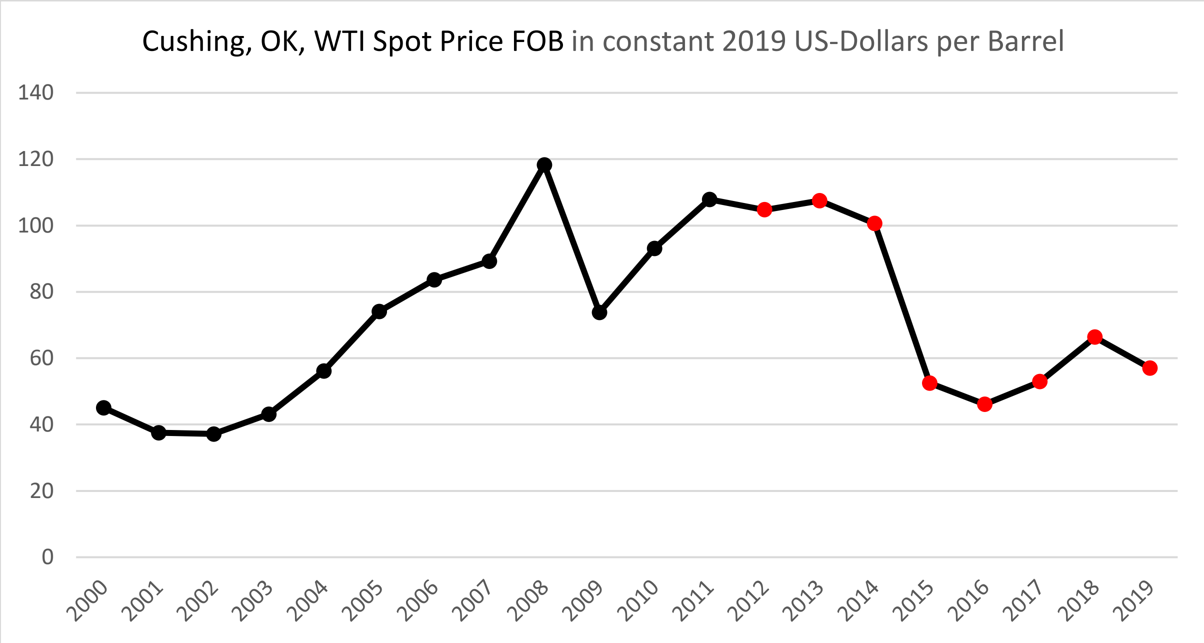
The first four columns report results for urban subsamples according to the definition listed above each regression specification's results. The last four columns report rural subsample results. The respective number of included PUMAs and states are also reported. The latter are all the states that have at least one PUMA according to the definition of the respective column. The household Gini is the pre-tax household-income Gini. The first line of coefficients presents symmetric ones, whereas the following two present the asymmetric ones. Usual controls included. Standard errors clustered at the state level are in parentheses. The null hypothesis of the F-test are symmetric resource sector employment share coefficients. * p < 0.1, ** p < 0.05, *** p < 0.01

Table 3.13: Household Income Shares Results

	(1) House-hold Gini	(2) House-hold GE(2)	(3) House-hold Q1	(4) House-hold Q2	(5) House-hold Q3	(6) House-hold Q4
resource sector	-0.002***	-0.008**	0.011	0.048***	0.106***	-0.149***
employment share	(0.000)	(0.003)	(0.011)	(0.013)	(0.032)	(0.054)
↑ resource sector	0.001	-0.005	-0.034**	0.012	0.099**	-0.062
employment share	(0.000)	(0.003)	(0.014)	(0.018)	(0.038)	(0.059)
↓ resource sector	-0.003***	-0.008**	0.019*	0.053***	0.108***	-0.165***
employment share	(0.000)	(0.003)	(0.010)	(0.012)	(0.031)	(0.049)
Controls	yes	yes	yes	yes	yes	yes
PUMA FEs	yes	yes	yes	yes	yes	yes
year FEs	yes	yes	yes	yes	yes	yes
N	18,238	18,238	18,238	18,238	18,238	18,238
PUMAs	2,332	2,332	2,332	2,332	2,332	2,332
States	49	49	49	49	49	49
T	8	8	8	8	8	8
within-R^2 symmetric	0.207	0.088	0.017	0.043	0.071	0.075
within-R^2 asymmetric	0.213	0.089	0.020	0.044	0.072	0.077
p-value F-test: symmetry	**0.000**	**0.023**	**0.000**	**0.018**	0.613	**0.005**

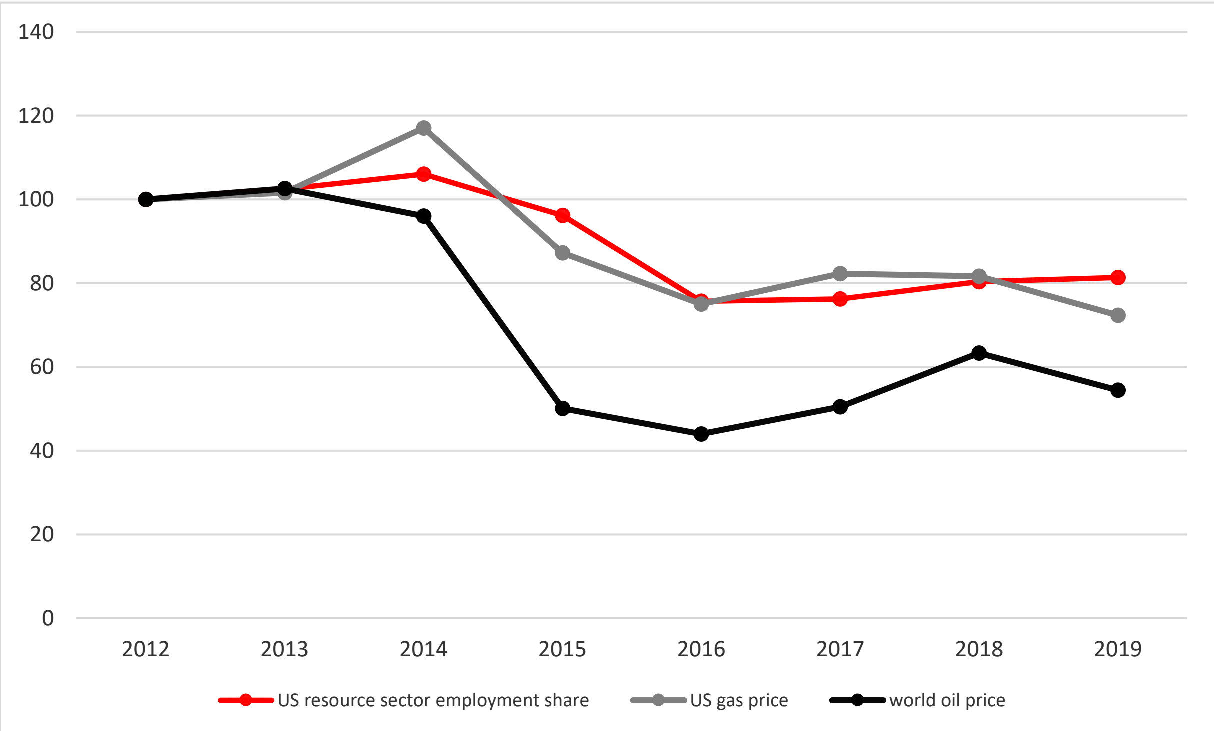
The household Gini is the pre-tax household-income Gini. Household GE(2) is the household-income Generalized Entropy Index with a=2, that is, half the square coefficient of variation. Household Qx is the household income share of the x^{th} income quartile in percent. One is the bottom-most income group, whereas four is the top one. The first line of coefficients presents symmetric ones, whereas the following two present the asymmetric ones. Controls include per capita income, population, demographic structure, educational attainment, and ethnic diversity at the PUMA level. The sample is restricted to the 48 contiguous US states plus Washington DC. Standard errors clustered at the state level are in parentheses. The null hypothesis of the F-test are symmetric resource sector employment share coefficients. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 3.3: Annual Crude Oil Price Average 2000—2019



Red dots mark the study period of the paper. *Source: own elaboration based on EIA data* (2021a)

Figure 3.4: Evolution of the US Resource Sector Employment Share, the US Gas Price, and the World Market Oil Price 2012—2019



The US resource sector employment share is the national-level share for the whole US as provided by the ACS. The US gas price is the annual US natural gas Citygate price average in dollars per thousand cubic feet (inflation-adjusted). The world oil price is the annual crude oil price (Cushing, OK WTI Spot Price FOB) average in US-Dollars per Barrel (inflation-adjusted). All the variables have been normalized so that their 2012 level corresponds to 100. *Source: own elaboration based on ACS and EIA data* (US Census Bureau, n.d.-b; US Energy Information Administration, 2021a, 2021b)

4 THE AFFORDABLE CARE ACT MEDICAID EXPANSION AND INTERSTATE MIGRATION IN BORDER REGIONS OF US STATES[24]

In the wake of the Affordable Care Act, some US states expanded Medicaid eligibility to low-income, working-age adults while others did not. This study investigates whether this divergence induces migration across state borders to obtain Medicaid, especially in border regions of expansion states. It compares border with interior regions' in-migration in the concerned subpopulation before and after the Medicaid expansion in linear probability difference-in-difference and triple difference regression frameworks. Using individual-level data from the American Community Surveys over 2012—2017, this study finds only a statistically significant increase in in-migration to border regions after the expansion in Arkansas. The differing results across states could stem from statistical power issues of the employed regression analysis but might also result from state peculiarities. For Arkansas, the odds of having migrated increase after the Medicaid expansion by 48 % for individuals residing in its border regions compared to before and control regions. If all these additional migrants take up Medicaid, the number of Medicaid beneficiaries in these regions increases by approximately 4 %. Thus, even if the induced migration is statistically significant, it appears unlikely to impose meaningful fiscal externalities at the regional level.

JEL classification: H5, H75, I13, R23

Keywords: Affordable Care Act, Medicaid expansion, interstate migration, border analysis, USA

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4.1 Introduction

States with relatively high welfare benefits are long believed to attract low-income individuals from lower-benefit states, acting as "welfare magnets" (Armenter & Ortega, 2010; Borjas, 1999; Brown & Oates, 1987). Similar reasoning might apply to the expansion of public health insurance programs such as Medicaid. Notably, the Patient Protection and Affordable Care Act (ACA) includes a Medicaid expansion from 2014 onward to low-income, non-disabled, working-age adults. However, not all states decided to implement it. Should expansion states then worry about an inflow of low-income individuals attracted by Medicaid?

Previous studies could not find any state-level migration effects induced by Medicaid expansions (Alm & Enami, 2017; Goodman, 2017; Schwartz & Sommers, 2014). However, much of this migration would arise in regions at the state border (McKinnish, 2005). This phenomenon can strain border regions considerably, even if the overall state-level migration effects are negligible.

Therefore, this paper aims at evaluating ACA Medicaid-induced border migration. It compares the in-migration of low-income, working-age individuals to border and interior regions of the same state before and after the ACA in difference-in-difference and triple difference frameworks based on individual-level American Community Survey (ACS) data (Ruggles et al., 2020).

This paper is not the first to study border migration induced by the ACA Medicaid expansion. Most notably, Goodman (2017) considers migration of individuals from border regions to border regions by restricting its sample accordingly. However, this substantially decreases the available number of observations and results in statistical power issues, making it impossible for Goodman (2017) to identify any border migration effects.

To overcome these issues, this paper uses the border-versus-interior-regions approach suggested by McKinnish (2005, 2007) and adapted to the 2006 Massachusetts Medicaid expansion by Alm & Enami (2017). This approach compares the evolution in migration rates to border regions to the ones of interior regions for the state enacting a reform. At a state border with a Medicaid coverage difference, the border regions on the expansion side should attract more in-migrants after the Medicaid expansion than before, and this increase should be larger than in interior regions. Using this approach, Alm & Enami (2017) could identify border migration effects for the Massachusetts Medicaid expansion.

The contribution of the present study to the literature is twofold. First, it is the first one that applies the border-versus-interior-regions approach to the ACA Medicaid expansion and evaluates Medicaid migration effects for five states at once (Arkansas, Illinois, Iowa, Maryland, and New Mexico). Second, the present study extends until 2017, adding three more years to Goodman's (2017) observation period. This allows both to increase the number of observations and study slightly longer-term effects of the Medicaid expansion on migration.

This paper can only identify a statistically significant, positive Medicaid migration effect for Arkansas. The other states exhibit insignificant migration effects, which sometimes even turn negative, indicating that no Medicaid migration occurs. The differing results across states could stem from statistical power issues but might also result from state peculiarities.

For Arkansas, the odds of having migrated increase post-ACA by 48 % in concerned border regions compared to before the ACA and interior regions. However, the effect is small in the aggregate due to the baseline migration odds of about 0.05. If all additional migrants take up Medicaid, the number of Medicaid beneficiaries in these regions increases by less than 4.2 %. This increase seems manageable, probably not imposing a meaningful fiscal externality on regional budgets.

The following section discusses the existing literature on public services migration in the US in more detail. Section 4.3 provides background on the ACA Medicaid expansion, the data, and study units, while section 4.4 presents the empirical strategy and discusses the parallel trend test results. Section 4.5 presents the main results, while section 4.6 discusses the reasons for the many insignificant results. Section 4.7 performs robustness checks, while section 4.8 concludes, and 4.9 is the appendix.

4.2 Public Services and Internal Migration

The basic economic migration model constitutes migration decisions as a cost-benefit analysis. In this framework, individuals weigh the costs and benefits of their location options and migrate when the benefits from relocation outweigh the costs (Molloy et al., 2011; Tiebout, 1956). Three main reasons for migration emerge from the literature: economic opportunity, public goods/services provision, and natural amenities. Public health insurance, such as Medicaid, can be considered a special kind of public services.

Public services migration also includes welfare migration, which has been studied extensively. It is related to the welfare-magnet theory. States with relatively high welfare benefits should attract low-income individuals from low-benefit states while concurrently retaining those already living in the state (Kennan & Walker, 2013). Thus, they act as "magnets" for low-income individuals. This could lead to a race to the bottom in setting benefit levels due to competition among states (Armenter & Ortega, 2010; Bailey & Rom, 2004; Saavedra, 2000).

Welfare migration is more likely to occur to and from border regions. In this case, interstate migration costs are lower as physical relocation and information costs are lower, while networks are more likely to persist (Baker, 2020; Greenwood, 1997; McKinnish, 2005). Welfare recipients have limited financial means, restraining their possibility of moving long distances (McKinnish, 2005, 2007; Snarr & Burkey, 2006). Concurrently, state public policies, such as welfare benefits and Medicaid coverage, change abruptly at the state border. This results in appreciable differences in benefit levels within a short-distance move (McKinnish, 2005). In sum, border regions play a particular role in welfare migration as comparatively short-distance moves can already lead to significant changes in the policy environment while only small migration costs arise.

There exists a sizable empirical literature on welfare migration. Welfare benefit generosity appears to have a positive but moderate effect on migration (Bailey, 2005; Brueckner, 2000; De Jong et al., 2005; Gelbach, 2004; Kennan & Walker, 2010). This effect's scale is typically not large enough to matter in the aggregate for states' budgets (Gelbach, 2004).

An innovative approach by McKinnish (2005, 2007) evaluates welfare migration by exploiting that welfare migration is more likely to occur in border regions. It compares welfare participation rates in state border counties to interior counties' ones for both high- and low-benefit states. More welfare-generous states should have higher welfare participation rates in their border regions compared to both interior ones and the neighboring low-benefit states' regions due to short-distance interstate border migration (McKinnish, 2005). This hypothesis is tested by estimating a difference-in-difference model. This approach de facto underestimates the actual migration effect as some migrants might also move to or from interior regions. Another study extends this framework to individual-level microdata and adds a time dimension by using observations from the 1980 and 1990 Censuses (McKinnish, 2007). The thereby obtained results are similar to other studies on welfare migration in finding significant but small welfare-migration effects (McKinnish, 2005, 2007).

Medicaid might also induce migration. This migration is not necessarily identical to the welfare benefits one as the target group and eligibility rules differ (Goodman, 2017). Potential and current Medicaid beneficiaries tend to move, ceteris paribus, to states with higher Medicaid benefits (Cebula & Clark, 2013). The decision to expand Medicaid might hence induce in-migration to an expansion state.

There were already Medicaid expansions in selected US states before the ACA. Several studies have exploited these policy changes to analyze their effect on interstate migration. They do not find any state-level effect of Medicaid expansion on migration (Alm & Enami, 2017; Schwartz & Sommers, 2014). Global effects could neither be found for the ACA Medicaid expansion, except for particular subpopulations such as homeless individuals (Baker, 2020; Goodman, 2017; Kumar, 2021). However, states are quite large and far apart, rendering migration mostly long-distance and hence costly.

Relevant border migration might nevertheless occur as it is less costly (Baker, 2020). Border migration might be so locally concentrated that migration flows are insignificant at the state level. They might still distinctively impact border regions as the latter have to cope with the low-income migrant influx and its associated costs (for instance, for hospital infrastructure, roads, housing, utilities provision, and police, while local taxes do not increase proportionally). Border migration effects have been found for the 2006 Massachusetts Medicaid expansion in border cities (Alm & Enami, 2017). These effects are identified by comparing the population growth of low-income individuals in border cities to its growth in Massachusetts's interior cities before and after the reform in a difference-in-difference framework in the spirit of McKinnish (2005, 2007). If Medicaid-expansion-induced border migration occurs, this population growth should be ceteris paribus higher in border cities compared to interior ones and pre-expansion growth rates. Empirically, a relatively large migration effect appears for cities close to the border. However, it decreases rapidly with increasing distance to the border and disappears completely beyond 25 km from the border (Alm & Enami, 2017). The results also hold when employing triple differences by additionally comparing to either population growth of higher-income individuals or the population growth pattern in neighboring, non-expansion states (Alm & Enami, 2017).

In contrast, Goodman (2017) cannot isolate any border migration effects for the ACA Medicaid expansion in 2014. The study assesses border migration by restricting its sample to border regions only. It analyzes commuting zones that straddle expansion/non-expansion state borders, respectively, Public Use Microdata Areas (PUMAs) with a population-weighted centroid within a certain distance from these borders (75, 150, and 250 km). Potentially Medicaid-eligible individuals from these border regions are not significantly more likely to migrate from a non-expansion to an expansion state than before the reform (Goodman, 2017). However, small border migration effects cannot be ruled out due to large confidence intervals (Goodman, 2017).

Goodman's (2017) approach appears the most intuitive one to study the particular effects of border migration. However, it substantially decreases the number of available observations, resulting in statistical power issues and rendering the identification of border migration effects difficult even though they might exist. Therefore, another approach is needed to identify potential migration effects of the Medicaid expansion. Using the border-versus-interior-regions approach, Alm & Enami (2017) were able to isolate a border migration effect for a Medicaid expansion by comparing border to interior regions.[25]

Consequently, the present paper employs the approach of Alm & Enami (2017) and McKinnish (2005, 2007) and applies it to the ACA Medicaid expansion to gain a more precise

[25] Alm & Enami (2017) can probably identify a border migration effect because of the larger amount of usable data per state with the border-versus-interior-regions approach. Even though the size of the treatment group remains the same, this approach allows for a more precise estimation of the control variables' effect on the probability of migrating, leading to a better isolation of the Medicaid migration effect.

picture of the latter's border migration effects. The approach will be adapted to the present case by using individual-level migration data with a more precise identification of the affected subpopulation, employing PUMAs to delimit regions, and studying five expansion states at once. The present paper follows otherwise closely Goodman's (2017) overall study set-up. This includes the data source (IPUMS USA ACS), individual-level data, subsample, and most control variables.

The present study also assesses whether the diverging results of Goodman (2017) and Alm and Enami (2017) are due to the considered states or the employed methodology. If the present study obtained statistically significant results, the technique used to study Medicaid border migration would matter. If the obtained results were not significant, there would be something special about the Massachusetts reform or about studying cities as opposed to PUMAs.

Furthermore, the present study extends until 2017, adding three more years to Goodman's (2017) observation period. This allows pooling years, increasing the number of observations and smoothing yearly fluctuations in the migration rate. Besides, this permits studying slightly longer-term migration effects of the Medicaid expansion. Migration effects might not be visible until 2015 due to increasing public awareness about Medicaid differences across states (Baker, 2020; Goodman, 2017; Kumar, 2021).

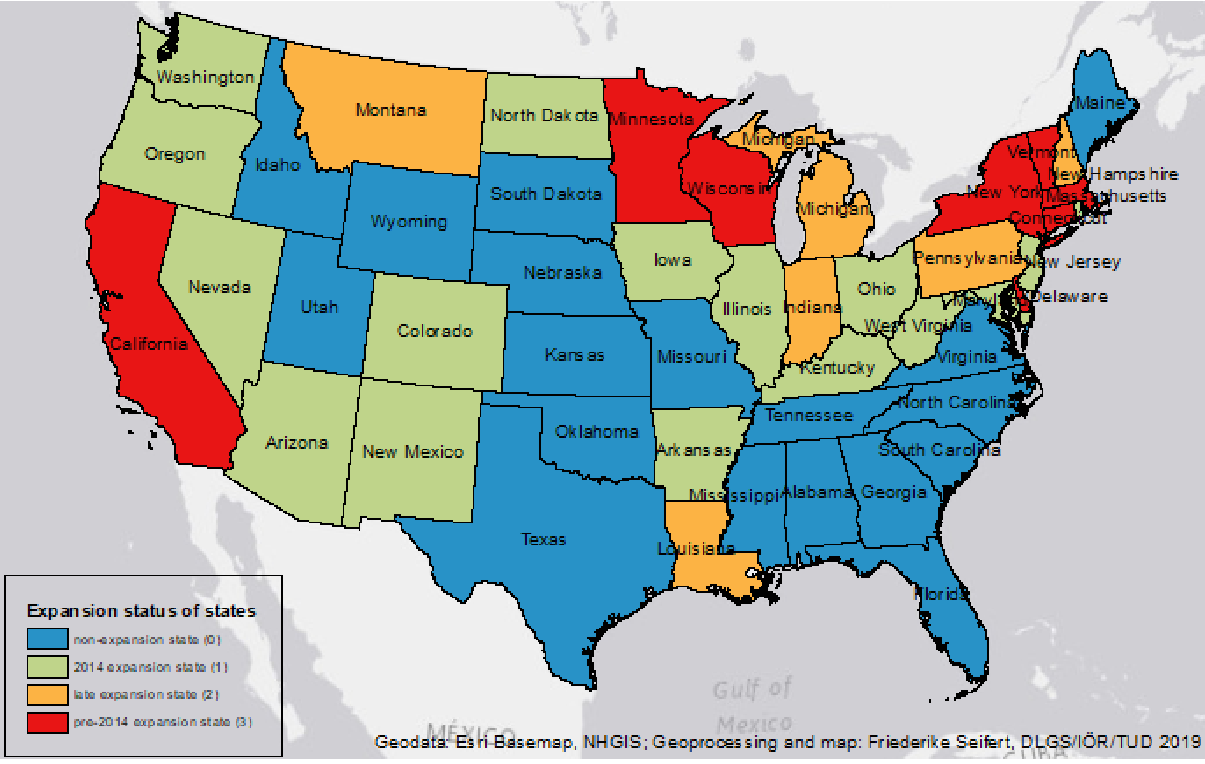
4.3 The Medicaid Expansion of the ACA, Data Sources and Study Unit

The ACA is a health insurance reform passed in March 2010. Its Medicaid expansion was implemented for the first time on January 1, 2014. The ACA aims at increasing health insurance coverage as previously approximately one-fifth of the non-elderly population was uninsured (Duggan et al., 2019). It includes several provisions, including expanding the public health insurance program Medicaid to previously ineligible parts of the population. This concerns working-age, non-disabled adults with a gross income of their Health Insurance Unit (approximately a family) equal to or below 138 % of the federal poverty (guide)line (FPL) (Leung & Mas, 2016). The ACA's further measures include health insurance subsidies to those with slightly higher income levels (between 100 % and 400 % of the FPL), private health insurance market reforms, and penalties on individuals without insurance (Duggan et al., 2019). Before the reform, working-age, non-disabled adults could either obtain health insurance through their employers (though not all offered it) or conclude insurance themselves (relatively expansive and given no severe pre-existing conditions) (Leung & Mas, 2016). In Medicaid non-expansion states, adults below the FPL are not eligible for any health insurance aid, thus potentially without coverage. Those between 100 and 138 % of the FPL can resort to subsidies, which are less advantageous for the individual than Medicaid (Goodman, 2017; Leung & Mas, 2016).

This paper focuses on the Medicaid expansion effects as this sub-program has an explicit cutoff and beneficiary group. It has been implemented uniformly across all states that chose to expand. The Medicaid expansion is a means-tested program and involves no cost-sharing for the beneficiary as the health insurance premiums are essentially equal to zero (Duggan et al., 2019). There is no blocking period for Medicaid. If one moves to another state, a new application is needed, but no minimum residency is required (Stringfellow, 2017).

Initially, an expansion of Medicaid for all US states was intended. However, the Supreme Court ruled in 2012 that states may choose whether to expand Medicaid or not (Goodman, 2017). A considerable number of states then decided not to expand Medicaid. Some states already had state-level provisions expanding Medicaid before the ACA, which were converted into the ACA Medicaid program. In the expansion states, the federal government fully finances the Medicaid expansion from 2014 until 2016. Afterward, federal funding gradually declines. From 2020 onward, the federal government covers 90 % of the Medicaid expansion and the respective state the rest (Goodman, 2017).

Figure 4.1: Map of US States by Expansion Status



Expansion status is assigned after Black et al. (2019) and The Henry J. Kaiser Family Foundation (2019). The present paper can ultimately only analyze five expansion states: Arkansas, Illinois, Iowa, Maryland, and New Mexico. For der underlying reasons, see the main text.

The expansion and non-expansion states are geographically dispersed (see figure 4.1). To give an overview of the Medicaid expansion status:

- 10 states (including the District of Columbia) had already expansions in place before 2014,
- 16 states expanded in January 2014,
- 7 states expanded later (2014—2016), and
- 18 states have not expanded until the end of 2017 (Black et al., 2019; The Henry J. Kaiser Family Foundation, 2019) (see appendix table 4.4 for a complete list of states).

The present paper uses individual-level ACS data retrieved from IPUMS USA (Ruggles et al., 2020). The ACS is a survey of a 1 % sample of the US population. It is representative of any place with a population larger than 65,000 (US Census Bureau, 2018). This data source provides yearly data and information on migration over the previous year. It is not possible to track individuals across years due to confidentiality protection.

The ACS provides data on health insurance**.** This data includes whether the individual in question is below the Medicaid eligibility threshold, is covered by Medicaid, and whether she has any health insurance coverage. The ACS further supplies a wide range of socioeconomic variables such as age, sex, race, income, marital status, family size, and educational attainment.

The studied subsample includes only individuals potentially eligible for the Medicaid expansion: income below or equal to 138 % FPL, and between 18 and 64 years of age. One eligible individual per Health Insurance Unit was selected randomly to reduce the design factor.

The smallest identifiable geographic unit in the data are PUMAs. A PUMA is a statistical entity consisting of one or more counties or census tracts combining a population of at least 100,000 (IPUMS-USA, n.d.). If a county has less than 100,000 inhabitants, it is merged with another county to create a PUMA. If an area has more than 200,000 inhabitants, it is split (IPUMS-USA, n.d.). No PUMA crosses state boundaries. Between 2011 and 2012, the PUMA delineations changed to ensure further compliance with the PUMA definition cited above (IPUMS-USA, n.d.). This study uses only the newer PUMA delineation to ensure consistency and covers the period 2012—2017.

Medicaid-induced migration might have occurred in the studied subpopulation before 2012 only due to single states passing state-level Medicaid expansion laws. The ACA Medicaid expansion was thought to cover all states uniformly before the Supreme Court ruling in June 2012. Thus, any anticipatory migration effects related to the ACA should only start in the second half of 2012 and ultimately in 2013. Data from 2013 might already have a partial treatment effect included (Goodman, 2017). However, the mean and median ACS interviews are conducted in June of the considered year (Goodman, 2017). Interviewees were always asked whether they moved within the last 12 months. Thus, 2014 interviews will pick up several moves that happened during 2013.

States are classified into non-expansion, expansion, late-expansion, and early- (pre-ACA-) expansion states, as shown in figure 4.1. This study focuses on the migration effects in expansion states. A border region in this study is a PUMA whose population-weighted centroid is less than 40 km away from the state border. This threshold is based on the work on welfare migration by McKinnish (2005, 2007), which uses the same threshold.

The treatment PUMAs of interest are the border PUMAs in an expansion state that border a non-expansion state. PUMAs bordering a late- or an early-expansion state might have distinct migration effects.[26] Therefore, they are excluded from the analysis sample. It is also possible that a PUMA borders two different states with different expansion statuses. If this involves a late- or early-expansion state, the PUMA is excluded from the analysis sample to avoid diluting the estimated migration effect. PUMAs bordering another expansion state should not experience any change in migration due to no Medicaid-related incentives to migrate. They are part of the control group together with the interior PUMAs. In sum, the following classification scheme has been applied to the PUMAs:

- bordering a late-expansion state: excluded from the analysis sample,
- bordering an early-expansion state: excluded from the analysis sample,
- bordering a non-expansion state, while not falling in the two categories above: treatment group (referred to hereafter as treated border regions),
- bordering only another expansion state: control group (control regions), and
- not bordering any state: control group (control regions).

Half of the sixteen expansion states do not have any treated border regions and are excluded ex-ante from the analysis[27]. The retained eight expansion states at this stage are Arkansas, Illinois, Iowa, Kentucky, Maryland, New Mexico, Washington, and West Virginia. Maps in the appendix show treatment and control regions.

4.4 Empirical Strategy

This paper investigates whether the treated border regions of expansion states attract more in-migrants after the ACA compared to both their pre-ACA migration rates and the

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[26] Expansion-state PUMAs bordering a late-expansion state should experience some Medicaid-induced in-migration in the early years until it became clear that the neighboring state will also expand. PUMAs bordering an early-expansion state might experience some in-migration by individuals, who migrated to the neighboring state to obtain Medicaid and now return back. However, this migration effect should be smaller than the original Medicaid-expansion one as migration is costly and many individuals probably do not return.

[27] New Jersey, Ohio, and Rhode Island do not border any non-expansion states. The population-weighted centroids of PUMAs bordering non-expansion states in Arizona, Colorado, Nevada, North Dakota, and Oregon are more than 40 km away from the border. The PUMAs there are very rural and hence very large.

migration evolution in these states' interior regions. Empirically, this is studied in a difference-in-difference framework. The first difference in this framework is between pre-and post-ACA years. The second difference is between treatment and control regions in expansion states. The interaction of these two differences then captures the effect of the ACA Medicaid expansion on migration.[28]

More precisely, the estimated model is the following:

$$y_{irst} = \alpha + \beta_1 post_t + \beta_2 border_r + \delta(border_r * post_t) + \theta \boldsymbol{X}_{it} + \mu_s + \varepsilon_{irst} \quad \text{(Equation 4.1)}$$

where y_{irst} is a dummy taking the value of one if an individual *i* residing in region *r* of state *s* at time *t* has migrated across any state border but within the US in the past 12 months before the interview in the considered year. Otherwise, the dummy takes the value of zero. Migrants from outside the US are dropped from the data set. Thus, the dummy captures in-migration in the past year.[29]

$post_t$ is a dummy taking the value of one for post-reform (treatment) years (2014—2017) and zero otherwise (2012 and 2013).

$border_r$ is the treatment region dummy, taking the value of one if an individual resides in a treated border PUMA as defined above. Otherwise, the dummy takes the value of zero. The dummy is coded as a missing value for individuals residing in PUMAs bordering late- or early-expansion states.

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[28] The present study hypothesizes that the migration induced by the Medicaid expansion is predominantly a border regions' one. Almost no state-level migration effects can be identified for Medicaid-expansion migration between expansion and non-expansion states for the present analysis sample (see appendix table 4.5). This is in line with former studies (Alm & Enami, 2017; Goodman, 2017; Schwartz & Sommers, 2014). Thus, the migration flows to interior regions appear to be negligible and are hence neglected in the present setup in favor of estimating a differential effect.

[29] If only migrants from neighboring non-expansion states are considered, the number of migrants drops so low for single states that no meaningful regression analysis can be run. Thus, this analysis retorts to the broader migration definition presented above. Even if the migrants' origin is not controlled for, they are still more likely to migrate to border regions ceteris paribus as all the other differences in potential pull-factors are controlled for by the difference-in-difference nature of the framework. This would not be the case if Medicaid migrants more than proportionally favored cities compared to other migrants. However, no evidence can be found for this (see robustness check subsection 4.7.2). Instead, a slight advantage of border regions still remains due to being close for migrants originating from the other side's border regions.

border$_r$**post*$_t$ is the interaction term of the *border* and *post* dummies. Its coefficient δ captures the treatment effect of the ACA Medicaid expansion on migration. δ is expected to be positive.

$\boldsymbol{X}_{it}$ is a vector of individual-level control variables detailed below. μ_s are state fixed effects (FEs), which are included when pooling several states together. ε_{irst} is the error term.

The difference-in-difference framework already captures a lot of variation between entities, especially those due to specific years (for instance, federal reforms or national economic downturns) or locations (treatment versus control regions, urban versus rural). It also captures baseline migration levels, which occur due to various other reasons than the studied Medicaid expansion (job, education, natural amenities, family ties, other public services). The difference-in-difference framework captures migration due to higher wage or lower price levels in certain regions if the respective differences in wages and prices are constant over time.[30] Besides, the state FEs capture state-specific, time-invariant characteristics when several states are pooled together.

Control variables for age, sex, race, income, marital status, family size, and educational attainment have been added as these factors all influence migration decisions (Foster, 2017; Molloy et al., 2011; Rosenbloom & Sundstrom, 2004). Including control variables improves precision by avoiding omitted variable bias and mitigating any effects of changes in the sample composition over time (Goodman, 2017).

This difference-in-difference framework can be expanded to a triple difference one by additionally comparing migration by higher-income individuals (between 200 and 400 % FPL) to migration by Medicaid-eligible ones. As the Medicaid expansion is almost exclusively financed by the federal government, the Medicaid expansion should not affect middle- and high-income individuals via state taxes, hence not resulting in any migration incentive. The triple difference set-up additionally controls for state-specific migration shocks unrelated to the Medicaid expansion. It requires adding a dummy for Medicaid

[30] Regional business cycle fluctuations might result in time-varying differences in income opportunities, which would pose a threat to identification. However, no effect of PUMA-level employment rates on migration and the obtained results can be identified (results available upon request).

eligibility and further interaction terms. The triple interaction term of Medicaid eligibility, treatment region, and post-ACA period now captures the treatment effect.

The corresponding regression equation is:

$$y_{irst} = \alpha + \beta_1 post_t + \beta_2 border_r + \beta_3 medicaid_i + \gamma_1(border_r * post_t) + \gamma_2(post_t * medicaid_i) + \gamma_3(border_r * medicaid_i) + \delta(border_r * post_t * medicaid_i) + \theta \boldsymbol{X}_{it} + \mu_s + \varepsilon_{irst}$$

(Equation 4.2)

Both frameworks require a linear probability model as the dependent variable in the regressions is binary. Both ordinary least squares (OLS) and logistic models are estimated. The calculated standard errors are robust and clustered at the state level when the regression includes several states.

Some prerequisites need to be fulfilled to estimate a valid difference-in-difference model. For instance, no self-selection into the treatment group should occur that might influence the estimated migration effect. Living in a treated border region previous to the ACA reform can be considered reasonably exogenous to the individual.[31]

Individuals might also self-select into eligibility for Medicaid by reducing their income to fall below the eligibility threshold. However, the literature can only identify tiny, if any, changes in labor supply after the ACA Medicaid expansion (Gangopadhyaya & Garrett, 2020; Gruber & Sommers, 2019; Kaestner et al., 2017). This renders it unlikely that a substantial number of individuals reduced their working hours, hence their income, to be eligible for Medicaid. However, a sample-selection effect due to income differences across regions is conceivable. An individual who lived in a high-income region 12 months before the ACS interview would be more likely to be in the selected sample if she migrated to a low-income region with fewer absolute earning opportunities. Conversely, an individual

[31] The Medicaid expansion of the neighboring state as well as the non-expansion of one's own state of residence might be unsurprising given the respective states' track record on public programs. Thus, residents of a state with a poor track record are less likely to out-migrate to obtain Medicaid as they have chosen to reside in a non-generous state in the first place. This kind of self-selection would lead to an underestimation of the Medicaid-induced migration flow. Any obtained estimate can be considered a lower bound in this sense. Nevertheless, no evidence can be found that surprising (non-) expansion states experience larger in-(out-) migration after the ACA.

would forego her Medicaid eligibility if her income rises above the eligibility threshold after migrating to a higher-income destination. In the latter case, the Medicaid-related incentive to migrate would be reduced. Both channels result in an upward bias of the estimated migration flow to a low-income region. However, there is no correlation between being a treated border region and having a lower average income.

The parallel trend assumption has also to hold to estimate the ACA Medicaid expansion's causal effect on migration. The in-migration rates to treated border and control regions should exhibit parallel trends before the expansion. There should neither be any third factor inducing differences between the treatment and control group concurrently with the reform. However, no major reform with a similar geographic distribution is happening simultaneously as the ACA.

Parallel trend tests have been conducted using data for 2008—2011 and the old PUMA delineations.[32] Difference-in-difference and triple difference event study regressions have been estimated to this effect. The overall set-up is similar to the main regressions' one, but now several year dummies and interaction terms are included: one for each year over 2008—2010, 2011 being the reference year. In the case of parallel trends, there should not be any time-varying differences in migration rates between the treatment and the control group relative to the 2011 difference in migration rates, conditional on the control variables. The treatment effect coefficients for all years are thus expected not to be statistically significant. This is almost always the case for the expansion states (see appendix table 4.6 for Arkansas and Maryland; results for the other states available upon request).

Tests of joint significance have been conducted for all treatment effect coefficients together. In the case of parallel trends, the tests should not reject the null hypothesis of the

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[32] Data on compliance with the Medicaid eligibility threshold is available from 2008 onward only. One cannot use the main data from 2012 and 2013 for the parallel trend tests as the PUMA delineations change between 2011 and 2012. Only four of the eight expansion states with treated border regions exhibit PUMA delineations that did not change or only in such a way as to not overly affect the borders of the treatment and control regions (Arkansas, Kentucky, New Mexico, Washington). For these four states, parallel trend tests over 2008—2013 have also been run. Their results are very similar to the here presented ones (see robustness check subsection 4.7.4 and the maps in the appendix).

treatment effect coefficients being jointly equal to zero (respectively equal to one for the odds ratios of logistic regressions). The p-values should hence be high, at least > 0.5. If these tests favor parallel trends in migration over 2008—2011, this suggests parallel trends up until the ACA.

The obtained p-values vary considerably across states (table 4.1), indicating differing parallelism strengths in pre-reform migration rate trends. In the difference-in-difference setup, only Arkansas and Maryland have p-values above 0.5. In the triple difference regressions, this is the case again for Maryland as well as for Illinois, Iowa, and New Mexico.[33] Therefore, the present study only examines regressions results for these five states.

Table 4.1: p-Values of Parallel Trend Tests per State

	Difference-in- Difference		***Triple Difference***	
State	***OLS***	***logistic***	***OLS***	***logistic***
Arkansas	0.67	0.62	0.35	0.44
Illinois	0.03	0.04	0.57	0.76
Iowa	0.31	0.28	0.62	0.74
Kentucky	0.10	0.14	0.02	0.03
Maryland	0.64	0.51	0.92	0.87
New Mexico	0.25	0.31	0.86	0.45
Washington	0.25	0.24	0.14	0.14
West Virginia	0.13	0.12	0.24	0.43

This table reports the p-values of F- (OLS regressions) respectively X^2- (logistic regressions) tests of joint significance of all the treatment effect coefficients together ($border * year$ for the difference-in-difference and $border * year * medicaid$ for the triple difference regressions) from event study regressions over 2008—2011 including controls. The higher the p-value, the stronger is the case for having parallel trends in migration rates of treatment and control regions before the ACA. Cells marked in green have p-values above 0.5, strongly hinting at parallel trends in migration rates before the ACA.

[33] Adding further control variables such as PUMA-level employment rates or time trends does not improve the parallel trend test results. Implementing more advanced econometric techniques such as synthetic control groups or state-level pre-treatment trends (Willage, 2020) is not possible due to the regional definitions and limited data. Thus, the proposed regression framework appears the best possible given the available data and the research question at hand.

4.5 The Medicaid Expansion Migration Effect

This paper hypothesizes that the Medicaid expansion increases in-migration rates of treated border regions compared to their pre-expansion levels and control regions' migration rate evolution. Thus, the treatment effect coefficient is expected to be positive.

Tables 4.2 and 4.3 report the difference-in-difference (columns 1 and 2) and triple difference (columns 3—7) results for the expansion states with treated border regions and satisfactory parallel trend test results. The states are presented in decreasing order of their parallel trend tests' p-values. Summary statistics for Arkansas and Maryland can be found in appendix table 4.7.

In the difference-in-difference regressions, both Arkansas and Maryland have, as expected, a positive treatment effect coefficient of Medicaid-expansion-induced migration. However, it is only statistically significant at the 5 % level for Arkansas. For the latter, the probability of having migrated to its treated border regions increases by 1.3 percentage points following the ACA compared to before and control regions according to the OLS regression. The baseline migration rate in these regions was 5.5 % in 2012. According to the logistic regression, the odds of having migrated increase by 48 % for individuals residing in these regions after the ACA compared to before and control regions. The *post* coefficients are not statistically significant in these regressions, indicating that overall migration has not increased in the post-ACA years (2014—2017). The *border* coefficient is only statistically significant and positive for Arkansas, indicating that migration to Arkansas' treated border regions has always been higher than to its interior ones. No such effect is discernable for Maryland.[34]

A back-of-the-envelope calculation for Arkansas shows that the additional number of Medicaid beneficiaries due to induced migration is inconsequential from a regional-level perspective. In 2012, 14,499 low-income individuals migrated interstate to treated border

[34] This insignificant effect might be due to Maryland only having one treated border PUMA, potentially leading to its insignificant *border* and treatment effects at the same time (discussed more in detail in section 4.6). However, one cannot conclude per se that an (in)significant *border* coefficient implies an (in)significant treatment effect as the dummy's task is to control for pre-existing differences in migration trends across the regions so that they do not interfere with the treatment effect.

regions in Arkansas, while 251,534 lived there already, resulting in a migration rate of 5.5 %. The Medicaid-induced migration rate increase by 1.3 percentage points amounts to 3,853 additional migrants. This increases the number of Medicaid-eligible working-age adults in treated border regions by 1.5 %. The total number of "native" Medicaid beneficiaries in these regions in 2015 equals 92,658. If all additional migrants take up Medicaid, the number of Medicaid beneficiaries increases by less than 4.2 %. The corresponding state-level shares are even smaller. Thus, the number of additional migrants due to the Medicaid expansion is statistically significant for Arkansas but is presumably not relevant size-wise for policymakers at the regional and state level.[35]

The difference-in-difference framework can be turned into a triple difference one by adding observations for higher-income, non-Medicaid eligible individuals as comparison group. Columns 3—6 report the results by state. The treatment effect's coefficient is now only statistically significant at the 10 % level for Iowa in the OLS regression, while it is insignificant in all the other cases. The coefficient even turns negative for Maryland (OLS) and New Mexico (OLS and logistic).

[35] If the migrants are sicker than the average "native" Medicaid-eligible population this would result in disproportionate costs for their future Medicaid coverage and other social assistance. However, no evidence for sicker individuals (i.e. self-reported disabled or older) being more likely to migrate can be found: neither in general nor following the ACA Medicaid expansion. To the contrary, sicker individuals are significantly less likely to migrate for Medicaid than healthy individuals. This is in line with Goodman's (2017) results and probably due to migrating being more costly and cumbersome, if not impossible, if you are sicker. Furthermore, some disabled individuals were already covered by Medicaid previous to the ACA.

Table 4.2: Pre- versus Post-ACA Migration: OLS

	(1) OLS migration	(2) OLS migration	(3) OLS migration	(4) OLS migration	(5) OLS migration	(6) OLS migration	(7) OLS migration
post	-0.005	0.001	0.006	-0.003	-0.000	0.001	0.001
	(0.004)	(0.003)	(0.005)	(0.005)	(0.004)	(0.002)	(0.001)
border	0.010**	0.002	0.008	-0.011	0.016*	0.005	0.006
	(0.005)	(0.011)	(0.011)	(0.010)	(0.009)	(0.005)	(0.005)
medicaid			0.012**	0.006	0.022***	0.013***	0.013**
			(0.005)	(0.006)	(0.006)	(0.003)	(0.003)
treatment DD	**0.013****	**0.005**	0.011	0.043***	-0.002	0.001	0.005
($border * post$)	(0.006)	(0.015)	(0.017)	(0.016)	(0.010)	(0.006)	(0.007)
	[0.001;0.025]	[-0.024;0.034]					
*border*medicaid*			-0.005	0.001	-0.020	-0.012*	-0.012**
			(0.016)	(0.013)	(0.014)	(0.006)	(0.004)
*post*medicaid*			-0.004	-0.000	-0.008	-0.004	-0.004**
			(0.006)	(0.006)	(0.006)	(0.003)	(0.001)
treatment DDD			**-0.006**	**-0.014**	**0.030***	**0.007**	**0.010**
($border * post *$			(0.022)	(0.020)	(0.017)	(0.008)	(0.007)
$medicaid$)			[-0.050; 0.038]	[-0.053;0.026]	[-0.003;0.063]	[-0.008;0.023]	[-0.012;0.032]
age	-0.000***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
male	-0.003	-0.001	0.002	0.001	0.001	0.001	0.001**
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.001)	(0.000)
white non-His-	-0.003	-0.004	0.004	0.032***	-0.016*	-0.008**	-0.000
panic	(0.008)	(0.013)	(0.009)	(0.004)	(0.009)	(0.003)	(0.010)
Hispanic	-0.012	0.014	0.017	-0.006	0.006	-0.007*	-0.006*
	(0.009)	(0.017)	(0.013)	(0.003)	(0.011)	(0.004)	(0.002)
Black	-0.011	-0.025**	-0.016*	0.008	0.006	-0.003	-0.004
	(0.009)	(0.012)	(0.008)	(0.010)	(0.012)	(0.004)	(0.010)

total family income (2014 $)	0.000	-0.000**	-0.000***	0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
married	0.013***	0.009	0.011**	0.008**	0.002	0.005***	0.006**
	(0.005)	(0.007)	(0.005)	(0.003)	(0.003)	(0.002)	(0.002)
family size	-0.002*	-0.005***	-0.006***	-0.006***	-0.006***	-0.004***	-0.005***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
bachelor plus	0.028***	0.041***	0.036***	0.027***	0.034***	0.024***	0.029***
	(0.009)	(0.010)	(0.007)	(0.005)	(0.005)	(0.002)	(0.004)
college plus	0.000	0.013***	0.013***	-0.005	-0.000	-0.001	-0.000
	(0.004)	(0.005)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)
high school diploma plus	0.004	0.004	0.004	-0.006	-0.000	0.002	-0.001
	(0.004)	(0.003)	(0.003)	(0.004)	(0.005)	(0.002)	(0.003)
constant	0.047***	0.079***	0.062***	0.084***	0.082***	0.046***	0.054***
	(0.011)	(0.016)	(0.012)	(0.010)	(0.012)	(0.005)	(0.006)
N	26,352	10,306	17,755	31,918	35,355	95,190	180,218
R^2	0.007	0.034	0.035	0.020	0.018	0.009	0.014
State	Arkansas	Maryland	Maryland	New Mexico	Iowa	Illinois	Maryland, New Mexico, Iowa, Illinois
p-value parallel trend test	0.67	0.64	0.92	0.86	0.62	0.57	-

The first two columns report the results from OLS difference-in-difference regressions of the form $y_{irst} = \alpha + \beta_1 post_t + \beta_2 border_r + \delta(border_r * post_t) + \theta X_{it} + \varepsilon_{irst}$ estimated at the individual level. Columns 3 to 7 report the results from OLS triple difference regressions of the form $y_{irst} = \alpha + \beta_1 post_t + \beta_2 border_r + \beta_3 medicaid_i + \gamma_1(border_r * post_t) + \gamma_2(post_t * medicaid_i) + \gamma_3(border_r * medicaid_i) + \delta(border_r * post_t * medicaid_i) + \theta X_{it} + \varepsilon_{irst}$ also estimated at the individual level. Column 7 additionally includes state FEs. The years 2014—2017 are considered *post* years, while 2012 and 2013 are not. *Medicaid* indicates Medicaid eligibility. The comparison group consists of individuals with an income between 200 and 400 % FPL. For more details, see the main text. Robust standard errors (additionally clustered at the state level in column 7) are in parentheses; 95 % confidence interval bounds are in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01

Table 4.3: Pre- versus Post-ACA Migration: Logistic

	(1) logistic migration	(2) logistic migration	(3) logistic migration	(4) logistic migration	(5) logistic migration	(6) logistic migration	(7) logistic migration
post	0.832	1.029	1.349	0.888	0.997	1.093	1.030
	(0.122)	(0.165)	(0.302)	(0.135)	(0.177)	(0.135)	(0.061)
border	1.357**	1.190	1.789	0.610	1.861**	1.321	1.358
	(0.208)	(0.440)	(0.777)	(0.257)	(0.532)	(0.363)	(0.302)
medicaid			1.805**	1.192	1.942***	1.959***	1.725***
			(0.425)	(0.199)	(0.350)	(0.259)	(0.204)
treatment DD	**1.484****	**1.163**	1.149	3.742***	0.918	1.038	1.226
($border{*}post$)	(0.283)	(0.559)	(0.608)	(1.846)	(0.320)	(0.347)	(0.309)
	[1.021;2.156]	[0.454;2.981]					
*border*medicaid*			0.686	1.097	0.539	0.552*	0.610***
			(0.395)	(0.589)	(0.213)	(0.185)	(0.086)
*post*medicaid*			0.766	1.009	0.822	0.809	0.860***
			(0.212)	(0.196)	(0.177)	(0.126)	(0.049)
treatment DDD			**1.021**	**0.643**	**2.005**	**1.413**	**1.396**
($border{*}post{*}$			(0.732)	(0.404)	(0.940)	(0.576)	(0.295)
$medicaid$)			[0.250; 4.164]	[0.188;2.205]	[0.800;5.025]	[0.636;3.142]	[0.922;2.112]
age	0.987***	0.956***	0.951***	0.965***	0.967***	0.972***	0.966***
	(0.003)	(0.006)	(0.005)	(0.004)	(0.004)	(0.003)	(0.003)
male	0.903	0.975	1.102	1.033	1.041	1.048	1.053***
	(0.083)	(0.149)	(0.133)	(0.090)	(0.092)	(0.069)	(0.009)
white non-Hispanic	0.932	1.038	1.263	2.380***	0.690**	0.767**	1.024
	(0.187)	(0.295)	(0.282)	(0.346)	(0.128)	(0.090)	(0.338)
Hispanic	0.720	1.640	1.809*	0.855	1.241	0.780	0.779*
	(0.183)	(0.630)	(0.562)	(0.130)	(0.287)	(0.126)	(0.107)
Black	0.695	0.407***	0.511***	1.380	1.270	0.978	0.889
	(0.157)	(0.130)	(0.132)	(0.432)	(0.311)	(0.162)	(0.325)

total family income (2014 $)	1.000	1.000	1.000*	1.000	1.000	1.000	1.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
married	1.475***	1.807**	1.885***	1.328**	0.976	1.284***	1.302***
	(0.197)	(0.518)	(0.357)	(0.157)	(0.129)	(0.123)	(0.124)
family size	0.941*	0.772***	0.763***	0.813***	0.840***	0.801***	0.800***
	(0.032)	(0.054)	(0.044)	(0.031)	(0.032)	(0.025)	(0.013)
bachelor plus	1.891***	1.875***	2.002***	1.975***	2.546***	2.612***	2.512***
	(0.317)	(0.375)	(0.315)	(0.228)	(0.296)	(0.231)	(0.118)
college plus	1.017	1.726***	1.915***	0.868	0.992	0.941	0.993
	(0.113)	(0.347)	(0.326)	(0.098)	(0.112)	(0.087)	(0.076)
high school diploma plus	1.163	1.561	1.474	0.859	0.926	1.128	0.986
	(0.163)	(0.496)	(0.436)	(0.138)	(0.155)	(0.150)	(0.098)
N	26,352	10,306	17,755	31,918	35,355	95,190	180,218
Pseudo-R^2	0.024	0.129	0.140	0.065	0.060	0.044	0.055
State	Arkansas	Maryland	Maryland	New Mexico	Iowa	Illinois	Maryland, New Mexico, Iowa, Illinois
p-value parallel trend test	0.62	0.51	0.87	0.45	0.74	0.76	-

The first two columns report the results from logistic difference-in-difference regressions of the form $y_{irst} = \alpha + \beta_1 post_t + \beta_2 border_r + \delta(border_r * post_t) + \theta X_{it} + \varepsilon_{irst}$ estimated at the individual level. Columns 3 to 7 report the results from logistic triple difference regressions of the form $y_{irst} = \alpha + \beta_1 post_t + \beta_2 border_r + \beta_3 medicaid_i + \gamma_1(border_r * post_t) + \gamma_2(post_t * medicaid_i) + \gamma_3(border_r * medicaid_i) + \delta(border_r * post_t * medicaid_i) + \theta X_{it} + \varepsilon_{irst}$ also estimated at the individual level. Column 7 additionally includes state FEs. The years 2014—2017 are considered *post* years, while 2012 and 2013 are not. *Medicaid* indicates Medicaid eligibility. The comparison group consists of individuals with an income between 200 and 400 % FPL. For more details, see the main text. Robust standard errors (additionally clustered at the state level in column 7) are in parentheses; 95 % confidence interval bounds are in brackets; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Column 7 of each table presents results for grouping the four considered states (Maryland, New Mexico, Iowa, and Illinois) together. These regressions include state FEs to control for time-invariant state characteristics and cluster standard errors at the state level. Pooling several states together can improve statistical precision by increasing the number of observations available relative to the number of included regressors. It also smooths out the migration rate fluctuations, further increasing the estimations' precision. The treatment effect coefficient is positive in the state group regressions. However, it is still not statistically significant at the 10 % level despite the standard errors being smaller than in the single-state regressions.

The *post* coefficients are not statistically significant in all triple difference regressions, indicating that overall migration has not increased in the post-ACA years (2014—2017). The *border* coefficient is only statistically significant at the 10 % level and positive for Iowa, indicating that migration to Iowa's treated border regions has always been higher than to its interior ones. No such effect is discernable for the other states. The Medicaid dummy's coefficients are always positive and, in most cases, statistically significant at the 5 % level. This result indicates that Medicaid-eligible individuals are generally more likely to migrate ceteris paribus than slightly higher-income individuals. This fact might be due to this group also including students who have low incomes but migrate relatively frequently across states for and after college. Furthermore, this effect is offset by the negative coefficients for the interaction terms of Medicaid eligibility with respectively *border* and *post*. The coefficient of the interaction of *border* and *post* is not statistically significant in these regressions, except for New Mexico (positive).

Overall, the obtained results do not allow for a straightforward interpretation. Some states do not exhibit a Medicaid migration effect, while others have a statistically significant effect or a tendency towards it. The following section discusses more in detail possible reasons for these diverging results. However, even if a statistically significant migration effect occurs, the absolute number of additional migrants is so small in the aggregate that it seems manageable at the regional and state level. Thus, Medicaid migrants do not appear to impose a meaningful fiscal externality on these budgets. Excessively attracting low-income individuals due to the Medicaid expansion is unlikely.

The obtained results mostly correspond to Goodman's (2017) insignificant and imprecise ones. The positive, but small in aggregate size, migration effect for Arkansas aligns with Alm & Enami (2017) and the empirical welfare migration literature (Gelbach, 2004). However, its border migration effect is not as clear-cut as in McKinnish (2005, 2007) and Alm & Enami (2017).

4.6 Discussion

Statistical power issues might be partly responsible for the mostly statistically insignificant results, visible in the large standard errors. These issues might be due to several reasons.

First, the number of migrant observations for a single state is relatively small in the considered low-income sample. Arkansas has the most migrant observations in treated border regions of any analyzed state. Still, it has only about 150 migrant observations in treated border regions per year, although almost half of Arkansas' regions are treated border ones (appendix table 4.8). Note that this number is not proportional to the total number of migrants in these regions due to the applied weights in the ACS (duly considered in the regressions). Furthermore, this number includes all interstate migrants, also those migrating for other reasons than Medicaid. At the extreme, Maryland has only 48 migrant observations in treated border regions across the whole study period. This number still fulfills the rule of thumb of a minimum of 10 cases for a regression analysis but is far from ideal, potentially leading to Maryland's statistically insignificant treatment effect and *border* dummy coefficients.

Second, the relatively small number of migrants also leads to random fluctuations in the annual migration rates, which are only incompletely smoothed by pooling the years together. Thus, the migration rate is volatile in these regions as individual decisions matter more for the observed migration rate in small samples. This volatility increases the noise in the regressions. The very low R^2 and Pseudo-R^2 of the regressions (at the most 0.04 and 0.14 respectively) illustrate the difficulties of explaining migration decisions even with control variables included. Why one individual migrates and another not remains to a high degree unpredictable as many unobservables influence this decision. Again, this effect would be particularly strong for Maryland with its few migrant observations.

Third, logistic regressions might be particularly prone to small sample bias as they are based on the Maximum Likelihood approach. OLS regressions have their own issues in the linear probability case, especially when the mean of the dependent variable is close to 0 or 1, as in the present case with a migration probability below 6 %. These shortcomings aggravate the discussed identification issues.

Fourth, it is more difficult to identify a treatment effect in difference-in-difference regressions if the considered groups are very different in size. The larger the difference in the respective group sizes, the stronger the treatment effect must be to be detectable. For instance, Maryland has only one treated border PUMA, constituting 10 % of all included observations. In contrast, the ratio is almost 50:50 for Arkansas, hence much more well-balanced. The absolute number of observations in all subgroups is also higher in Arkansas. Thus, it is easier to identify a treatment effect for Arkansas than for Maryland, given that one exists. The Maryland case appears also underpowered according to the standard formula for the minimum detectable effect. Furthermore, the border-versus-interior-regions approach underestimates the migration effect by ignoring migration to interior regions, exacerbating this issue.

Statistical power issues are not the only explanation for statistically insignificant results. It might also be that simply no migration effect exists in Maryland. Maryland's estimated treatment effect coefficients are smaller than the Arkansas ones. Furthermore, New Mexico and Illinois also exhibit insignificant treatment effect coefficients while they are only once marginally significant for Iowa. An inexistent Medicaid migration response would not necessarily imply that the concerned individuals do not value Medicaid. The involved migration costs might be too high and the obtained benefits too uncertain for the considered low-income group (Finkelstein et al., 2019; Goodman, 2017). Information deficits about the program and one's eligibility in another state, as well as hopes for future expansions in one's state of residence, might also play a role (Baker, 2020; Goodman, 2017).

Arkansas' Medicaid migration effect might constitute a peculiarity, maybe due to unobserved regional conditions, or be spurious. Differences across states in advertising the Medicaid expansion might lead to differing migration effects (Baker, 2020). Third factors, such as the presence of children, youth, Medicaid insurance status, immigration, commuting, or the PUMA-level unemployment rate, cannot explain the differing results across states. Their inclusion into the regression does not affect the estimated migration effects (cf. robustness check subsection 4.7.2).

Having said that, Arkansas's migration effect is strong and robust, rendering it impossible to neglect it and rule out any migration effects altogether. Although statistically insignificant, Iowa and Illinois also exhibit positive migration effects of comparable size or even larger. However, even for Arkansas with its strong migration effect, the aggregate absolute number of additional migrants remains small and appears manageable at the regional level.

4.7 Robustness Checks

This section presents the results of several robustness checks. The first subsection considers different border cutoffs than 40 km, while the second discusses regression results for different subsamples. The third subsection examines pseudo-regressions for intrastate migration, while the fourth considers regressions over the whole 2008—2017 period for selected states.

4.7.1 Different Border Cutoffs

A border region in this study is a PUMA whose population-weighted centroid is less than 40 km away from the state border. This threshold is based on McKinnish's (2005, 2007) work on welfare migration. However, Alm & Enami (2017) do not find any migration effect for the Massachusetts Medicaid reform after 25 km. The advantage of a more restrictive border definition is its increased likelihood of picking up border migration, which typically declines with distance. Its disadvantage is the reduced number of observations in treatment regions. Therefore, the main regressions have been repeated with mutually exclusive 10 km border intervals up to 40 km. The results can be found in the appendix tables 4.9 and 4.10. Only Arkansas has observations for all the four considered distance intervals.

In the difference-in-difference regressions, the treatment effects are always positive. However, they are only statistically significant at the 10 % level for Arkansas' [10, 20] and [20, 30] km intervals. The treatment effect sizes are similar throughout, while the [30, 40] km one is smaller than the others. This pattern indicates a relatively stable migration response up to 30 km from the border, which then might decline. Maryland has only one treated border PUMA, which is in the [30, 40] km range. Its migration effect is hence not statistically significant as in the main regressions.

In the triple difference regressions, only the treatment effect for the [30, 40] km interval is sometimes statistically significant at the 10 % level for New Mexico, Iowa, and Illinois. It is positive for the latter two, while negative for New Mexico. This unexpected negative effect might be due to the low number of observations per interval, resulting in PUMA-level migration peculiarities shining through. Maryland has only one treated border PUMA, resulting in a not statistically significant migration effect as in the main regressions.

These oddities disappear when increasing the number of observations per distance interval by pooling all the four states together. Only the [10, 20] km interval's treatment effect is statistically significant at the 5 % level and positive. Thus, the Medicaid migration effect tends to be stronger closer to the border if it occurs. All the remaining treatment effects are positive, albeit not statistically significant, with varying sizes. The migration effect does not linearly decline with distance. However, this might be due to the low number of migrant observations per distance category.[36]

The present study sometimes finds migration effects beyond Alm & Enami's (2017) 25 km cutoff, but at the latest, the effect seems to disappear after McKinnish's (2005, 2007) 40 km threshold. The cutoff distance is distinctively smaller than the 75, 150, and 250 km thresholds used in Goodman (2017). However, the gradually declining border migration effect found for Massachusetts by Alm & Enami (2017) could not be confirmed. This effect might be either a specificity of Massachusetts or due to them studying cities, which are area-wise more concentrated than PUMAs. Identifying individuals living close to a treatment border remains imprecise as the PUMAs are pretty large in area, especially in rural areas. Furthermore, the here-obtained results need to be considered with caution due to

[36] The main regressions have been repeated with respectively a 10 and 20 km threshold. In these cases, no statistically significant migrant effect can be identified. When instead all other border regions up until a threshold of 40 km are excluded, the 10 and 20 km regions exhibit sometimes a statistically significant positive migration effect. This is in line with the interval results and probably due to some migration effect remaining between 20 and 40 km. If one does not exclude these regions, they are part of the control group and meddle the migration effect. (Results are available upon request.)

the low number of migrant observations available. Thus, they do not allow an ultimate answer to the here considered question.

4.7.2 Subsamples

This subsection presents the results of several regressions, where the sample changes: urban/rural distinction, childless subsample, youth subsample, border regions to late- and early-expansion states, and non-expansion states.

The migration response to the Medicaid expansion might differ in urban and rural areas, which the difference-in-difference framework would not capture. Notably, it might be that if individuals decide to move for Medicaid, they rather move to urban areas than rural ones. For instance, Kumar (2021) could only identify an ACA migration effect for homeless individuals for metropolitan border counties but not for rural ones. Therefore, this robustness check includes a dummy variable and interaction terms for urban status. Restricting the sample to urban areas is not viable. It reduces the number of treated border regions considerably, further aggravating the issues discussed in the previous section.

The regressions include a dummy variable taking the value of one if an individual currently lives in a PUMA located at least partly in a central city of a metropolitan statistical area as defined by the US Census Bureau. Some regressions also include the respective interaction terms of this dummy. The results can be found in the appendix table 4.11 for Arkansas (difference-in-difference) and in table 4.12 for the four states pooled together (triple difference; remaining results upon request). The urban dummy's coefficient is only statistically significant at the 5 % level in the triple difference regressions, then being positive. In this case, more individuals migrate to urban areas than rural ones. However, the coefficients of its interaction term with the treated border region dummy and the treatment effect are never statistically significant. Thus, no difference between urban and rural areas regarding Medicaid migration can be identified. Most importantly, the inclusion of the urban dummy and its interaction terms does not affect the estimated migration effect. It remains positive and of almost identical size for Arkansas, albeit the significance level drops to 10 %. For the four triple difference states, the treatment effect also remains positive and of comparable size as in the main regressions. It is now statistically significant at the 5 % level in the logistic regression while remaining insignificant in the OLS one.[37]

Individuals living in a household with an underage child might be less likely to migrate ceteris paribus. Children do not gain additional coverage with the ACA Medicaid expansion, but they increase one's migration costs (Goodman, 2017). Therefore, a stronger migration effect is expected for childless individuals. On the other hand, the family size control variable might already capture some of this effect. Running the regressions on a restricted subsample of childless individuals does not seem advisable as the sample size is reduced considerably by at least 25 %. Therefore, a childless dummy and its interaction terms are included into the regressions: either for being childless altogether or for not having a child under age 18. Regardless of its definition, the childless dummy's and its interaction terms' coefficients are never statistically significant at the 10 % level. Thus, the estimated migration effect remains as previously (results available upon request).

Young people (aged 19—25) might have benefitted disproportionally from the ACA Medicaid expansion as they historically had the highest uninsurance rates among all age groups (Johnston, 2021). Furthermore, they are generally more likely to migrate, as shown by the age control variables' statistically significant, negative coefficient. Thus, they might be disproportionally inclined to migrate for Medicaid. Therefore, a youth dummy (below 26 respectively 31 years) and its interaction terms are included into the regressions. Running regressions on a youth subsample is again not viable as the sample size would be more than halved.

In the difference-in-difference regressions, the youth Medicaid migration effect is always positive but never statistically significant at the 5 % level, while the main migration effect turns not statistically significant for both considered states. A complex picture emerges in

[37] If using other urban definitions such as the OECD threshold of inhabitants per square km or being in a metropolitan statistical area at all, the urban dummies and their interaction terms never exhibit statistically significant coefficients. Consequently, the estimated migration effect remains unaffected.

the triple difference regressions. Significance levels and signs vary between OLS and logistic regressions as well as depending on the considered age threshold. However, the main migration effect is never statistically significant, similar to the main regressions. Overall, this hints at the Medicaid migration response possibly being particularly strong among the youth. However, this cannot be evaluated conclusively due to the small size of the youth subsample. (Results are available upon request.)

The main analysis excludes any border regions to late- and early-expansion states. Thus, it also excludes border regions that border at the same time non-expansion and late- or early-expansion states. This exclusion might lead to an incomplete picture of the occurring Medicaid-induced migration. Furthermore, the number of border regions and hence their observations decreases substantially in some states (for instance, by about 70 % for Maryland).[38] Therefore, dummies and interaction terms for border regions neighboring late- and early-expansion states have been added into the regressions. The obtained results mirror the main ones. The estimated migration effects are not statistically significant but mostly positive. Thus, this approach does not change the insignificant results either. (Results are available upon request.)

The ACA Medicaid expansion should as well affect the non-expansion states. In these states, the Medicaid expansion should lead a priori to a decrease in in-migration rates of treated border regions compared to their pre-expansion levels and interior regions' migration evolution. Thus, the treatment effect coefficient is expected to be negative. No statistically significant treatment effects emerge in difference-in-difference and triple difference regressions (results available upon request). Thus, no ACA Medicaid expansion effect on interstate migration is discernable for non-expansion states, in accordance with Baker (2020).

4.7.3 Pseudo-Regressions for Intrastate Migration

This subsection presents results of pseudo-regressions with intra-state migration instead of inter-state migration. The intrastate migration should not have been affected by the

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[38] In contrast, this effect is irrelevant for Arkansas. Arkansas has no border PUMAs to late- or early-expansion states.

ACA Medicaid expansion. The pseudo-treatment effect coefficient (post-ACA* *border*) should not statistically significantly differ from zero.

The dependent variable is replaced in these regressions. The migration dummy now equals one if an individual has migrated across PUMAs but within the state. It is zero if the individual has not migrated or only within the PUMA. The dummy is set to a missing value if the individual has migrated across states or from abroad. Thus, the sample size is slightly reduced (by approximately 3 %) compared to the main regressions, while the migration rate is higher (approximately 7.5 vs. 4.7 %). The regression set-up remains otherwise the same.

These regressions should fulfill the parallel trend test requirements to infer from them regarding the pseudo-treatment over the 2012—2017 period. Therefore, parallel trend tests over 2008—2011 have been run. However, very few states exhibit parallel trend tests with a p-value > 0.5. For the states considered in the main analysis, this is only the case for New Mexico (triple difference). Furthermore, Kentucky (both regression models) and West Virginia (difference-in-difference) have this kind of parallel trend test result. Therefore, the pseudo-regressions have been run for the mentioned states (appendix table 4.13 presents the results for New Mexico; remaining results available upon request).

The pseudo-treatment coefficient is never significant at the 10 % level. The t-test p-values of the pseudo-treatment effect coefficients are often above 0.7. Thus, one can be confident that the observed interstate migration effect originates from the ACA Medicaid expansion if the state exhibits good parallelism in pre-existing trends.

4.7.4 Regressions over 2008—2017

The PUMA delineation changes between 2011 and 2012 render it impossible to use the 2008—2011 data for the main analysis. However, in Arkansas, Kentucky, New Mexico, and Washington, the PUMA delineations did not change or only in such a way as not to affect the borders of the overall treatment and control regions, which might consist of several PUMAs. It is hence possible for these four states to combine the 2008—2011 with the 2012—2017 data.

First, the parallel trend tests have been repeated over the whole pre-period 2008—2013. The obtained results are very similar to the 2008—2011 ones. The p-values of the F- respectively X^2- tests of joint significance of all the treatment effect coefficients of the event study regression are again only convincing for Arkansas in the difference-in-difference setting (> 0.7, triple difference < 0.4) and for New Mexico in the triple difference one (> 0.5, difference-in-difference < 0.2). Kentucky (< 0.3 respectively < 0.1) and Washington (< 0.2) continue to exhibit unsatisfactory parallel trend test results. These results confirm the 2008—2011 ones, indicating that they are a good proxy test for parallel trends in migration before the ACA.

Second, the longer pre-period renders it possible to run pseudo-regressions mirroring the set-up of the main ones: two pre- and four treatment years. 2008 and 2009 are now the pseudo-pre-treatment, while 2010—2013 are the pseudo-treatment years. As no reform occurred, the coefficient of the pseudo-treatment effect should not be statistically significant. This is the case for all four states. Thus, the statistically significant results obtained in the main regressions for Arkansas have not been there before but are particular to the post-ACA years.

Third, it is now possible to estimate regressions with a six-year pre-treatment period (2008—2013) for Arkansas and New Mexico (appendix table 4.14). The results resemble the main ones for both states. Arkansas again exhibits a statistically significant and positive migration effect, while New Mexico's is not statistically significantly negative.

4.8 Conclusion

This paper studies the 2014 ACA Medicaid expansion's migration effects, especially for border regions in expansion states bordering a non-expansion state. It compares these border with interior regions' in-migration before and after the ACA in difference-in-difference and triple difference frameworks to assess the border migration effects. Treated border regions of expansion states are expected to attract more migrants after the ACA compared to both their pre-ACA migration rates and the migration evolution in these states' interior regions.

The present paper's contribution is the combination of Goodman's (2017) analysis of the ACA Medicaid expansion with the border-versus-interior-regions approach by Alm &

Enami (2017) to evaluate migration induced by a Medicaid expansion. Additionally, the present study extends up until 2017. This allows for both pooling years and studying slightly longer-term effects of the ACA Medicaid expansion on migration.

The paper could not identify border migration effects for most considered states, but Arkansas exhibits a statistically significant effect. Even for Arkansas, however, the aggregate number of additional migrants is so small that it seems manageable at the regional and state level. Thus, Medicaid migrants do not appear to impose a meaningful fiscal externality on these budgets. Excessively attracting low-income individuals due to the Medicaid expansion is unlikely even if the migration effect is statistically significant for the considered state.

Employing the border-versus-interior-regions approach and pooling several years can help identify border migration effects that otherwise could not be isolated. However, this approach is not enough when a state has few treated border regions and a low number of migrant observations. In these cases, the statistical power issues of previous studies resurface, rendering the identification of a Medicaid migration effect impossible. Therefore, the latter's existence, precise size, and differences across states remain an unsolved puzzle requiring further research.

4.9 Appendix

Table 4.4: Expansion Status of States

Expansion states (January 1, 2014)	**Early-expansion states**	**Late-expansion states**	**Non-expansion states**
Arizona	California (2010)	Michigan (April 1, 2014)	Alabama
Arkansas	Connecticut (2010)	New Hampshire (August 15, 2014)	Florida
Colorado	Delaware (1996)	Pennsylvania (January 1, 2015)	Georgia
Illinois	District of Columbia (2010)	Indiana (February 1, 2015)	Idaho
Iowa	Hawaii (1994)	Alaska (September 1, 2015)	Kansas
Kentucky	Massachusetts (2006)	Montana (January 1, 2016)	Maine
Maryland	Minnesota (2010)	Louisiana (July 1, 2016)	Mississippi
Nevada	New York (2001)		Missouri
New Jersey	Vermont (1996)		Nebraska
New Mexico	Wisconsin (2009)		North Carolina
North Dakota			Oklahoma
Ohio			South Carolina
Oregon			South Dakota
Rhode Island			Tennessee
Washington			Texas
West Virginia			Utah
			Virginia
			Wyoming

After Black et al. (2019) and The Henry J. Kaiser Family Foundation (2019)

Table 4.5: State-Level Migration Pre- versus Post-ACA

	(1)	(2)	(3)	(4)
	Difference-in-Difference (DD)		Triple Difference (DDD)	
	OLS	logistic	OLS	logistic
	migration	migration	migration	migration
treatment DD $(expansion * post)$	**0.001*** (0.001)	**1.047*** (0.026)		
treatment DDD $(expansion * post * medicaid)$			**0.001** (0.001)	**1.039** (0.042)
States included	all expansion and non-expansion states			
N	1,547,708	1,547,708	2,795,100	2,795,100
(Pseudo-) R^2	0.014	0.041	0.013	0.046
p-value parallel trend test	0.45	0.39	0.57	0.59

This table reports results of a global, state-level analysis of migration before and after the ACA Medicaid expansion. It assesses whether migration to expansion states as a whole increased after the ACA compared to migration to non-expansion states. Early- and late-expansion states are excluded from the analysis. The first two columns report the treatment effect coefficient (δ) from difference-in-difference regressions of the form $y_{ist} = \alpha + \beta_1 post_t + \beta_2 expansion_s + \delta(expansion_s * post_t) + \theta X_{it} + \mu_s + \varepsilon_{ist}$. The last two columns report the treatment effect coefficient (δ) from triple difference regressions of the form $y_{ist} = \alpha + \beta_1 post_t + \beta_2 expansion_s + \beta_3 medicaid_i + \gamma_1(expansion_s * post_t) + \gamma_2(medicaid_i * post_t) + \gamma_3(medicaid_i * expansion_s) + \delta(expansion_s * post_t * medicaid_i) + \theta X_{it} + \mu_s + \varepsilon_{ist}$. The regressions are estimated at the individual level. The years 2014—2017 are considered *post* years, while 2012 and 2013 are not. *Medicaid* indicates Medicaid eligibility. The comparison group are individuals with an income between 200 and 400 % FPL. The treatment effect is expected to be positive. Robust standard errors are in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4.6: Parallel Trend Test Regressions: Arkansas and Maryland

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Difference-in-Difference		Triple Difference		Difference-in-Difference		Triple Difference	
	OLS	logistic	OLS	logistic	OLS	logistic	OLS	logistic
	migration	migration	migration	migration	migration	migration	migration	migration
$border * 2008$	-0.012	0.712			0.012	1.208		
	(0.011)	(0.214)			(0.041)	(0.789)		
$border * 2009$	-0.005	0.861			-0.013	0.553		
	(0.011)	(0.268)			(0.042)	(0.381)		
$border * 2010$	-0.011	0.719			-0.031	0.545		
	(0.011)	(0.211)			(0.037)	(0.392)		
$border * 2008 * medicaid$			-0.026*	0.422			-0.015	0.683
			(0.015)	(0.235)			(0.055)	(0.690)
$border * 2009 * medicaid$			-0.010	0.722			-0.012	0.568
			(0.015)	(0.399)			(0.048)	(0.566)
$border * 2010 * medicaid$			-0.020	0.544			-0.031	0.387
			(0.015)	(0.306)			(0.045)	(0.448)
p-value: 2008—2010 equal 0	**0.67**	**0.62**	**0.35**	**0.44**	**0.64**	**0.51**	**0.92**	**0.87**
Controls	yes	yes	yes	yes	yes	yes	yes	yes
Year FEs	yes	yes	yes	yes	yes	yes	yes	yes
State	Arkansas				Maryland			
N	16,706	16,706	29,699	29,699	6,454	6,454	11,640	11,640
(Pseudo-) R^2	0.007	0.020	0.010	0.032	0.034	0.104	0.034	0.115

This table reports the treatment effect coefficients (δ) from event study regressions for Arkansas and Maryland. The difference-in-difference regressions have the form $y_{irst} = \alpha + \beta_1 year_t + \beta_2 border_r + \delta(border_r * year_t) + \theta X_{it} + \varepsilon_{irst}$ while the triple difference ones have the form $y_{irst} = \alpha + \beta_1 year_t + \beta_2 border_r + \beta_3 medicaid_i + \gamma_1(border_r * year_t) + \gamma_2(medicaid_i * year_t) + \gamma_3(medicaid_i * border_r) + \delta(border_r * year_t * medicaid_i) + \theta X_{it} + \varepsilon_{irst}$ Both are estimated at the individual level over 2008—2011. 2011 is the reference year. *Medicaid* indicates Medicaid eligibility. The comparison group consists of individuals with an income between 200 and 400 % FPL. The table also reports the respective p-values of F- (OLS regressions) or X^2- (logistic regressions) tests of joint significance of all the treatment effect coefficients (δ) together. For more details, see the main text. Robust standard errors are in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4.7: Summary Statistics for Arkansas and Maryland

Variable	Treatment Regions	Control Regions	Diffe-rence
Arkansas			
migration dummy	0.0443 (0.0026)	0.0252 (0.0019)	0.0191 (0.000***)
age	35.76 (0.1722)	36.08 (0.1669)	-0.32 (0.182)
total family income in the previous 12 months (2014 US-$)	31,129 (588.2)	31,662 (517.8)	-533 (0.500)
bachelor degree or higher (share)	0.0664 (0.0032)	0.0808 (0.0033)	-0.0144 (0.002***)
family size (number of family members)	3.009 (0.0260)	2.907 (0.0230)	0.102 (0.003***)
26,352 observations	*12,976 observations*	*13,376 observations*	
Maryland			
migration dummy	0.0361 (0.0078)	0.0233 (0.0019)	0.0128 (0.109)
age	33.71 (0.5814)	36.87 (0.1840)	-3.16 (0.000***)
total family income in the previous 12 months (2014 US-$)	70,645 (3270.8)	36,421 (662.4)	34,224 (0.000***)
bachelor degree or higher (share)	0.0843 (0.0105)	0.1004 (0.0038)	-0.0161 (0.148)
family size (number of family members)	3.453 (0.0906)	3.058 (0.0331)	0.395 (0.000***)
10,306 observations	*887 observations*	*9,419 observations*	

This table reports the analysis sample mean over 2012—2017 of several variables for Arkansas and Maryland. The averages are reported for the treatment (treated border) and control regions. The last column reports the difference between the treatment and the control regions' average. Linearized standard errors are in parenthesis (accounting for the ACS cluster and strata structure), except for the difference column, which reports the p-values of the adjusted Wald test. The latter tests the null hypothesis that the difference between the two is equal to zero. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4.8: Distribution of Migrant and Non-Migrant Observations across Regions

	Migrant Observations	**Non-Migrant Observations**
Arkansas		
Treated Border regions	898	14,148
Interior regions	632	16,664
Illinois		
Treated Border regions	229	8,236
Interior regions	1,694	52,479
Iowa		
Treated Border regions	225	3,101
Interior regions	1,191	17,222
Maryland		
Treated Border regions	48	1,136
Interior regions	465	11,167
New Mexico		
Treated Border regions	141	1,919
Interior regions	839	20,357

This table reports the migrant respectively non-migrant observations in treated border and interior regions for the five considered expansion states over 2012—2017. Due to the applied weights in the ACS, the observations are not proportional to the total number of migrants in these regions.

Table 4.9: Different Border Cut-Offs: Pre- versus Post-ACA Migration: OLS

	(1) OLS migration	(2) OLS migration	(3) OLS migration	(4) OLS migration	(5) OLS migration	(6) OLS migration	(7) OLS migration
treatment [0, 10] ($border_{0,10} * post$)	-	0.017 (0.012)					
treatment [10, 20] ($border_{10,20} * post$)	-	0.023* (0.012)					
treatment [20, 30] ($border_{20,30} * post$)	-	0.016* (0.009)					
treatment [30, 40] ($border_{30,40} * post$)	0.005 (0.015)	0.003 (0.009)					
treatment [0, 10] ($border_{0,10} * post * medicaid$)			-	-	0.030 (0.033)	-	0.043** (0.010)
treatment [10, 20] ($border_{10,20} * post * medicaid$)			-	0.020 (0.028)	-	-0.003 (0.013)	0.008 (0.010)
treatment [20, 30] ($border_{20,30} * post * medicaid$)			-	-	0.027 (0.024)	-0.017 (0.025)	0.002 (0.016)
treatment [30, 40] ($border_{30,40} * post * medicaid$)			-0.006 (0.022)	-0.038 (0.028)	0.040* (0.024)	0.024** (0.010)	0.012 (0.012)
N	10,306	26,352	17,755	31,918	35,355	95,190	180,218
R^2	0.034	0.008	0.035	0.020	0.019	0.010	0.014
State	Maryland	Arkansas	Maryland	New Mexico	Iowa	Illinois	Maryland, New Mexico, Iowa, Illinois

This table reports the treatment effect coefficients from OLS difference-in-difference and triple difference regressions as previously, but treated border regions are now grouped according to their distance to the border in 10 km intervals. For more details, see the main text. Robust standard errors (additionally clustered at the state level in column 7) are in parentheses; * p < 0.1, ** p < 0.05, *** p < 0.01

Table 4.10: Different Border Cut-Offs: Pre- versus Post-ACA Migration: Logistic

	(1) logistic migration	(2) logistic migration	(3) logistic migration	(4) logistic migration	(5) logistic migration	(6) logistic migration	(7) logistic migration
treatment [0, 10] ($border_{0,10} * post$)	-	1.623 (0.491)					
treatment [10, 20] ($border_{10,20} * post$)	-	1.935^{*} (0.683)					
treatment [20, 30] ($border_{20,30} * post$)	-	1.601^{*} (0.407)					
treatment [30, 40] ($border_{30,40} * post$)	1.163 (0.559)	1.155 (0.310)					
treatment [0, 10] ($border_{0,10} * post * medicaid$)			-	-	2.826 (2.378)	-	4.718^{***} (2.786)
treatment [10, 20] ($border_{10,20} * post * medicaid$)			-	1.950 (1.564)	-	0.751 (0.531)	1.401 (0.572)
treatment [20, 30] ($border_{20,30} * post * medicaid$)			-	-	1.826 (1.050)	0.642 (0.486)	1.060 (0.431)
treatment [30, 40] ($border_{30,40} * post * medicaid$)			1.021 (0.732)	0.0786^{**} (0.093)	1.525 (1.459)	2.893^{*} (1.779)	1.120 (0.589)
N	10,306	26,352	17,755	3,1918	35,355	95,190	180,218
Pseudo-R^2	0.129	0.025	0.140	0.066	0.064	0.047	0.057
State	Maryland	Arkansas	Maryland	New Mexico	Iowa	Illinois	Maryland, New Mexico, Iowa, Illinois

This table reports the treatment effect coefficients from logistic difference-in-difference and triple difference regressions as previously. However, treated border regions are now grouped according to their distance to the border in 10 km intervals. For more details, see the main text. Robust standard errors (additionally clustered at the state level in column 7) are in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4.11: Urban Indicator: DD: Pre- versus Post-ACA Migration: Arkansas

	(1) OLS migration	(2) logistic migration	(3) OLS migration	(4) logistic migration	(5) OLS migration	(6) logistic migration
treatment	**0.013****	**1.483****	**0.013****	**1.483****	**0.013***	**1.481***
($border * post$)	(0.006)	(0.283)	(0.006)	(0.283)	(0.008)	(0.348)
urban*treatment					**-0.000**	**1.012**
					(0.013)	(0.408)
urban*border			-0.000	0.977	-0.000	0.974
			(0.007)	(0.193)	(0.010)	(0.310)
urban	-0.003	0.913	-0.002	0.926	-0.001	0.968
	(0.003)	(0.090)	(0.004)	(0.142)	(0.007)	(0.239)
N	26,352	26,352	26,352	26,352	26,352	26,352
(Pseudo-) R^2	0.007	0.024	0.007	0.024	0.007	0.024

This table reports selected coefficients from difference-and-difference regressions for Arkansas as previously. However, the regressions now include a dummy variable taking the value of one if an individual is living in an urban area (defined as living in a PUMA located at least partly in a central city of a metropolitan statistical area) as well as its interaction terms. Robust standard errors are in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4.12: Urban Indicator: DDD: Pre- versus Post-ACA Migration: Pooled

	(1) OLS migration	(2) logistic migration	(3) OLS migration	(4) logistic migration	(5) OLS migration	(6) logistic migration
treatment	**0.010**	**1.389**	**0.010**	**1.390**	**0.009**	**1.343****
($border * post * medicaid$)	(0.007)	(0.294)	(0.007)	(0.301)	(0.005)	(0.172)
urban*treatment					**0.002**	**1.025**
					(0.010)	(0.295)
urban*border			0.002	0.975	0.005	1.154
			(0.007)	(0.234)	(0.008)	(0.237)
urban	0.003*	1.104***	0.003	1.108**	0.000	1.037
	(0.001)	(0.029)	(0.002)	(0.046)	(0.003)	(0.141)
States included	Maryland, New Mexico, Iowa, Illinois					
N	180,218	180,218	180,218	180,218	180,218	180,218
(Pseudo-) R^2	0.014	0.056	0.014	0.056	0.014	0.056

This table reports selected coefficients from pooled triple difference regressions as previously. However, the regressions now include a dummy variable taking the value of one if an individual is living in an urban area (defined as living in a PUMA located at least partly in a central city of a metropolitan statistical area) as well as its interaction terms. Robust standard errors are in parentheses and clustered by state; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4.13: Intrastate Migration: Pre- versus Post-ACA: New Mexico DDD

	(1) OLS migration	(2) logistic migration
post	-0.003	0.836
	(0.005)	(0.189)
border	-0.011	0.533
	(0.008)	(0.305)
medicaid	-0.001	0.947
	(0.005)	(0.204)
*border*post*	0.012	2.101
	(0.013)	(1.544)
*border*medicaid*	0.004	1.315
	(0.012)	(0.893)
*post*medicaid*	0.003	1.204
	(0.006)	(0.323)
pseudo-treatment $(border * post * medicaid)$	**-0.000**	**0.756**
	(0.017)	**(0.648)**
Controls	yes	yes
N	30,993	30,993
(Pseudo)-R^2	0.012	0.054
p-value parallel trend test	0.84	0.76

The table reports selected coefficients from triple difference pseudo-regressions for New Mexico. The set-up of the respective regressions remains as previously. However, the dependent variable is now migration from one PUMA to another within a given state, while migrants from outside the state are excluded from the sample. These regressions estimate a pseudo-treatment effect, which should not be statistically significant. Robust standard errors are in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

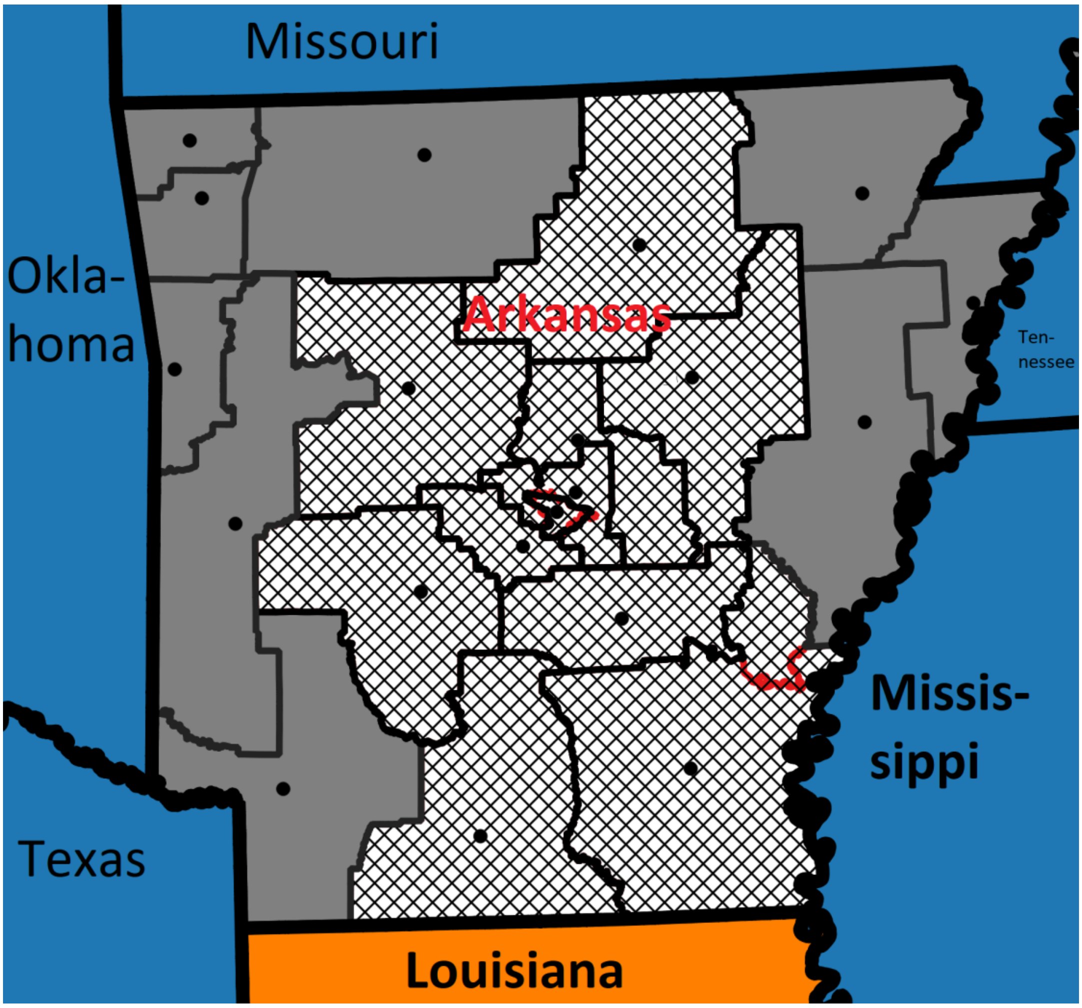
Table 4.14: 2008—2017 Pre- versus Post-ACA Migration: Arkansas and New Mexico

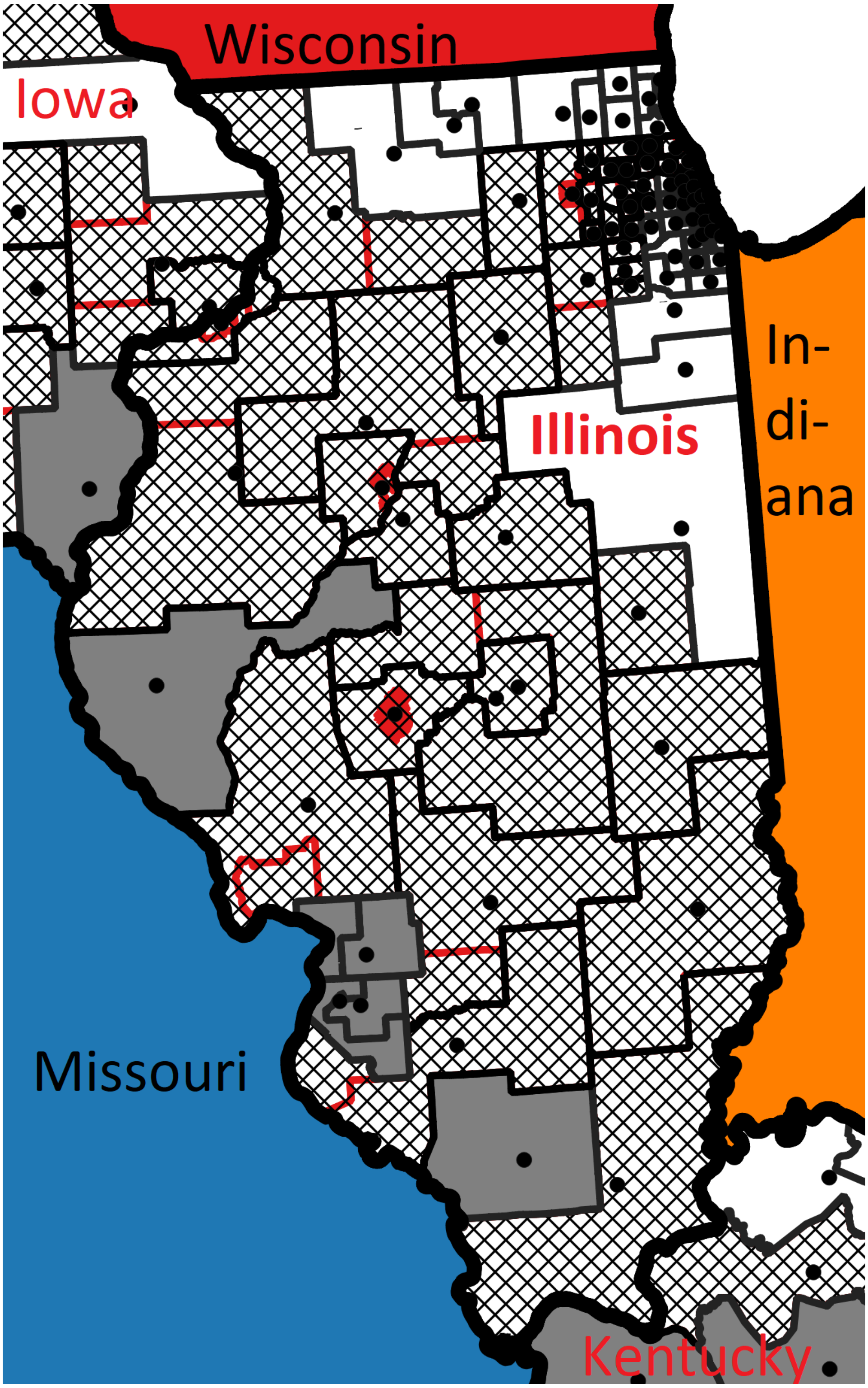
	(1)	(2)	(3)	(4)
	Difference-in-Difference (DD)		Triple Difference (DDD)	
	OLS	logistic	OLS	logistic
	migration	migration	migration	migration
post	-0.010***	0.684***	-0.004	0.864
	(0.003)	(0.076)	(0.004)	(0.101)
border	0.006*	1.168*	-0.004	0.853
	(0.003)	(0.103)	(0.006)	(0.190)
medicaid			0.008**	1.270**
			(0.004)	(0.129)
treatment DD	**0.017*****	**1.709*****	0.034**	2.573***
$(border * post)$	**(0.005)**	**(0.248)**	(0.014)	(0.869)
*border*medicaid*			0.004	1.147
			(0.009)	(0.315)
*post*medicaid*			-0.001	0.980
			(0.005)	(0.147)
treatment DDD			**-0.017**	**0.616**
$(border * post * medicaid)$			**(0.018)**	**(0.262)**
Controls	yes	yes	yes	yes
State	Arkansas		New Mexico	
N	43,058	43,058	51,889	51,889
(Pseudo-) R^2	0.006	0.020	0.018	0.057
p-value parallel trend test	0.79	0.71	0.84	0.51

This table reports selected coefficients from difference-in-difference and triple difference regressions for Arkansas and New Mexico as previously. However, the analysis period now extends over 2008—2017. The years 2014—2017 are considered *post* years, while 2008—2013 are not. For more details, see the main text. Robust standard errors are in parentheses; * p < 0.1, ** p < 0.05, *** p < 0.01

Maps of Analyzed Expansion States

Each map shows the PUMAs of the respective expansion state (Arkansas, Illinois, Iowa, Maryland, and New Mexico). Treated border PUMAs are in grey, interior (control) PUMAs are hatched, while excluded PUMAs (due to neighboring an early- or late-expansion state) are in white. A black dot indicates the PUMA's population-weighted centroid. Black lines represent 2012—2017 PUMA boundaries, while red lines show where the pre-2012 PUMA delineations differed for the control group. Bold black lines indicate state borders. Neighboring non-expansion states are in blue, early-expansion states in red, and late-expansion states in orange. *Source: own elaboration based on IPUMS National Historical Geographic Information System shapefiles and data (Manson et al., 2019)*

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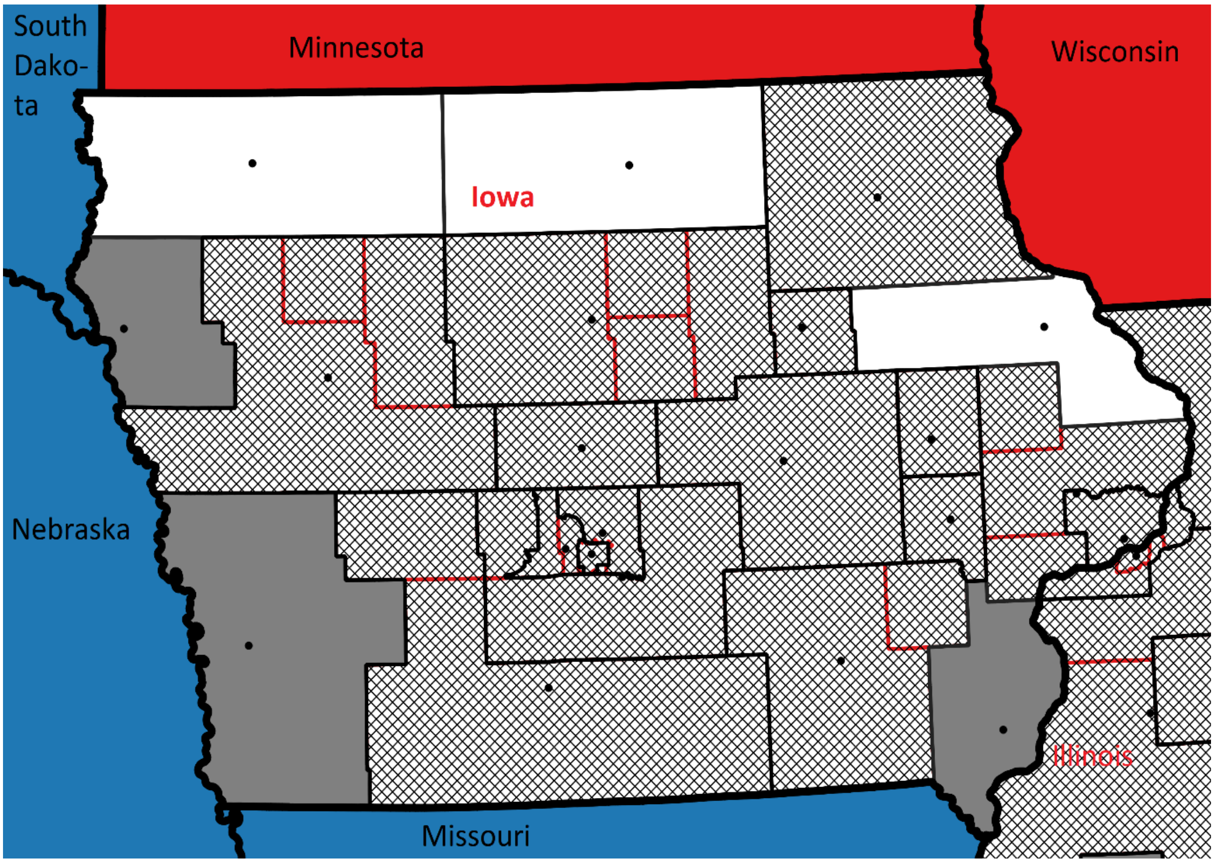
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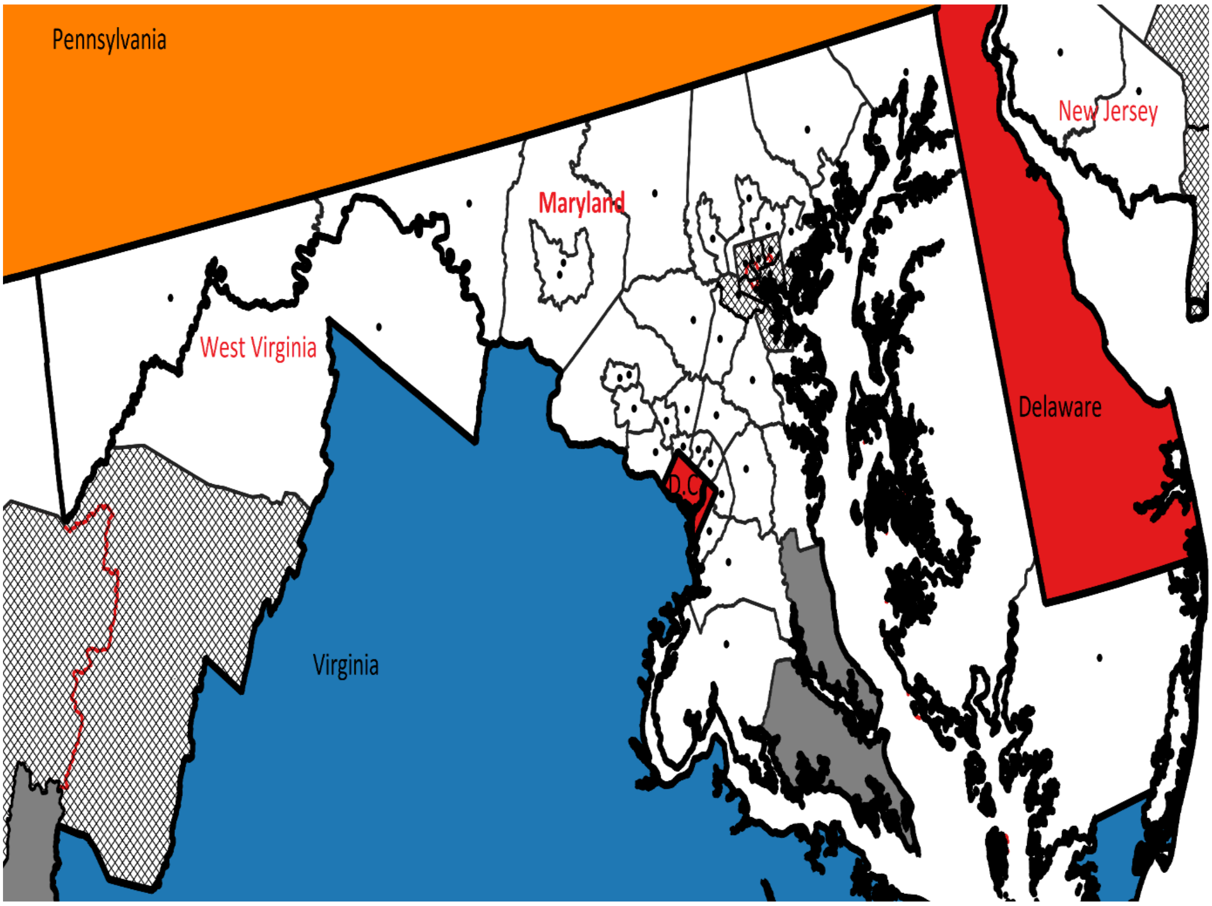
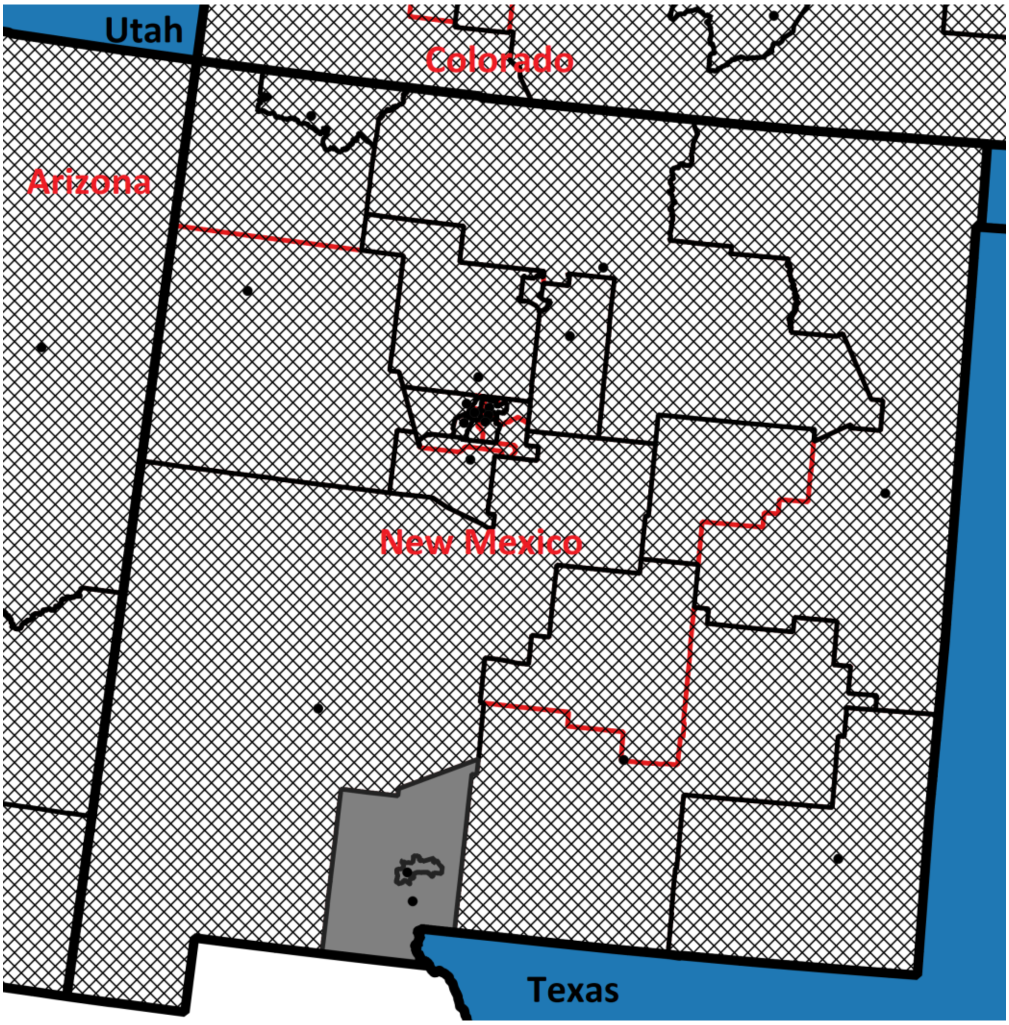
Illinois


Pennsylvania
New Jersey
Maryland
West Virginia
Delaware
D.C.
Virginia


Utah
Colorado
Arizona
New Mexico
Texas

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