

Province-Level Spending, Tax Reforms, and Labour Market in Poland

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December 2023

Abstract

While the impacts of fiscal changes on labour outcomes have been widely studied, less attention is paid to the dynamics of local territorial units in emerging economies. In this paper, I leverage the characteristics of the 2021 *Polish Deal* tax reform to study the influence of austerity on local labour markets in an emerging economy setting. Using Poland's regional data on job offers, unemployment, and political affiliation of provincial authorities, I apply the close election RD-DID methodology to reach three conclusions. First, local budgetary contractions harm job creation: a 175,000 *zlotys* ($\simeq \$44,200$) expenditure fall corresponds to one job not being posted. Second, likely owing to the tightness of Poland's labour conditions, unemployment continues falling with austerity, highlighting the greater sensitivity of the job-creation channel. Third, delving into separate expenditure categories highlights the importance of province fiscal budget constraints in the propagation of austerity.

1 Introduction

Across the EU, local governments deliver education, health services, infrastructure, and more. This means that, despite the lower profile of local policymaking, council-level fiscal measures can have a disproportionate impact on economic outcomes. This article investigates labour market outcomes of local fiscal consolidations. Using the quasi-experiment of Poland's 2021 tax reform, I find that the provinces experiencing deeper spending cuts faced a lower job growth rate and declining unemployment, with 175,000 *zlotys* ($\simeq \$44,200$) in budgetary contraction corresponding to one job not being created. This suggests that local territorial units adjust to austerity along the job creation (extensive) margin while still facing tight labour conditions.

There are multiple mechanisms linking negative demand shocks induced by spending cuts and local labour outcomes. A sudden drop in expenditure causes demand to decline for the local council's contractors. They, in turn, can lower the employment or wages of their workforce. Downward wage rigidity implies that firms disproportionately adjust along the extensive margin ([Elsby et al., 2009](#)). Local austerity measures can also lead to a decrease in business sentiment, putting a pause on investments by local businesses. Conversely, the potential lower morale among the workforce is likely to add to a decrease in productivity and negatively impact job search.

In exploiting the reform's nature, I contribute to the literature that uses quasi-experiments to estimate geographic cross-sectional multipliers along two dimensions. First, most of the works in this field investigate fiscal stimuli in developed economies. With the notable exceptions of [Acconcia et al. \(2014\)](#) and [Komarek et al. \(2022\)](#) who analyse budgetary contractions in Italy and the US, most of the studies focus on the stimulus introduced by the 2009 *American and Recovery and Re-Investment Act* ([Chodorow-Reich, 2019](#)). My article's novelty is adapting the setting to that of an emerging economy and EU member state. The closest work to mine is that of [Komarek et al. \(2022\)](#) who exploit the automatic policy changes induced by the 2011 *Budget Control Act* in the US. They leverage the independence of cuts introduced by federal agencies and apply shift-share instruments to estimate that a \$1M reduction in federal spending corresponds to around 10 jobs and \$0.19 aggregate wages lost. Similarly, I study an across-the-board expenditure reduction. While

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they use administrative data covering the federal government's contractors, I pull the province-level budget expenditure and labour data and combine it with the RD-DiD methodology to show that an expenditure reduction of 175,000 *zlotys* ($\simeq \$44,200$) leads to one job not being posted. I contribute to the public labour literature by showing how such expenditure cuts negatively influence job creation, even when unemployment is not affected.

Second, this article builds on the burgeoning body of works that exploit exogenous political variation to estimate geographic cross-sectional multipliers. The closest studies to mine are that of [Feyrer and Sacerdote \(2011\)](#) and [Cohen et al. \(2011\)](#). The latter paper leverages the exogenous changes to the powerful committee chairpersonships to show how federal spending shocks dampen local corporate investments. The strength of their work is that, by breaking the link between private economic performance and public spending, it allows for causally inferring the impact of fiscal changes on the private economy. Like them, I put exogenous political variation to use by taking the close election RD-DiD approach. While [Cohen et al. \(2011\)](#)'s methodology focuses on corporate investment, I scrutinise the spending's impact on job creation and the overall level of unemployment.

This paper studies how labour market outcomes depend on local budgetary cuts in Poland. I start by presenting the argument as to why I could leverage the party-political differences between province councils and the national government in the context of the 2021 tax reform. Then, I adapt the theoretical argument of [Chodorow-Reich \(2019\)](#) to explain intuitively that estimated multipliers constitute a lower bound of a closed economy no-monetary-policy-response that is of interest for fiscal policymakers. To highlight why an RD-based approach is appropriate, I illustrate the across-the-board fall in total real expenditure among local authorities following the implementation of the *Polish Deal* in late 2021. The regions with the same political affiliation as the national government are found to have seen a different level of spending change after the reform, which motivates the use of the RD-DiD methodology. Doing so, I demonstrate that the areas facing budgetary cuts achieve worse labour market outcomes in job creation: a 175,000 *zlotys* ($\simeq \$44,200$) reduction leads to the loss of one private-sector job. This highlights that adjustment to fiscal contractions takes place largely along the extensive margin, echoing the results of [Komarek et al. \(2022\)](#). On the other hand, unemployment-based findings are counterintuitive, which likely highlights the extreme tightness in

Poland’s labour markets ([IMF, 2023](#)).

2 Policy Context: The *Polish Deal* of 2021

I leverage the 2021 tax reform thanks to its two qualities: (1) it did not significantly change taxpayers’ private incomes, but (2) it caused local governments’ total expenditure to fall. The *Polish Deal* is a private income tax reform that was first presented to the public in May 2021. While ambitious at the beginning, the policy was subsequently watered down, with only a minor fraction of taxpayers affected ([Nizioł, 2023](#)). Voted by the parliament in October 2021 and enforced in January 2022 (for more, see the timeline in figure 1), the measure entailed a “grace” period of one year for the taxpayers most likely to lose out as a result of the changes ([Nizioł, 2023](#)). As a result, the reform is unlikely to have caused changes to household spending and labour participation. Although taxpayers were little affected, the government used the bill to alter the financing rules of Poland’s local authorities. From January 2022, the central government was to receive a larger share of the private income tax proceeds at their expense ([Łożykowski, 2023; Kańduła, 2023](#)). If unable to meet the costs of their statutory activities or significant investment projects, the province-level elected authorities were able to apply for a discretionary top-up from the *Ministry of Finance*. As a result, 2022 saw an across-the-board expenditure cut in Poland, visualised in figure 2, and allegations that the central government favoured politically aligned provinces via discretionary top-ups ([Lukomska and Swianiewicz, 2023](#)). These characteristics allow me to leverage the reform in a quasi-experimental setting to estimate the impact of fiscal contractions on local labour markets.

3 Economic Theory

[Chodorow-Reich \(2019\)](#) reasons that local multipliers constitute a lower bound of a closed economy no-monetary-policy-response multiplier. I argue that this holds in the Polish case, as far as job creation is concerned. Consider a closed economy consisting of a continuum of infinitesimal local areas. Say that, at time t , the authorities announce a spending change of $\Delta G_{i,t+h}$ in area i at time horizon h . The government spending path remains unaffected in the rest of the economy. Denote the labour outcome in area i at time t as $Y_{i,t}$ (with Y_t representing the average outcome for the

4. VARIABLE DEFINITIONS & SOURCES

entire economy at t). The local employment multiplier, $\beta_{i,h}$ is obtained from:

$$\beta_{i,h} = \frac{D_{t,t+h}Y_i - D_{t,t+h}Y}{\Delta G_{i,t+h}}, \quad (1)$$

where $D_{t,t+h}X \equiv X_{t+h} - X_t$ for variable X . While the case study of Poland's 2021 tax reform departs from the above assumptions, the standard economic channels make my empirical estimates of $\beta_{i,h}$ for job offers a lower bound of the national closed economy no-monetary-policy-response multiplier that is of interest for potential policymakers.

Monetary Policy Reaction. As visualised in figure 2, the reform constituted across-the-board austerity in Poland. Monetary policy's reaction to such a reform, such as halting or reducing the pace of policy rate hikes to accommodate the negative demand shock, provides a cushion to the policy's impact. Let Y_i^{NM} and $\beta_{i,h}^{\text{NM}}$ be the employment level and local multiplier in the environment without monetary policy response. With inactive monetary authorities, the fiscal contraction's impact on employment is more severe: $Y_{i,t+h}^{\text{NM}} \leq Y_{i,t+h}$. All else constant, this means that $D_{t,t+h}Y_i^{\text{NM}} \leq D_{t,t+h}Y_i < 0$. With $\Delta G_{i,t+h} < 0$, this implies that $\beta_{i,h} \leq \beta_{i,h}^{\text{NM}}$. Against this backdrop, my findings of local labour multipliers are likely to constitute a lower bound of their national counterpart.

Income Effects, Expenditure Switching & Factor Mobility. The expenditure cuts following the reform were heterogeneous, as exemplified in figure 2. The liquidity-constrained agents likely reduced their consumption of both locally-produced goods and those produced in other regions. On top of that, with fewer jobs created in the most affected areas, workers were likely to find employment in less affected provinces. Given such a varying impact, my approximation of $\beta_{i,h}$ is likely to constitute a lower bound of the national labour outcome multiplier which is of more interest to policymakers.

4 Variable Definitions & Sources

To investigate the reform's impact, I create a sample spanning 2021 and 2022. I pull the province-level data from the *Central Statistical Office of Poland* (GUS). I use the monthly job offers and unemployment numbers as labour outcome variables. Their average changes between 2021 and

2021 are visualised in figures 4 and 5. For the local government expenditure, I compile all available annual spending categories and represent them in millions of *zlotys* at the monthly frequency.¹ Every variable is deflated with the annual price index of consumer goods and services, which is also sourced from the GUS. Finally, I obtain the party affiliation of the elected district leaders and provincial councillors from the *National Electoral Commission of Poland* based on the 2018 local elections results. I calculate the effective net support of the ruling party (*Law and Justice*) at the provincial level (expressed in per cent terms).²

5 Empirical Specification

My objective is to quantify how local spending reductions affect local labour markets. To do so, I start with the standard local multiplier framework for province i in month t (Nakamura and Steinsson, 2014):

$$Y_{i,t} = \rho_i + \tau_t + \psi B_{i,t} + \varphi_{i,t}, \quad (2)$$

where $Y_{i,t}$ is the labour-market outcome (the number of unemployed workers or job offers) and $B_{i,t}$ is the province-level total monthly expenditures in millions of zlotys. ρ_i and τ_t are province i and time t fixed effects (FE), respectively. I include location FE to control the economic history of the province. Conversely, plugging time FE helps in capturing nationwide specific shocks at particular dates (e.g., the inflow of Ukrainian refugees following Russia's invasion of Ukraine). In an ideal setting, ψ could be interpreted as an approximation of average $\beta_{i,1}$ from equation (1). There are, however, three challenges to interpreting ψ as causal. First, with income taxes constituting the source of provincial budgets, I cannot rule out reverse causality in regression (2). Second, expenditure reductions are not random. Figure 2 presents the spatial distribution of province-level

¹ Like Komarek et al. (2022), I impose a uniform distribution of spending across the year.

² There are multiple provinces with large representation of local or single-issue parties that are not present in national politics. Given that they are often aligned with the *Law and Justice* party in running councils, I approximate the provincial support of the ruling party with the results of district-level leadership races. Each province is composed of a few districts where leaders (e.g., mayors) are elected in first-past-the-post voting. As such races are mostly contested by the largest parties, there is no presence of the local or single-issue parties. With $S_i^P \equiv$ province i 's level support of *Law and Justice* in the council election and $\tilde{S}_i^P \equiv$ province i 's average support of *Law and Justice* in district leadership races, I use the effective net support measure defined by: $x_i \equiv \max\{S_i^P, \tilde{S}_i^P\} - 50\%$. For example, the observation of $x_i = (-20)\%$ indicates that the *Law and Justice* party lacks 20% to have a majority in province i .

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expenditure cuts. While some regions avoided budgetary contractions, these are not clustered in any region of the country. Third, local spending is not randomly distributed. For example, this could be related to local economic specialisation. Take coastal areas which are found to allocate more funds to fishing industry-related activities than landlocked provinces. Facing austerity, such regions need to look either for cuts in non-statutory categories of spending (e.g., stimulating the local fishing industry) or for new sources of revenue (e.g., taxing the local fishing industry).

That said, I argue that the 2021 tax reform quasi-experiment could be leveraged in an RD framework to avoid these problems. The reform imposed across-the-board funding cuts that negatively affected province-level total real expenditure, as visualised in figure 2. Given that provinces are required to prioritise statutory activities, such as healthcare and education provision, many were at risk of not affording other budgetary items. To alleviate that, they were able to request discretionary grants for other projects from the central government, although with no guarantee of receiving the money ([Lożykowski, 2023](#); [Kańdula, 2023](#)). For example, if such a request were accepted, a coastal province could avoid slashing fishing industry-related expenditures. Against the backdrop of the discretionary nature of the budget top-up, I find that the provinces that are politically aligned with the national government faced a lower total real expenditure drop between 2021 and 2022 than those affiliated with the opposition parties. This pattern is especially strong around the cut-off between the ruling party and opposition support in the most recent local elections, as highlighted in figure 7. There, one can see a clear “jump” between the government and opposition-controlled provinces at almost all bandwidths.³ Figure 8 visualises the real expenditure changes between 2018 and 2019. In doing so, it constitutes a placebo test indicating that, before the reform, the opposition-run provinces enjoyed a higher expenditure growth than those run by the ruling party. Further, I inspect the changes to the average labour market outcomes between 2021 and 2022 in figures 9 and 10. Both provide evidence of a distinct performance cut-off between the government and opposition-controlled areas.⁴

³ I choose to divide the sample into 3 bandwidths: 15%, 20%, and 25% net *Law and Justice* support. All of them are centred at 0. Figure 6 presents the number of provinces in each set. Figure 3 shows the spatial distribution of the bandwidths.

⁴ While figure 10 suggests that the opposition-run provinces perform better when it comes to unemployment, this can be explained by different economic histories (captured by province FE). What matters for motivating regressions (3a) and (3b) is the evidence of clear differences in performance between these types of regions.

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With evidence of temporal expenditure changes and the discontinuity around provinces' political affiliation at hand, I use the RD-DiD setting of [Persson \(2020\)](#) and [Lalive \(2008\)](#) to leverage the reform quasi-experiment:

$$(\text{First Stage}) \quad B_{it} = \delta_i + \omega_t + \alpha_1 x_i + \alpha_2 R_t + \alpha_3 R_t \times \mathbf{1}\{x_i > 0\} + \varepsilon_{it}; \text{ and} \quad (3a)$$

$$(\text{Reduced Form}) \quad Y_{it} = \gamma_i + \zeta_t + \beta_1 x_i + \beta_2 R_t + \beta_3 R_t \times \mathbf{1}\{x_i > 0\} + \epsilon_{it}, \quad (3b)$$

where B_{it} and Y_{it} represent the expenditure and labour outcomes, respectively. δ_i and γ_i are province-level FE. ω_t and ζ_t are time FE. x_i is the net support of the ruling party (above the majority mark). R_t is the 2021 tax reform dummy. $\frac{\hat{\beta}_3}{\hat{\alpha}_3}$ can be causally interpreted as an approximation of average β_i from equation (1) if the following identification assumptions are met:

A.1 conditional on x_i , the impact of B_{it} on Y_{it} is continuous;

A.2 $R_t \times \mathbf{1}\{x_i > 0\}$ impacts Y_{it} only through B_{it} (exclusion restriction); and

A.3 the impacts of R_t and $R_t \times \mathbf{1}\{x_i > 0\}$ are additively separable.

In both plots of 11, I present bin-scatters of the change between the average numbers of unemployed workers and job offers for different levels of province-level budget change between 2021 and 2022. In each case, I residualise the expenditure and both labour market outcomes on the ruling party's support. I show there is no clear evidence of discontinuities in the impact of real expenditure change on the labour market outcomes, conditional on the *Law and Justice* party's support level. In doing so, I claim there is little evidence against assumption **A.1**. Given the arguments in [Nizioł \(2023\)](#), I assume that the tax reform of 2021 had a uniform impact across the country through channels other than the province-level expenditure. If that is the case, assumption **A.2** is met. In what echoes the setting in [Persson \(2020\)](#), assumption **A.3** is purely mechanistic in the context of the *Polish Deal* policy.

6 Results

Running regressions (3a) and (3b) with the number of job offers as the dependent labour outcome produces results indicating a positive relationship between government spending and job creation. As visualised in the first panel of figure 12, my estimates of the local expenditure multiplier for the number of job offers, $\frac{\beta_3}{\alpha_3}$, range from 4.6 to 14.5 for the 15% bandwidth of the ruling party's net support and all provinces, respectively. While the coefficients are of the expected positive sign, only the ones of the 25% bandwidth and for all provinces are statistically different from 0 at the 95% significance level. The coefficient calculated at the 25% bandwidth indicates that a loss of 175,000 *zlotys* ($\simeq \$44,200$) in monthly spending translates into one local job not being posted. This is remarkably in line with public-labour literature. For example, Komarek et al. (2022) use shift-share instruments to estimate that a \$100,000 spending reduction destroys one posting in the US.⁵ My results also show that labour markets adjust to local demand shocks of less public spending along the extensive margin, echoing both the macro-labour literature (Elsby et al., 2009) and public economic works (Komarek et al., 2022).

When the unemployment level is the outcome variable, my results appear counterintuitive but provide rich insight into Poland's labour conditions. The first panel of figure 13 highlights the regression results for three bandwidths of the ruling party's net support and that for all of the provinces. The estimates range from 29.8 to 65.6, all statistically distinct from 0 at the 95% significance level. Their positive sign hints at a negative relationship between province-level expenditure and employment, which is contrary to the findings in public-labour literature (Chodorow-Reich, 2019). For example, the estimated coefficient at the 25% bandwidth indicates that an extra 25,800 *zlotys* ($\simeq \$6,520$) of monthly spending leads to an additional job separation. The estimates' counterintuitive direction could have origins in the data. Poland is characterised by tight labour markets, even after the inflow of Ukrainian refugees in 2022 (IMF, 2023). This likely means that province-level total expenditure can influence the number of job offers while having little impact on unemployment. The changes in unemployment could be a result of unobserved factors, such as the distribution of

⁵ The estimates for Poland and the US are of different magnitudes, but so are the average earnings.

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Ukrainian refugees across the country. The exclusion restriction, assumption A.2, being violated is another culprit. Namely, if the ruling party governs the provinces where the reform exerted a much deeper burden or where the pandemic-related measures had different repercussions than elsewhere, the restriction fails.⁶

I include the reform indicator variable which absorbs the average impact of the tax reform and that of the subsiding pandemic-era reforms and war in Ukraine. As figure 1 indicates, the reform coincided with the graduate phase-out of the pandemic-era stimulus and partially overlapped with the consequences of Russia's invasion of Ukraine. If the pandemic and war-related changes can be shown to have uniform impact across Poland's provinces,⁷ then estimates of β_2 absorb all the bias, leaving the multiplier coefficient of interest, $\frac{\beta_3}{\alpha_3}$, less likely to be influenced. The second panel of figures 12 and 13 present large negative coefficients of the reform indicator, β_2 , in regressions for two types of labour outcomes. In the case of the number of job offers, this means that the number of new openings is negatively correlated with the tax and other changes happening in Poland in 2022. On the other hand, the same changes move in tandem with the overall drop in unemployment. This indicates that the triple impact of austerity, receding pandemic-era support, and military conflict failed to reverse the trend of tightening labour markets in Poland (IMF, 2023).

Finally, I re-run regressions (3a) and (3b) using different sub-categories of spending as the instrumented variable to infer what types of expenditure are likely to drive short-term changes in local labour markets. I pick four categories which are available for every province: education, healthcare, social policy, and municipal economy. Figures 14 and 15 showcase estimates for the number of job offers and unemployment. For the former, education and municipal economy expenditure have no statistically significant impact on labour markets. Interestingly, larger healthcare spending is correlated with fewer jobs being created. Social policy expenditure is associated with more job offers. For the number of unemployed workers, only healthcare and social policy spending impacts

⁶ As visualised on map 3, the *Law and Justice* party rarely governs in large metropolitan areas or the west of Poland (Lukomska and Swianiewicz, 2023). Such regions have traditionally seen higher productivity and output growth rates. The empirical specification is supposed to isolate these differences through province FE and the RD component, but there could be structural differences that I have not captured.

⁷ This could be demonstrated by analysing the temporal and spatial distributions of COVID-19 cases, levels of public financial support to private enterprises, and the location of Ukrainian refugees. With such an extensive analysis requiring access to administrative data, I assume these factors to have a uniform impact across Poland.

are statistically significant (with a negative and positive relationship with unemployment, respectively). For both labour outcomes, the influence of health spending is opposite to that of the total expenditure. Provinces are legally required to fund local healthcare, which effectively implies that increases in health spending among liquidity-constrained territorial units likely come from cuts to other types of spending. This highlights the importance of understanding the impact of binding budget constraints in analysing local budgets ([Adelino et al., 2017](#)).

7 Conclusion

This article asks how labour market outcomes depend on local fiscal consolidations in Poland. To answer the question, I employ the RD-DiD methodology of [Persson \(2020\)](#) and [Lalive \(2008\)](#) in the context of the 2021 *Polish Deal* policy. Leveraging the reform's qualities allows for reaching three conclusions about labour markets in emerging economies, such as Poland, that apply to policymakers. First, local austerity measures impact job creation. I evidence that an extra 175,000 *zlotys* ($\simeq \$44,200$) of monthly spending translates into an additional local job being posted. This constitutes an upper bound of the no-monetary-policy-response multiplier in Poland.⁸ Second, labour markets can adjust to negative demand shocks originating in fiscal policy along the job-creation margin while the overall unemployment level is falling. Third, negative correlations between healthcare spending and labour market outcomes highlight the importance of public expenditure budget constraints in analysing the impacts of local fiscal policies.

The paper's natural extension lies in improving the dataset. For example, I would prioritise accessing administrative data to seek out more frequent and reliable labour variables. I would also group the 72 expenditure categories into statutory and non-statutory spending types to delve into the binding budget constraint argument of [Adelino et al. \(2017\)](#). Nonetheless, given the triple impact of the reform, receding pandemic measures, and the war in Ukraine, I would consider whether using pre-2020 natural experiments is more viable for studying local labour markets in Poland.

⁸ In section 3, I argue that the coefficient is a lower bound of the no-monetary-policy-response multiplier. The cost-per-job is calculated as its inverse, which means that the costs-per-job calculated in this paper serve as an upper bound for their no-monetary policy counterparts.

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A Motivating Evidence: Reform Timeline & Maps

Note that the timeline figure is produced in L^AT_EX.⁹

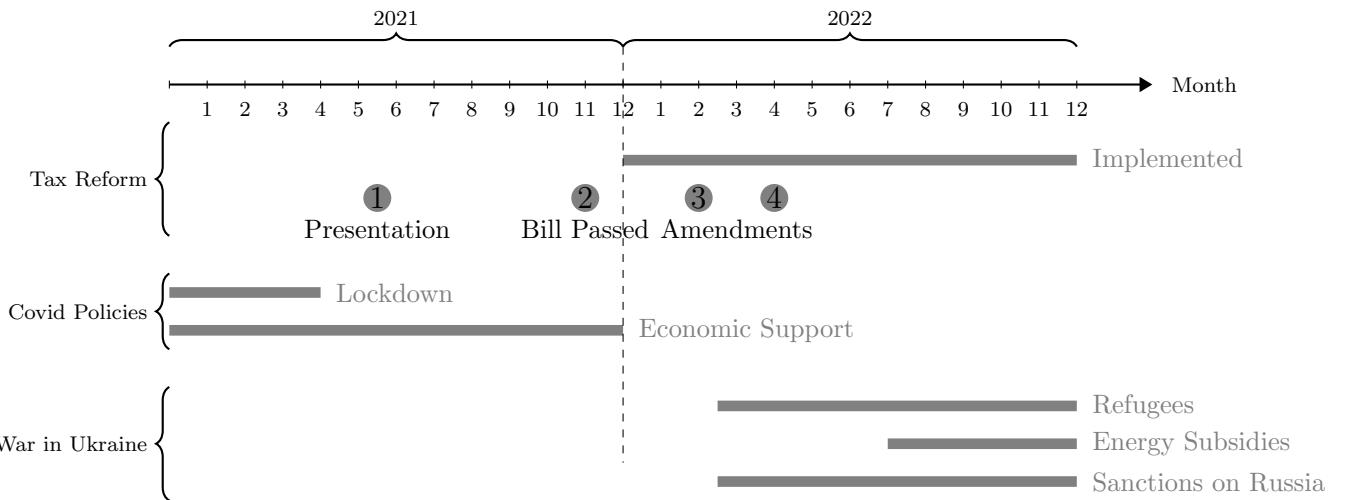


Figure 1: Simplified timeline of Poland’s tax reform milestones, Covid-19 policies, and events related to Russia’s invasion on Ukraine

Sources: Tax reform timeline is based on [Łożykowski \(2023\)](#); [Kańduła \(2023\)](#). The timing of COVID-19 policies is based on news reports.

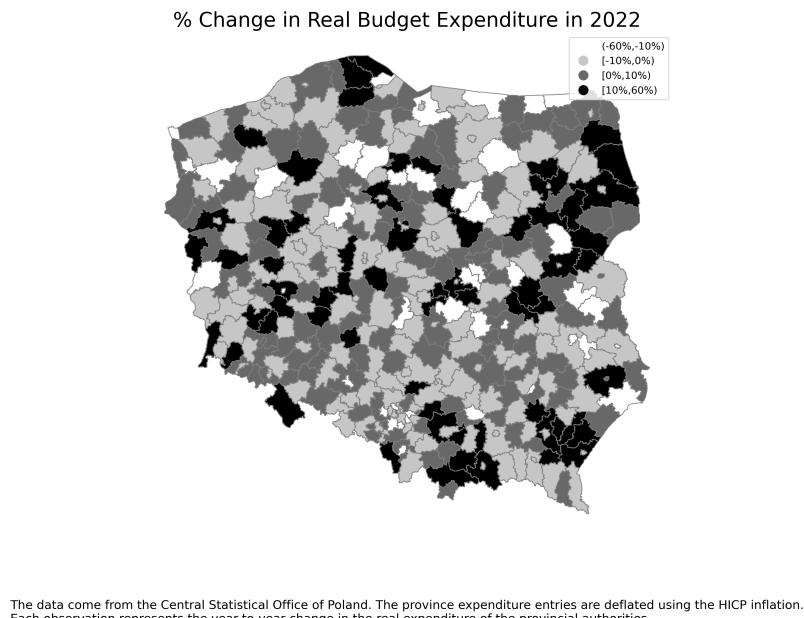
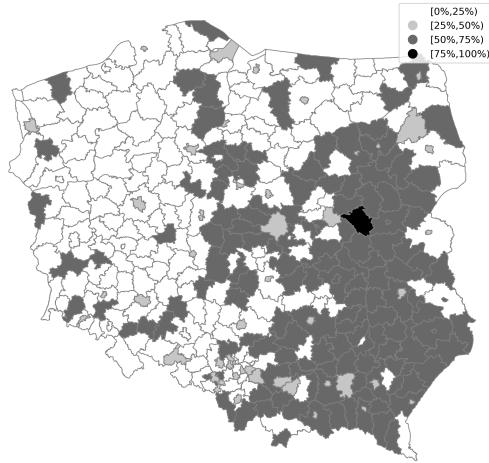


Figure 2: Percentage Change of Total Real Expenditure of Poland’s Provinces between 2021 and 2022.

⁹It will not be reproduced by the replication package.

A. MOTIVATING EVIDENCE: REFORM TIMELINE & MAPS

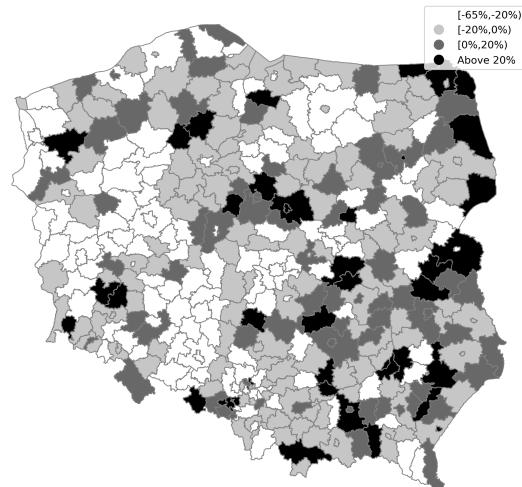
The Law and Justice Support at the Provincial Level



The data come from the 2018 local election pulled from the National Electoral Commission of Poland.

Figure 3: Support of the Ruling *Law and Justice* Party in the Local Elections of 2018.

Change in Average Monthly Number of Job Offers in 2022



The data come from the Central Statistical Office of Poland.

Figure 4: Percentage Change in the Average Monthly Number of Job Offer Per Province between 2021 and 2022.

B. EVIDENCE SUPPORTING EMPIRICAL SPECIFICATION

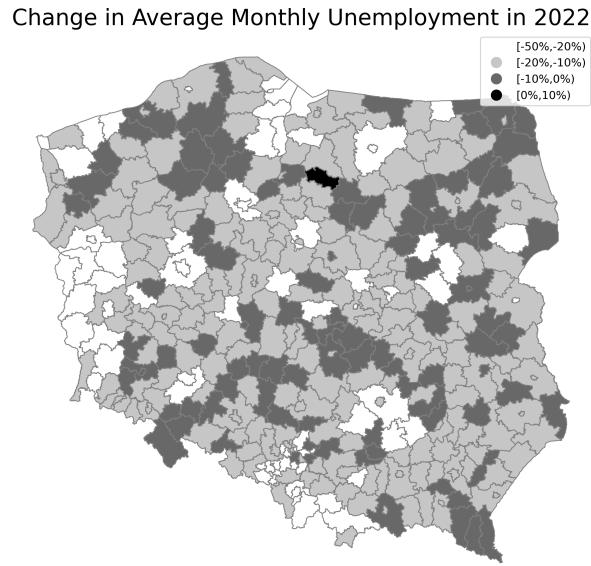


Figure 5: Percentage Change in the Average Monthly Number of Unemployed Per Province between 2021 and 2022.

B Evidence Supporting Empirical Specification

B.1 Dividing Sample into Bandwidths

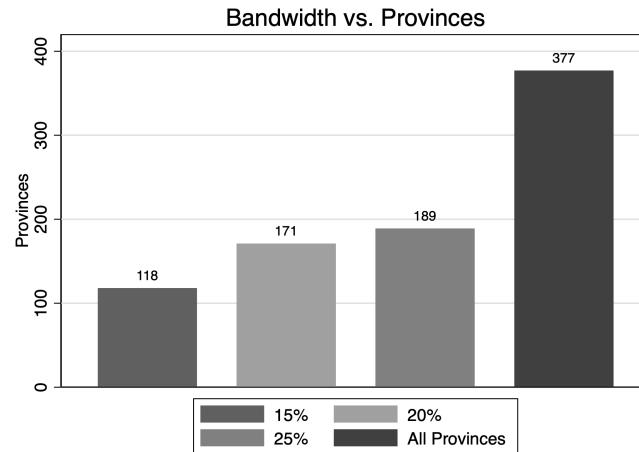


Figure 6: Number of Provinces at Different Bandwidths of the Net *Law and Justice* Party Support.

B.2 Expenditure Changes

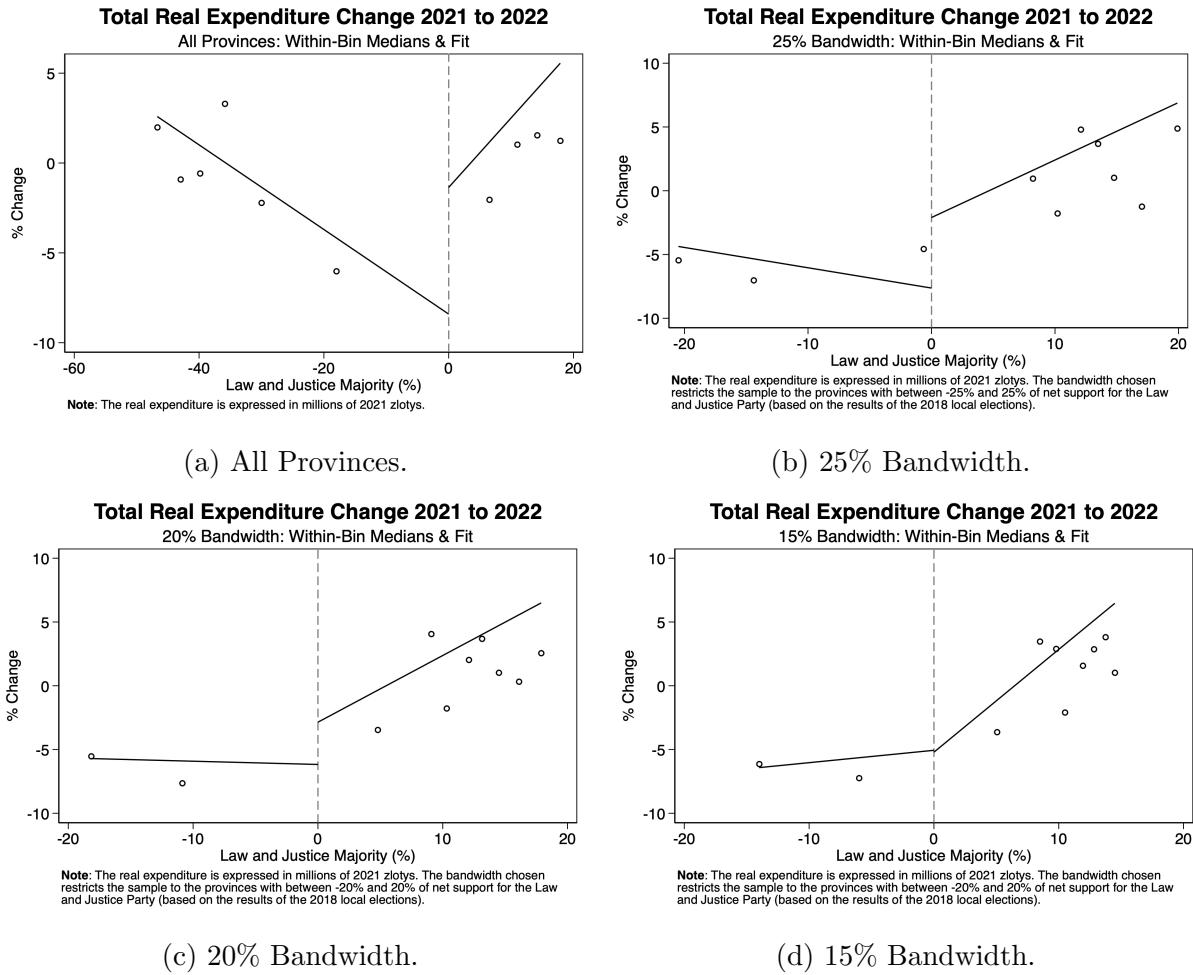


Figure 7: Scatters Plots of Year-to-Year Changes to Real Expenditure in Provinces at Different Levels of Net Support for the *Law and Justice* Party.

B.3 Placebo Tests

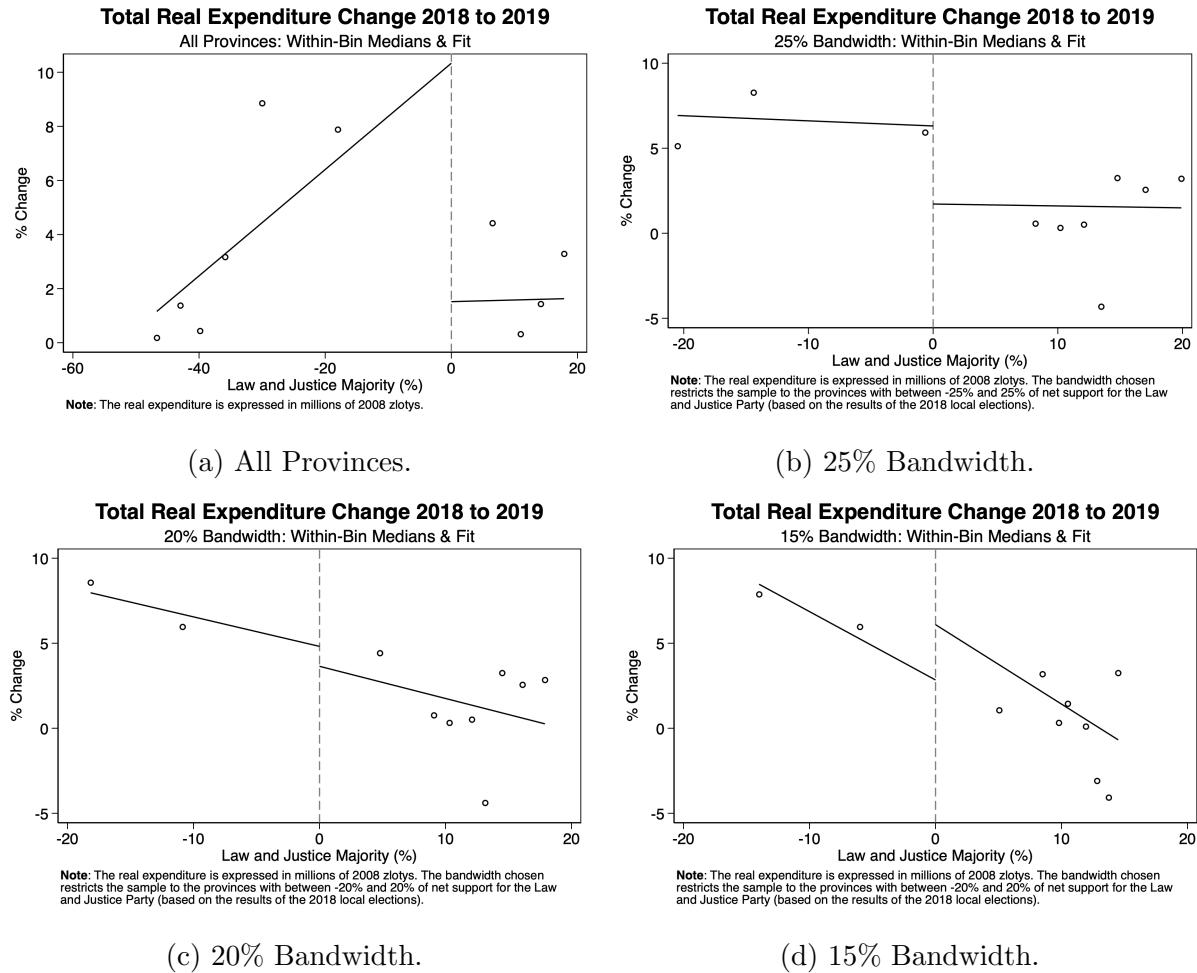


Figure 8: Placebo Testing: Scatters Plots of Year-to-Year Changes to Real Expenditure in Provinces at Different Levels of Net Support for the *Law and Justice* Party (2018-2019).

B.4 Job Offers

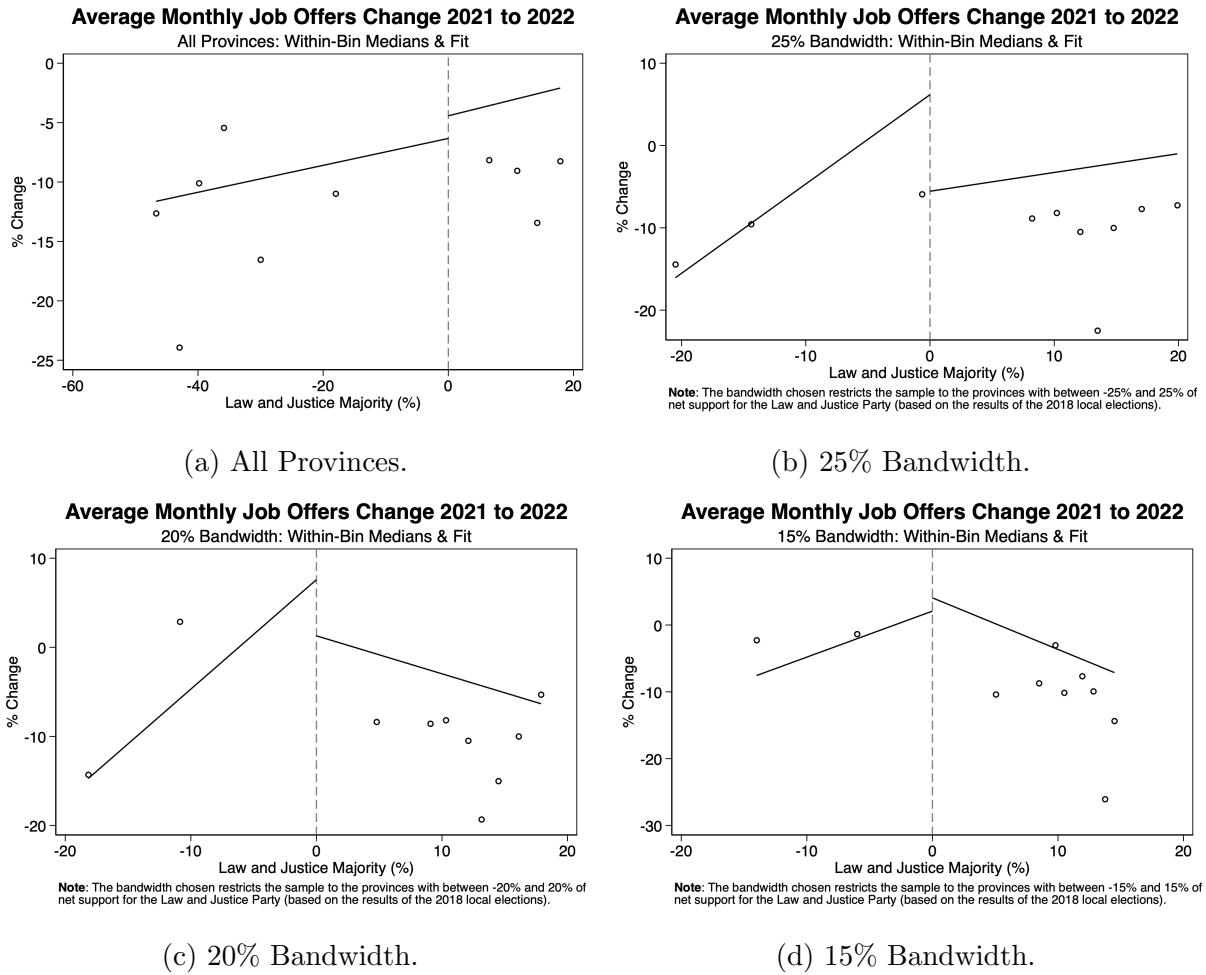


Figure 9: Scatters Plots of Year-to-Year Changes to Job Offers in Provinces at Different Levels of Net Support for the *Law and Justice* Party.

B.5 Unemployment

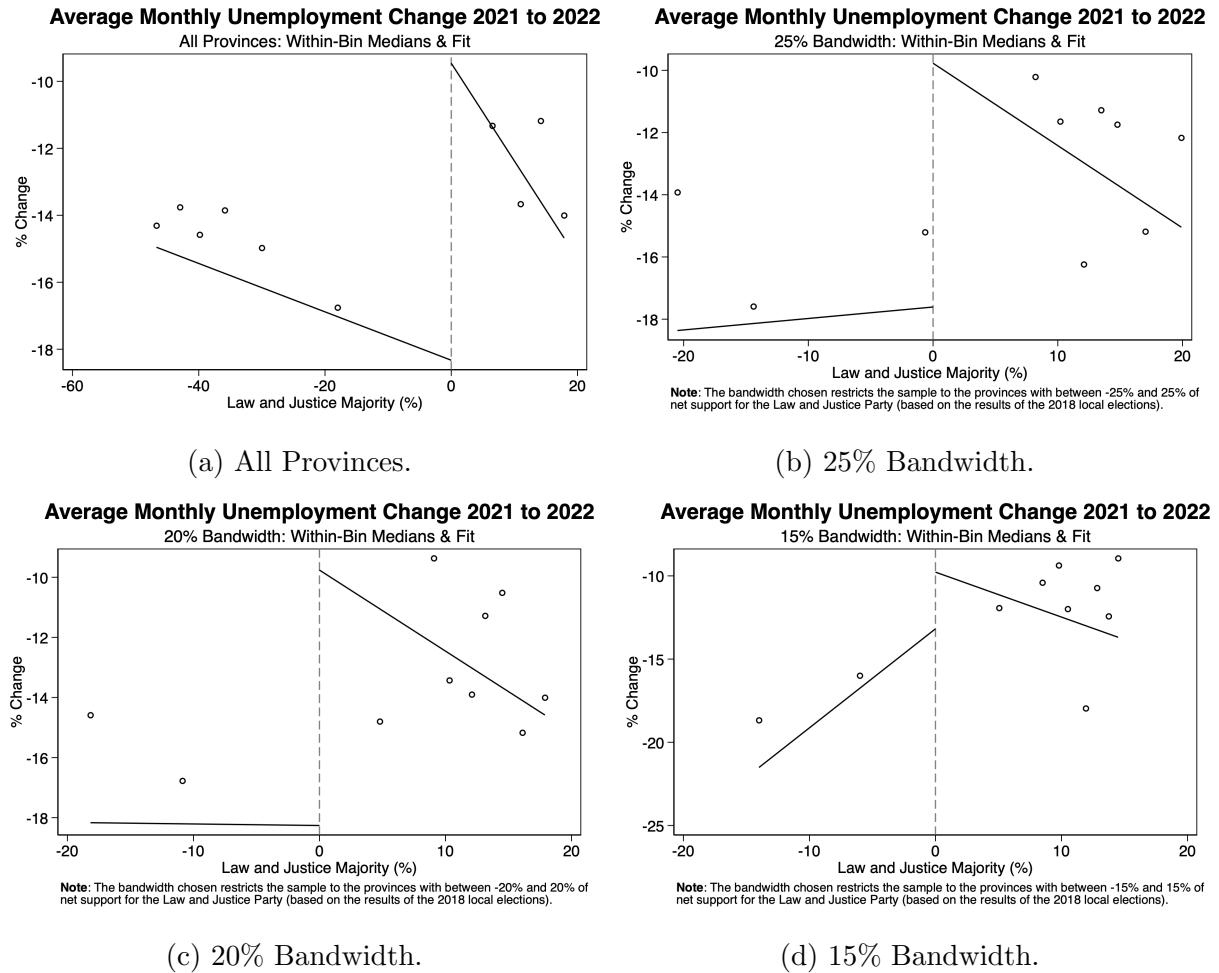


Figure 10: Scatters Plots of Year-to-Year Changes to Unemployment in Provinces at Different Levels of Net Support for the *Law and Justice* Party.

C. KEY RESULTS

B.6 Continuity Assumption

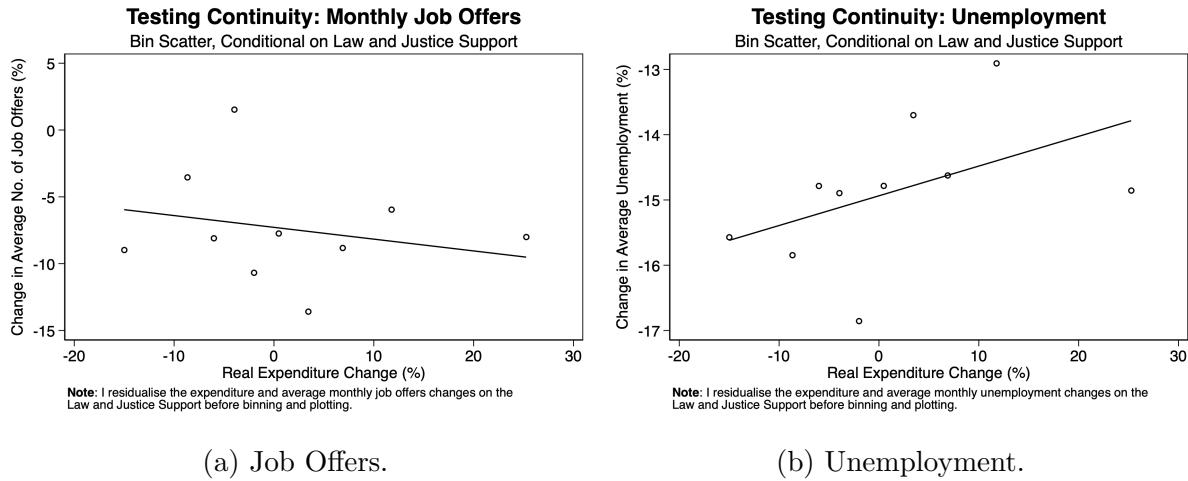


Figure 11: Scatters Plots of Year-to-Year Changes to Unemployment in Provinces at Different Levels of Net Support for the *Law and Justice* Party.

C Key Results

C.1 Job Offers

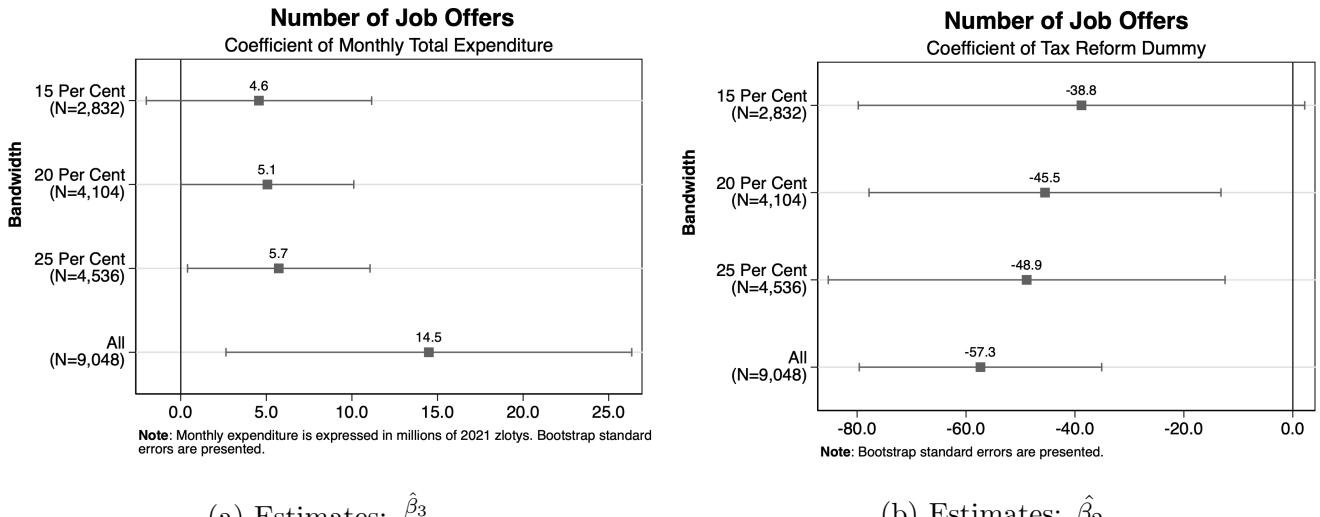
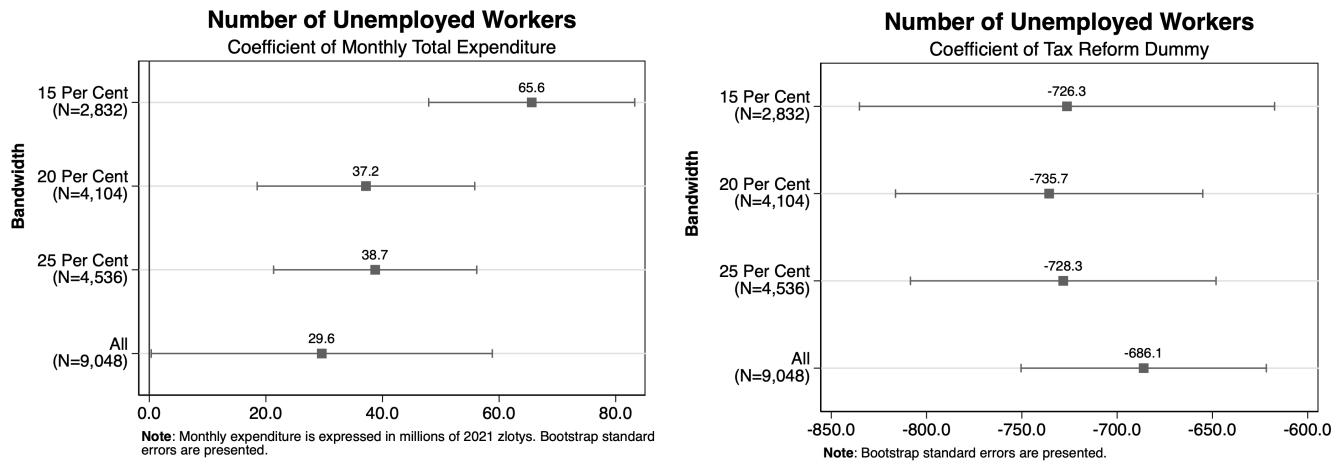


Figure 12: Coefficients of the Monthly Total Expenditure, $\frac{\hat{\beta}_3}{\hat{\alpha}_3}$, and Reform Dummy, $\hat{\beta}_2$, Based on Running Regressions (3a) and (3b) with the Number of Job Offers as the Dependent Variable.

C. KEY RESULTS

C.2 Unemployment



(a) Estimates: $\hat{\beta}_3$.

(b) Estimates: $\hat{\beta}_2$.

Figure 13: Coefficients of the Monthly Total Expenditure, $\hat{\beta}_3$, and Reform Dummy, $\hat{\beta}_2$, Based on Running Regressions (3a) and (3b) with the Number of Unemployed as the Dependent Variable.

D Further Analysis

D.1 Job Offers

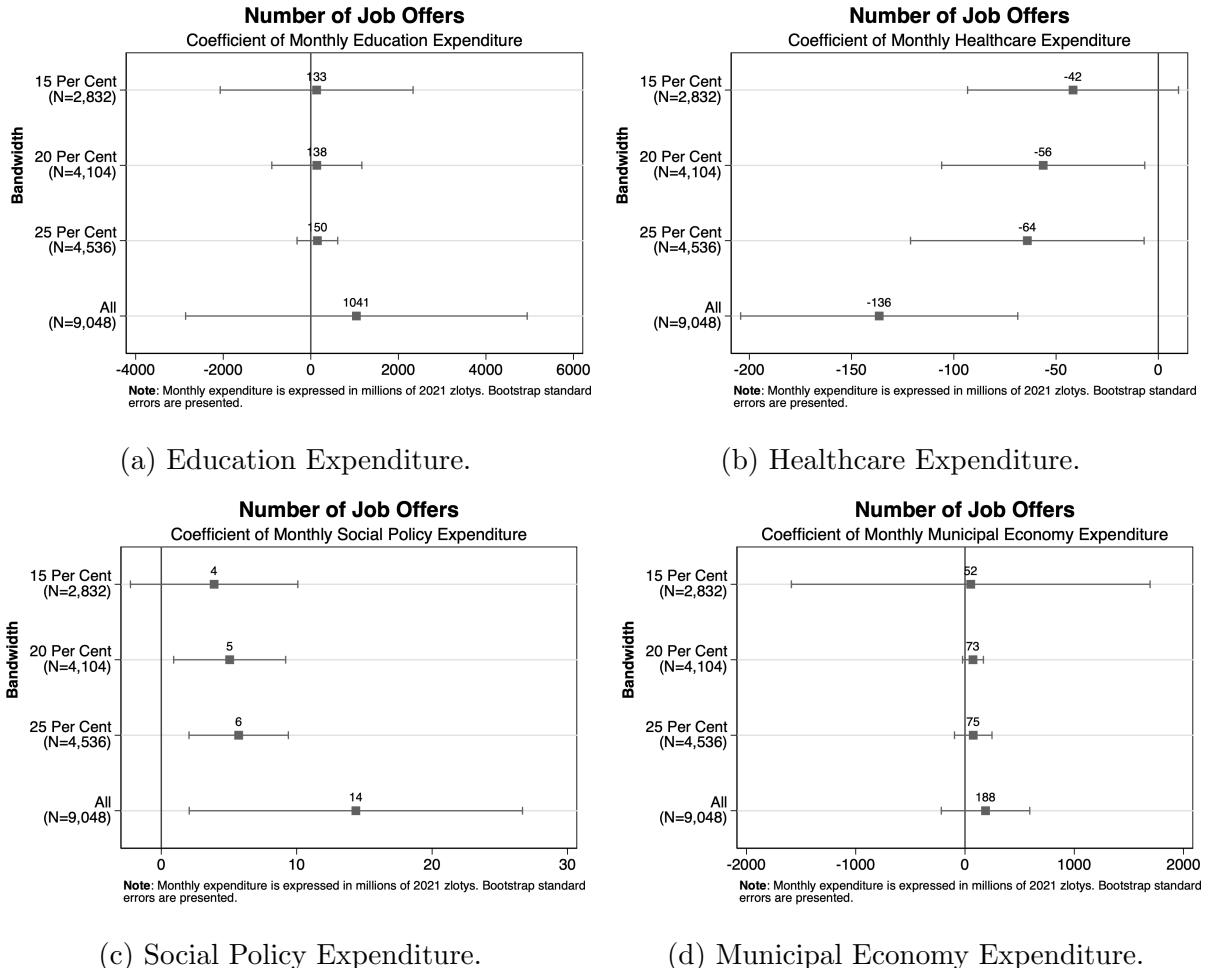


Figure 14: Coefficients of Various Sub-Categories of Total Expenditure, $\frac{\hat{\beta}_3}{\hat{\alpha}_3}$, and Reform Dummy, $\hat{\beta}_2$, Based on Running Regressions (3a) and (3b) with the Number of Job Offers as the Dependent Variable.

D.2 Unemployment

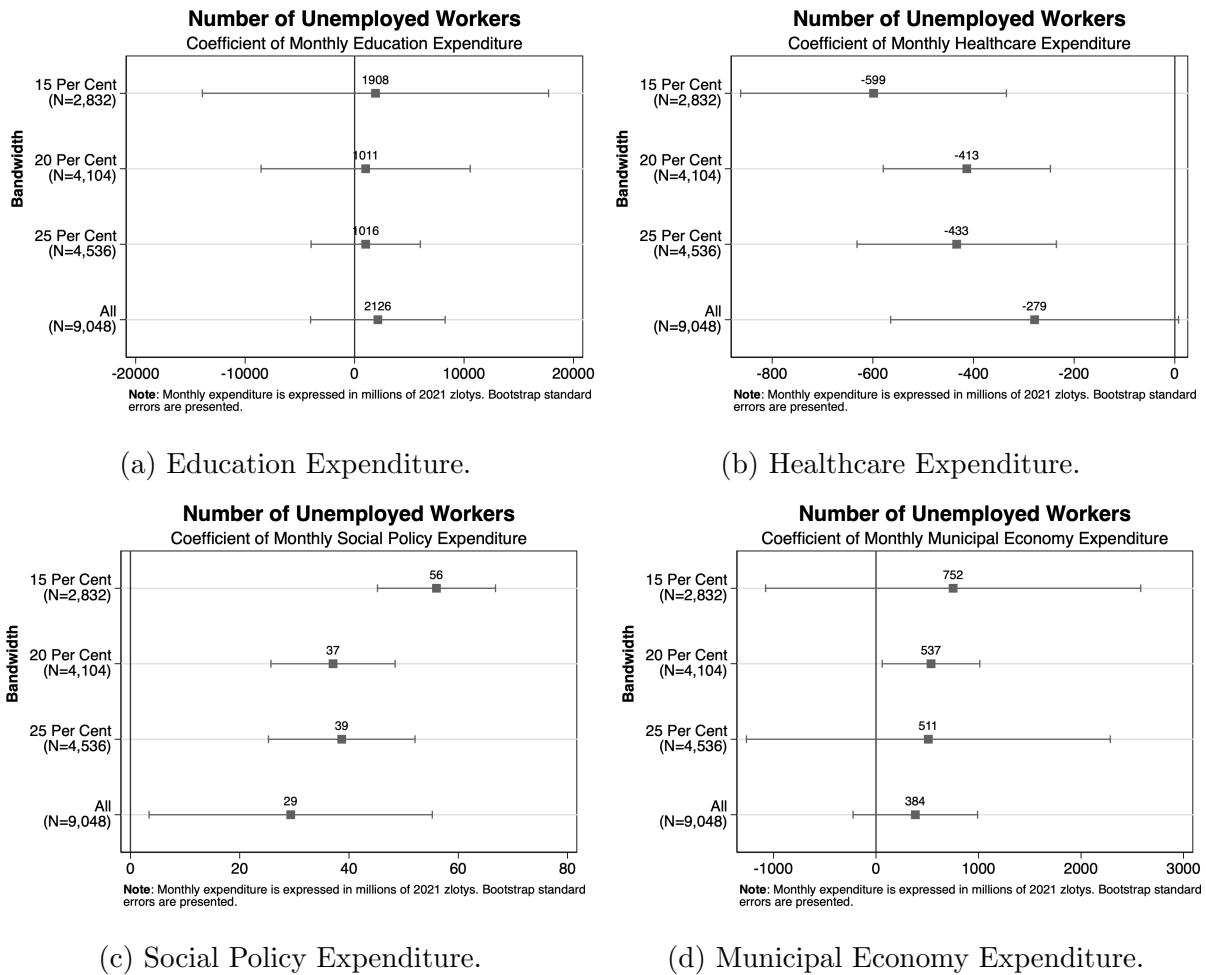


Figure 15: Coefficients of Various Sub-Categories of Total Expenditure, $\hat{\beta}_3$, and Reform Dummy, $\hat{\beta}_2$, Based on Running Regressions (3a) and (3b) with the Number of Unemployed as the Dependent Variable.