RECESSIONS AND REGISTRATIONS:

DO LOCAL ECONOMIC DOWNTURNS

IMPACT VOTER PARTICIPATION?

by

Robert Thomas Hurly

A thesis submitted in partial fulfillment
of the requirements for the degree

of

Master of Science

in

Applied Economics

MONTANA STATE UNIVERSITY
Bozeman, Montana

May 2026

TABLE OF CONTENTS

1. INTRODUCTION 1

2. BACKGROUND 6

2.1 Conceptual Framework: Political Participation Under Economic Stress 6
2.2 Registration versus Turnout as Measures of Participation 8
2.3 What Voter Registration Measures 10
2.4 Empirical Measures of Economic Shock: Sahm and WARN 12
2.5 Institutional Setting: Ohio Voting System 15

3. DATA AND EMPIRICAL STRATEGY 16

3.1 Outcome Data 16
3.2 Empirical Strategy 21
3.3 Treatment Variables 29

4. RESULTS 34

5. INTERPRETATION 38

6. SENSITIVITY AND ROBUSTNESS 41

6.1 Sensitivity to Alternative WARN Specifications 41
6.2 Robustness to Alternative Period Definitions 43
6.3 Additional Specification and Identification Considerations 45

7. CONCLUSION 50

REFERENCES CITED 52

APPENDICES 56

APPENDIX A : Additional Tables 57
APPENDIX B : Additional Figures 60
APPENDIX C : Ohio Institutional Details 69
APPENDIX D : Use of the Sahm Rule in Policy and Government Analysis 72
D.1 Federal-Level Implementation and Policy Use 73
D.2 Broader Applied and Subnational Use 74

LIST OF TABLES

Table Page

1. Table 1 Descriptive Statistics for the Main County-Month Analysis Samples 18
2. Table 2 Main Results for Total Registrations 34
3. Table 3 Main Results for First-Time Registrations 36
4. Table 4 Alternative WARN Specifications for Total Registrations 42
5. Table 5 Alternative Period Definitions for Total Registrations 44
6. Table 6 Alternative WARN Specifications for First-Time Registrations 58
7. Table 7 Alternative Period Definitions for First-Time Registrations 59

LIST OF FIGURES

Figure Page

1. Figure 1 Monthly Mean Registration Rates across the Inter-Recession Windows 19
2. Figure 2 Cross-County Distribution of Average Monthly Registration Rates 20
3. Figure 3 Share of Counties Not Yet Treated over Time 25
4. Figure 4 TWFE Event-Study Estimates of Registration Effects 26
5. Figure 5 Sun and Abraham Event-Study Estimates of Registration Effects 28
6. Figure 6 County Sahm Trigger Timing, Window 2 31
7. Figure 7 Baseline and Two-Month Sahm Trigger Specifications 46
8. Figure 8 Ohio State Unemployment and State Sahm Trigger Timing 48
9. Figure 9 Distribution of First Treatment Onset by County 61
10. Figure 10 Cross-County Distribution of Average Monthly First-Time Registration Rates 62
11. Figure 11 TWFE Event-Study Estimates for First-Time Registrations 63
12. Figure 12 Sun and Abraham Event-Study Estimates for First-Time Registrations 64
13. Figure 13 County Sahm Trigger Timing, Window 1 65
14. Figure 14 County Sahm Trigger Timing, Window 2 66
15. Figure 15 WARN Notice Timing by County, Window 1 67
16. Figure 16 WARN Notice Timing by County, Window 2 68

ABSTRACT

This paper examines how local economic shocks affect political participation, focusing on voter registration as an early-stage margin of engagement. While a sizable literature studies the impact of economic conditions on voter turnout, less is known about how these conditions influence the decision to enter the electorate. Voter registration requires forward-looking effort and information acquisition, implying that economic shocks may affect participation through both resource constraints and attention channels. I test these predictions using a county-month panel of voter registrations in Ohio from 2000 to 2024, constructed from state voter files and combined with measures of local economic shocks based on Sahm rule recession indicators and mass layoff events from WARN notices. Across specifications, I find little evidence that local economic shocks meaningfully affect registration behavior. Neither measure produces statistically significant changes in registrations. These results hold across alternative specifications, event-study designs, and after excluding election-driven registration spikes. I interpret this pattern as consistent with models in which participation decisions made well in advance of elections are less responsive to short-run economic conditions, even when such conditions influence later-stage behaviors such as turnout or vote choice.

CHAPTER ONE

INTRODUCTION

Political participation is a foundational component of democratic systems, determining both the distribution of political power and the responsiveness of elected officials to public preferences.[1] In representative democracies such as the United States, participation governs who is included in the policymaking process and whose interests are reflected in electoral outcomes. When participation is uneven across individuals or regions, representation becomes systematically biased, raising concerns about equity and democratic accountability.[2] This naturally leads to a fundamental question: to what extent are participation decisions shaped by external economic conditions?

A central claim in the existing literature is that economic conditions are among the most salient forces influencing political behavior. Individuals experience economic change both directly—through employment, income, and financial stability—and indirectly through broader macroeconomic conditions.[3] Prior work has shown that economic outcomes shape vote choice and incumbent support, with downturns often leading to electoral punishment of incumbents. However, considerably less attention has been devoted to the question of whether economic conditions influence the decision to participate at all. Existing research offers competing theoretical predictions. On one hand, economic hardship may

[1] Research by Cantoni et al. (2024) highlights that electoral participation is central to both democratic legitimacy and policy outcomes, as variation in participation directly influences government decision-making and representation.

[2] Liu et al. (2025) show that disparities in political participation can shape the size and scope of government policy, emphasizing how unequal engagement translates into unequal political influence.

[3] Bloom and Price (1975) and Kramer (1971) provide foundational evidence that macroeconomic conditions play an important role in shaping electoral behavior, particularly through their effects on incumbent support, forming the basis of the economic voting literature.

mobilize individuals by increasing dissatisfaction and the perceived importance of political engagement. On the other, it may induce withdrawal by increasing the costs associated with participation and reducing available resources. Empirical findings reflect this ambiguity, with evidence supporting both mobilization and withdrawal effects depending on context and measurement.[4] This unresolved tension highlights a gap in our understanding of how economic shocks influence the extensive margin of participation.

This ambiguity is compounded by a key limitation in the existing literature: the overwhelming focus on voter turnout as the primary measure of political participation. Turnout captures the final act of engagement, but it obscures earlier decisions that are necessary for participation to occur. In many institutional settings, including much of the United States,[5] individuals must first register to vote before they can cast a ballot. Registration represents a distinct behavioral margin with its own costs, timing, and informational requirements. It is often more administratively burdensome and less salient than voting itself, requiring individuals to complete bureaucratic steps prior to peak political engagement periods. This distinction implies that registration may respond differently to economic conditions than turnout, as it reflects an earlier and potentially less time-sensitive decision. By focusing exclusively on turnout, the literature may overlook an important dynamic in how individuals enter the electorate.

This paper addresses this gap by examining voter registration as a measure of political participation. Specifically, it asks whether local economic shocks influence the decision to register to vote, thereby affecting participation at its earliest stage. This approach contributes to the literature in two primary ways. First, it introduces voter registration as a novel and underexplored outcome variable in the study of political participation.

[4]Evidence of these effects has differing names across the literature, but this paper refers to them as "withdrawal" when costs to voting push potential voters out of the electorate and "mobilization" when the electorate grows in response to economic impacts.

[5]26 states require registration prior to voting, with several others having limited early voting periods (Same Day Voter Registration, n.d.).

By shifting attention from turnout to registration, the analysis captures a dimension of engagement that reflects entry into the electorate rather than final participation. Second, the paper provides causal evidence using two complementary measures of local economic shocks. Most importantly, it adapts the Sahm rule—a measure used to identify the onset of recessions (US Business Cycle Expansions and Contractions, n.d.)—to a county-level analysis. This allows for the identification of localized labor market downturns in near real time. This represents a methodological contribution, as it enables the detection of economically meaningful shocks before they are reflected in conventional recession dating or broader macroeconomic indicators. In parallel, the analysis incorporates Worker Adjustment and Retraining Notification (WARN) notices as a measure of salient, discrete job loss events, which are both highly localized and forward-looking indicators of labor market deterioration (Krolikowski & Lunsford, 2024). Together, these measures distinguish between diffuse and salient economic shocks, providing a unified empirical framework to test competing theories of political behavior.

The empirical analysis is conducted in the context of Ohio, which provides a consistent institutional setting with meaningful variation in local economic conditions over time. Ohio requires voter registration a minimum of 30 days prior to Election Day and does not allow same-day registration, introducing administrative frictions that may influence participation decisions. Over the study period, which spans multiple inter-recession expansions between 2001 and 2024, these institutional rules remain relatively stable, allowing for a clean examination of how economic conditions affect behavior. The analysis leverages county-level variation to construct a county–month panel dataset of voter registrations, including both total and first-time registrations normalized by the voting-age population. State voter files provide precise and comprehensive measures of local registration activity. This panel structure facilitates comparison and estimation of the causal effects of local economic shocks on voter registration.

To estimate these effects, the analysis begins with a two-way fixed effects (TWFE) specification that exploits variation in the timing of local economic shocks across counties. This approach controls for time-invariant differences across counties and common shocks over time, isolating within-county changes in voter registration associated with changes in local economic conditions. Identification therefore comes from comparing counties experiencing a shock to those that have not yet experienced one within the same period. Windows of analysis are defined between national recessions,[6] ensuring that estimates are not driven by broad national downturns and instead reflect localized economic variation. Building on this baseline, an event study framework follows to examine the dynamic response of voter registration before and after the onset of economic shocks. This allows for a more flexible assessment of timing, including tests for pre-trends and the persistence of effects over time. Together, the TWFE and event study specifications provide a complementary approach: the former offers a parsimonious estimate of average effects, while the latter illustrates the underlying dynamics and supports the credibility of the identification strategy.

The main findings indicate no consistent effect of local economic shocks on voter registration rates. Across specifications and measures of economic conditions, estimated effects are generally small and statistically indistinguishable from zero. This result contrasts with parts of the turnout literature that find meaningful responses to economic conditions and suggests that registration may be less sensitive to short-run shocks. One interpretation is that the competing mechanisms of mobilization and withdrawal offset one another. Another is that institutional frictions and the timing of registration decisions dampen responsiveness to economic conditions. These findings highlight the importance of distinguishing between different margins of participation and suggest that the relationship between economic conditions and political engagement may depend critically on how participation is measured.

[6]Using official business cycle dates from the National Bureau of Economic Research (NBER), as reported in U.S. Business Cycle Expansions and Contractions (n.d.).

The remainder of the paper proceeds as follows. Chapter Two develops the theoretical framework, outlining the mechanisms through which economic shocks may influence participation and distinguishing between registration and turnout. Chapter Three describes the data and empirical strategy, including the construction of outcome variables, treatment measures, and the identification approach. Chapter Four presents the main results, including descriptive evidence and event study estimates. Chapter Five interprets the findings in the context of competing theoretical mechanisms. Chapter Six provides robustness checks and sensitivity analyses. Chapter Seven concludes with implications for the measurement of political participation and directions for future research.

CHAPTER TWO

BACKGROUND

2.1 Conceptual Framework: Political Participation Under Economic Stress

The question of why people participate in the political process, particularly in response to broader economic conditions, has been a long-time ponderance for the realm of political economy. Political participation can be understood as the outcome of a decision-making process in which individuals weigh the expected benefits of engagement against its associated costs, subject to constraints on resources and information. This framework originates with the rational choice model of voting developed by Downs (1957); a cost-benefit model in which individuals participate if the expected utility of doing so exceeds the associated costs. However, this formulation gives rise to the well-known "paradox of voting," (Dhillon & Peralta, 2002) as the probability that any single vote is pivotal approaches zero in large electorates. Subsequent theoretical work resolves this paradox by expanding the participation decision beyond purely instrumental considerations. Dhillon and Peralta (2002) synthesize these extensions, emphasizing that participation depends not only on expected policy influence, but also on expressive benefits, informational frictions, and resource constraints. Individuals may derive utility from expressing political preferences (Fiorina, 1976; Schuessler, 2000), fulfilling civic or ethical obligations[7] (Feddersen & Sandroni, 2006; Harsanyi, 1980), or acting under uncertainty and limited information (Feddersen & Pesendorfer, 1996). These extensions imply that participation is best understood as a function of three core components: costs, resources, and motivation. This framework provides a natural foundation for analyzing how economic conditions affect political behavior. Economic shocks directly influence individuals' resources (income, employment, time availability) and indirectly shape

[7]Often described as a "warm glow" effect.

their motivations (perceptions of government performance, political dissatisfaction). As a result, economic conditions can alter participation decisions even when the probability of being pivotal remains negligible.

Within this framework, the literature identifies two competing mechanisms through which economic shocks affect political participation: a withdrawal effect and a mobilization effect. The withdrawal effect arises from cost-based and resource-driven models of participation. Negative economic conditions—such as unemployment, income loss, or financial instability—reduce the resources available for political engagement and increase the opportunity cost of participation. Individuals facing economic hardship may reallocate time and attention toward job search, income stabilization, or other immediate concerns, thereby reducing their likelihood of participating. Empirical evidence shows that unemployment, poverty, and financial distress significantly reduce voter turnout, as individuals "spend scarce resources holding body and soul together" (Rosenstone, 1982) rather than engaging in politics. Swartz et al. (2009) and Martins and Veiga (2013) find that economic hardship has the capacity depress civic participation to degrees that vary depending on context and institutional setting. Economic conditions may also affect participation through information channels, as increased labor market activity reduces exposure to political information (Charles & Stephens Jr., 2013). These effects are particularly relevant for activities that require planning, information acquisition, or administrative effort. Voter registration, for instance, imposes additional costs relative to voting itself (Wolfinger & Rosenstone, 1980).

In contrast, the mobilization effect emerges from models emphasizing expressive motivations and grievance-based political behavior. Economic hardship may increase participation by heightening dissatisfaction, raising the perceived stakes of political decisions, and strengthening incentives to influence policy outcomes. Individuals experiencing adverse economic conditions may attribute their situation to government policy or incumbent performance, increasing their likelihood of engaging politically. Empirical studies provide evidence consistent with this mechanism. Radcliff (1992) finds that economic conditions can increase participation through broader political engagement, while Burden and Wichowsky (2014) show that economic discontent can mobilize voters, particularly when unemployment is salient and politically attributable. Earlier work by Bloom and Price (1975) also suggests asymmetric political responses to economic downturns, consistent with grievance-driven participation.

Importantly, these mechanisms are not mutually exclusive as both can operate simultaneously, with their relative strength determining the net effect of economic shocks on participation. This coexistence generates ambiguous theoretical predictions: economic shocks may either increase or decrease participation depending on whether resource constraints or grievance-driven motivations dominate. At the aggregate level, these opposing forces may offset one another, producing net effects that are small or statistically indistinguishable from zero. While much of the existing literature evaluates these mechanisms using voter turnout, the underlying theoretical models apply more broadly to participation decisions. This provides a natural foundation for examining voter registration as a distinct margin of political engagement.

2.2 Registration versus Turnout as Measures of Participation

Voter registration and voter turnout represent distinct stages of political participation, each characterized by different costs, timing, and behavioral dynamics. While turnout has been the primary focus of the literature,[8] registration reflects an earlier decision that is necessary for participation to occur in many institutional settings. This distinction is critical

[8]The empirical literature on political participation has focused predominantly on voter turnout, with extensive work examining how economic conditions influence voting behavior (e.g., Rosenstone, 1982; Radcliff, 1992; Martins and Veiga, 2013; Charles and Stephens, 2011). By contrast, voter registration has received comparatively little attention in economic analyses, despite its role as a prerequisite for participation in many institutional settings (Wolfinger and Rosenstone, 1980).

for understanding how economic conditions affect political engagement, as the mechanisms driving registration may differ from those influencing turnout.

A central claim of this paper is that voter registration captures a latent or preparatory form of political engagement that is not fully reflected in turnout measures. Rather than representing the act of participation itself, registration can be understood as an upfront investment required to enter the political process. In this sense, it functions as a fixed cost of voting that must be paid in advance, lowering the marginal cost of participation on Election Day. By incurring this cost, individuals effectively make a soft commitment to participate in future elections, separating the decision to engage politically from the act of voting itself. Unlike voting, which occurs at a fixed and highly salient moment, registration requires individuals to take action prior to peak political attention, often in a lower-information environment. This process involves administrative and informational costs, including awareness of deadlines, completion of forms, and verification of eligibility. Because these costs must be incurred before participation is even possible, registration decisions are particularly sensitive to constraints on time, information, and cognitive resources.

This distinction has important implications for how economic shocks affect behavior. On the one hand, the timing of registration may attenuate immediate mobilization effects. Because registration is not tied to a specific moment of political action, individuals experiencing economic shocks may delay or forgo registration in the short run.[9] At the same time, the role of registration as an upfront cost of voting introduces the potential for longer-run mobilization. As individuals update their expectations about the importance of future political participation, they may choose to incur the cost of registration in advance, effectively preparing to vote when the opportunity arises. In this sense, mobilization may operate with a delay, reflecting the separation between the decision to enter the electorate

[9] In the presence of binding resource constraints, attention may be directed toward immediate needs such as job search or income stabilization, postponing lower-priority administrative actions like registration, even if those shocks increase political dissatisfaction.

and the act of voting itself.

On the other hand, economic hardship is likely to amplify withdrawal effects at the registration stage. Because registration represents a fixed cost required to enter the electorate, individuals facing constraints on time, income, or cognitive resources may be less willing or able to incur these costs when economic conditions deteriorate. In this framework, withdrawal operates not through reduced turnout among existing voters, but through a reduced likelihood of entering the electorate in the first place. As a result, economic shocks may suppress political participation at its earliest stage, preventing some individuals from ever reaching the point at which turnout decisions are made.

2.3 What Voter Registration Measures

A voter registration captures multiple forms of political behavior beyond simple entry into the electorate, reflecting a range of underlying individual decisions. Observed registrations include not only individuals choosing to participate in politics for the first time, but also those updating or reactivating their registration status. These updates may arise from routine life events—such as residential moves, name changes, or changes in eligibility—as well as deliberate re-engagement with the political process. As a result, registration data reflect both intentional political decisions and administrative adjustments required to maintain eligibility. This highlights that not all non-registered individuals are equivalent. Some individuals have never entered the electorate despite being eligible, potentially reflecting low political interest, high participation costs, or persistent disengagement. Others may have previously been registered but have not maintained an active status due to mobility, institutional barriers, or inattention. Consequently, variation in registration activity captures both the decision to newly enter the electorate and the decision to remain engaged over time.

There is clearly utility in differentiating what mechanism is motivating a potential voter to register. First-time registrations include individuals who are newly entering the electorate, but even within this group, there is important heterogeneity. Some first-time registrants represent individuals who could have participated in prior elections but did not, and whose registration may reflect mobilization or changing political preferences. Younger individuals of this group are newly eligible to participate, having reached voting age since the previous election cycle. These "newly eligible" registrants differ conceptually from other first-time entrants, as their participation is constrained by institutional eligibility rather than prior disengagement. This motivates the importance of separate outcome measures to distinguish the specific impact upon political participation. Despite this conceptual importance, data constraints restrict empirical analysis to focus on observable margins of registration, comparing total registrations to first-time registrations defined by age-based eligibility constraints.[10]

Aggregate registration measures also capture individuals with an underlying propensity to engage politically, independent of specific elections. Some registrations may reflect general civic engagement or responsiveness to local political dynamics rather than reactions to national electoral cycles. This is particularly relevant given that a single registration confers eligibility to participate across all levels of government. In Ohio, voter registration grants access to local, state, and federal elections simultaneously, meaning that observed registration activity reflects engagement with the political system broadly rather than any single contest. Turnout among registered voters ranges widely—from roughly 30% in low-salience off-cycle elections to over 70% in presidential elections.[11] These patterns imply that a meaningful portion of the voting-age population is either unregistered or registered but does not vote in a given election. As a result, three distinct groups emerge: individuals who are not

[10]First-time registration is identified using age-based eligibility thresholds, capturing individuals who could not have participated in prior elections. Other forms of entry into the electorate—such as previously eligible but unregistered individuals—are not separately observed in the data. This distinction is revisited in the data section.

[11]These are descriptive means derived from State voter files and OH government reports on voter turnout (Secretary of State Data Portal, n.d.)

registered, those who are registered but do not vote, and those who are registered and do vote. Separating these stages highlights the importance of treating registration and turnout as separate margins of political participation. Taken together, these features inform a capacity to treat registration as a distinct outcome variable rather than a proxy for turnout. By capturing both entry into and maintenance of electoral participation, registration provides a more comprehensive view of how individuals engage with the political process and how economic conditions may influence that engagement at its earliest stages.

2.4 Empirical Measures of Economic Shock: Sahm and WARN

To operationalize the competing theoretical mechanisms outlined above, this paper focuses on two types of local economic shocks: broad labor market downturns and acute job displacement events. These shocks are captured using the Sahm rule and Worker Adjustment and Retraining Notification (WARN) data, respectively, each of which provides a different lens on economic distress.

The Sahm rule identifies the onset of economic downturns based on sustained increases in unemployment rates. Specifically, it triggers when the three-month moving average of the unemployment rate rises by at least 0.5 percentage points above its minimum value over the previous twelve months.[12] Substantively, this condition indicates that the labor market has deteriorated in a persistent and non-transitory way: unemployment is not simply fluctuating, but has shifted upward relative to its recent baseline. A Sahm trigger can be interpreted as signaling a meaningful and ongoing weakening of local economic conditions rather than a short-lived shock. Originally developed by Claudia Sahm, the eponymous Sahm rule provides a real-time indicator of recession onset based on sustained increases in unemployment (Sahm, 2019). The rule has been widely adopted in macroeconomic and policy applications, including

[12] The precise construction of this measure, including moving average calculations and window definitions, is detailed in the Data section.

extensions to state-level triggers.[13] Its application at the county level extends this logic to a more localized setting, allowing for the identification of sub-state economic downturns that may be masked in aggregate data. However, this finer level of aggregation may introduce additional noise due to smaller sample sizes.

Conceptually, Sahm-based shocks represent diffuse and slow-moving economic deterioration.[14] They capture conditions that are broadly felt across a local economy and that emerge gradually over time. This diffuse nature has important implications for political behavior. On one hand, sustained economic distress may induce withdrawal: individuals facing prolonged unemployment or income loss may experience tighter resource constraints, higher opportunity costs of participation, and reduced attention to politics, all of which depress engagement (Rosenstone, 1982). On the other hand, such conditions may also generate mobilization if individuals attribute worsening economic outcomes to political actors and seek redress through participation. However, because Sahm-type shocks lack a clear focal event and evolve gradually, they may be less likely to generate immediate or salient mobilization responses. The attribution of responsibility is more diffuse, and the signal itself may be partially discounted by individuals who do not perceive a discrete change in conditions.

In contrast, WARN notices capture sharp, discrete job displacement events and thus provide a measure of acute economic shocks. Under the WARN Act, firms with more than 100 employees are required to provide advance notice—minimum of 60 days—of mass layoffs or plant closures. These notices identify specific, observable instances of job loss that are geographically concentrated and temporally well-defined. Importantly, WARN data have

[13]Prior work applies the Sahm rule at the state level , while extensions and refinements for empirical implementation are discussed in Feng & Sun (2020). Additional examples of government use of the Sahm rule at both national and state levels are provided in Appendix E.

[14]The characterization of Sahm-based shocks as "slow-moving" is relative to the discrete and highly salient nature of WARN events discussed below. While the Sahm rule captures gradual and diffuse labor market deterioration, it is nonetheless designed as a real-time indicator and typically identifies downturns substantially earlier than official recession dating by the National Bureau of Economic Research (NBER).

been shown to provide a timely and leading indicator of job separations and broader labor market deterioration (Krolikowski & Lunsford, 2024).

The discrete and salient nature of WARN events makes them particularly well-suited to capturing mobilization dynamics. Sudden job loss is more likely to be perceived as a concrete and attributable shock, potentially increasing political awareness, generating grievances, and heightening demand for policy responses. This aligns with theories in which economic hardship can spur participation when individuals view their circumstances as a collective or systemic issue. At the same time, WARN events may also induce withdrawal effects through the same resource-based channels described above. The net effect is therefore theoretically ambiguous, but the salience and immediacy of these shocks suggest a relatively stronger potential for mobilization compared to Sahm-based measures.

A key distinction between these measures lies in their scope and intensity. Sahm captures broad, cumulative labor market deterioration that affects a wide share of the local population, whereas WARN reflects concentrated shocks affecting a smaller subset of individuals. As a result, WARN events may generate strong behavioral responses among directly affected individuals but weaker aggregate effects, particularly in larger counties where the affected population represents a smaller share of the total.[15] Taken together, the use of the differing measures enables a distinction between diffuse and acute economic shocks central to the empirical strategy. If mobilization effects dominate, one would expect stronger responses to salient, discrete shocks such as WARN events. If withdrawal effects dominate, broader and more persistent downturns captured by Sahm indicators should lead to reduced participation. Comparing responses across these measures therefore provides a means to highlight the mechanisms linking economic conditions to political behavior.

[15] This difference is particularly relevant due to the outcomes being measured on a per voting-age population basis for comparability across counties.

2.5 Institutional Setting: Ohio Voting System

The empirical analysis focuses on the state of Ohio, which provides a stable institutional environment with meaningful variation in local economic conditions over time. A central feature of Ohio's electoral system is that individuals must have registered to vote at least 30 days prior to Election Day, creating a clear temporal separation between the decision to register and the act of voting that is constant across counties and over time. For the purposes of this analysis, the key implication of this institutional structure is that registration decisions cannot be adjusted contemporaneously with Election Day participation. Observed registration activity reflects decisions made within constrained administrative windows. This feature is particularly relevant when interpreting the timing of responses to economic shocks, as short-run fluctuations may not align with active registration periods.

Ohio's institutional environment is otherwise characterized by relative stability, allowing variation in registration behavior to be attributed primarily to changes in local economic conditions rather than shifting electoral rules. While additional institutional features—such as election timing and administrative procedures—may influence participation, these remain broadly consistent across the sample and are therefore unlikely to confound within-state comparisons.[16] Ohio exhibits substantial heterogeneity across counties in economic structure and labor market conditions. Differences in industrial composition, urbanization, and exposure to sector-specific shocks generate meaningful variation in local economic experiences. This cross-sectional variation, combined with a common institutional framework, provides a setting in which differences in registration behavior can be linked to localized economic shocks.

[16]Detailed information on registration deadlines and election timing is provided in Appendix D.

CHAPTER THREE

DATA AND EMPIRICAL STRATEGY

3.1 Outcome Data

To measure political participation using voter registration activity, which captures entry into and maintenance of engagement with the electorate, detailed administrative voter data has been collected from the state of Ohio, covering the period from 2000 to 2024.[17] These data are drawn from individual-level voter files and aggregated to the county-month level, forming the core panel structure used throughout the empirical analysis. This aggregation provides a balanced county-by-month unit of observation that aligns naturally with the timing of economic shocks and allows for high-frequency analysis of participation behavior.

The primary outcome is defined as registrations per 1,000 voting-age population (VAP),[18] which facilitates comparison across counties and over time by scaling participation relative to the eligible population. This normalization is essential for comparability across counties of different sizes and over time, as it scales participation relative to the eligible population. As discussed in the background section, registration reflects an upstream margin of participation that precedes turnout and incorporates both new entrants and individuals updating or maintaining their eligibility. Registrations per 1,000 captures broad participation responses, including both new entrants into the electorate and individuals re-engaging with the registration system.

In addition to total registrations, the analysis considers an analysis of first-time

[17]Ohio voter registration data are obtained from the Ohio Secretary of State voter file repository, which provides regularly updated individual-level registration records through the 2024 election cycle (Statewide Voter Files Download Page, n.d.)

[18]Voting-age population (VAP) is defined as the number of individuals aged 18 and older within each county. County-level population estimates are constructed using data from the Surveillance, Epidemiology, and End Results (SEER, 2025) Program, which provides annual demographic population estimates by age and geography.

registrations, defined as individuals who could not have participated in prior elections due to age. This restricted group only counts registration that occurred when the individual would have been ineligible to vote because of age at the time of the last general election[19]. This group represents a subset of first-time registrants with minimal prior political exposure, reducing concerns related to habit persistence or re-engagement. Youth registrations therefore provide a cleaner measure of initial political entry, though they may also be influenced by life-cycle factors such as aging into eligibility.

Because the analysis is designed to isolate localized economic shocks from broad national downturns, the sample is organized into inter-recession windows.[20] These windows separate periods of national expansion from periods in which recessionary conditions are widespread, allowing the descriptive statistics to be presented in the same structure used in the main empirical analysis. A detailed description of the construction of the windows follow in the empirical strategy. Table 1 reports descriptive statistics for the main analysis samples across these two inter-recession windows. Average registration rates increase substantially between the two periods, doubling from approximately 1 registration per 1,000 people of voting age in Window 1 to 2. When we restrict the highest propensity registration months we see relatively equivalent falls in registration amounts. A similar pattern holds for first-time registrations, which increase from about 2 to 3.5 registrations out of every 10,000 VAP across windows. This shift suggests a higher overall level of electoral engagement in the later period.

[19]Throughout the analysis, "general elections" refers specifically to November general elections in the United States electoral calendar.

[20]National recession dates are defined according to the National Bureau of Economic Research Business Cycle Dating Committee (US Business Cycle Expansions and Contractions, n.d.).

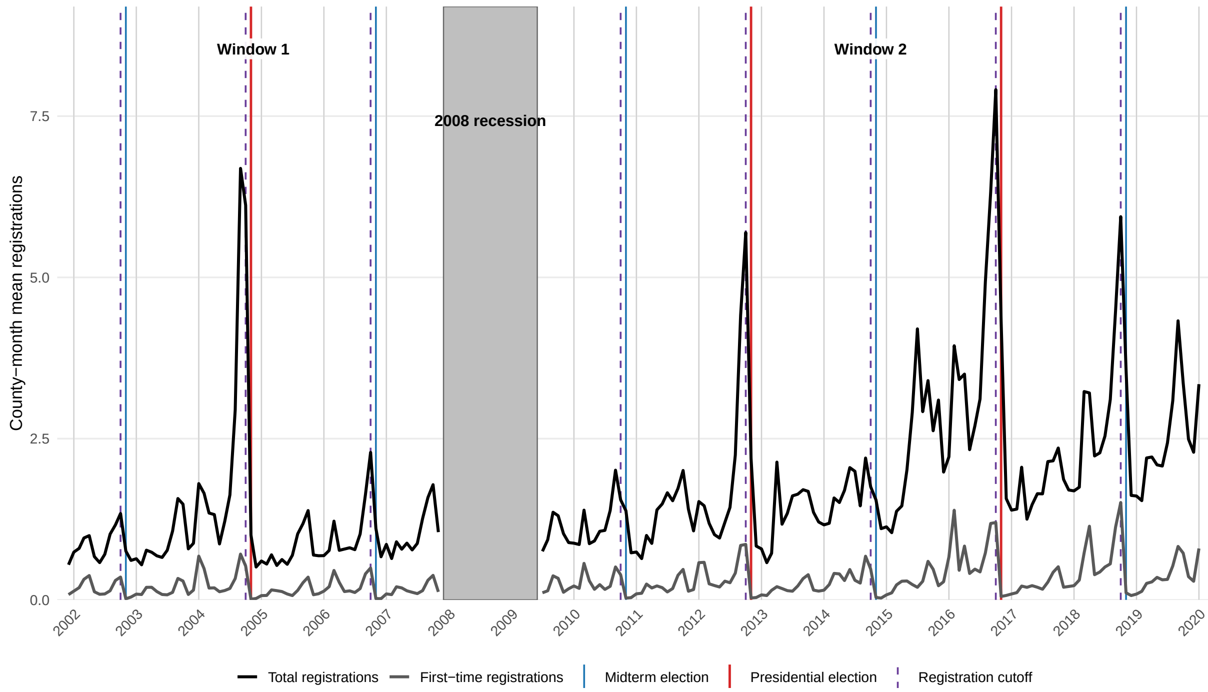
Table 1: Descriptive Statistics for the Main County-Month Analysis Samples

Window	Sample	Variable	Mean	SD
Window 1	Full sample	Registrations	1.136	1.272
		First-time registrations	0.192	0.252
		Unemployment rate	6.397	1.441
		Observations		6,336
Window 1	Restricted sample	Registrations	0.948	0.740
		First-time registrations	0.167	0.227
		Unemployment rate	6.402	1.438
		Observations		5,808
Window 2	Full sample	Registrations	2.044	1.550
		First-time registrations	0.344	0.399
		Unemployment rate	7.089	2.847
		Observations		11,176
Window 2	Restricted sample	Registrations	1.857	1.278
		First-time registrations	0.298	0.342
		Unemployment rate	7.099	2.862
		Observations		10,296

Notes: This table reports descriptive statistics for the county-month samples used in the main analysis. Registration outcomes are measured per 1,000 voting-age population. The unemployment rate is the county-level U3 unemployment rate. Standard deviations are reported in parentheses. Observations refer to county-months. There are 88 Ohio counties in the balanced panel. Restricted samples exclude September and October in election years to remove high-registration months associated with general-election timing.

Figure 1 displays the time series of registrations, along with indicators for election timing and recession periods. Registration activity exhibits pronounced and recurring spikes around election cycles, particularly in the months immediately preceding major elections. These spikes are consistent across both windows and are especially concentrated in September and October, motivating the use of restricted samples that exclude these months in certain specifications. Outside of election periods, registration rates are relatively stable but display gradual shifts in level across windows, consistent with the differences observed in Table 1.

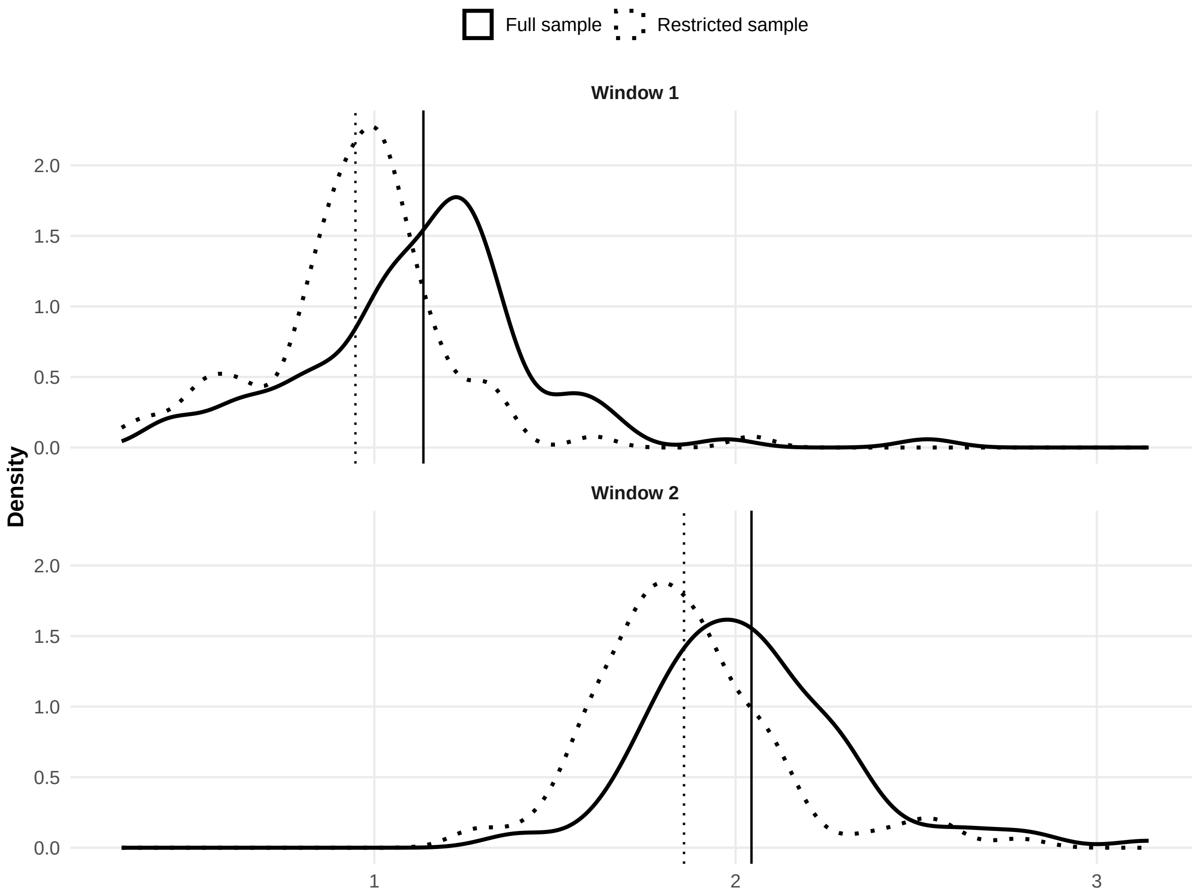
Figure 1: Monthly Mean Registration Rates across the Inter-Recession Windows



Notes: This figure plots the county-month mean of total registrations and first-time registrations across the two inter-recession windows. Registration outcomes are measured per 1,000 voting-age population. Vertical lines denote November general elections, distinguishing presidential and midterm election years. Dashed lines denote the registration cutoff month, defined here as one month before the November general election. The shaded region marks the omitted recession period separating the two analysis windows.

Substantial heterogeneity is also evident across counties. Figure 2 presents the cross-county distribution of average monthly registration rates for each window. In both periods, the distribution is dispersed, indicating meaningful differences in baseline participation levels across counties. However, the distribution shifts noticeably to the right in Window 2, reflecting higher average registration rates across most counties. The comparison between the full and restricted samples further shows that removing high-registration election months compresses the distribution and reduces variance, though the overall pattern of cross-county heterogeneity remains intact.

Figure 2: Cross-County Distribution of Average Monthly Registration Rates



Notes: This figure shows the cross-county distribution of average monthly registration rates separately by inter-recession window and sample definition. Registration rates are measured per 1,000 voting-age population. Full samples include all months in the relevant window. Restricted samples exclude September and October in election years. Vertical lines mark the corresponding sample means.

Taken together, these descriptive patterns highlight three key features of the data. First, registration activity is strongly influenced by election timing, generating sharp but predictable spikes. Second, there is substantial and persistent heterogeneity across counties, making normalization by population necessary for meaningful interpretation of registration rates. Third, within-county variation over time provides the primary source of identifying variation, as economic shocks occur at different times across locations. These features motivate the panel data framework used in this empirical analysis.

3.2 Empirical Strategy

A central feature of the empirical design is the restriction of the sample to inter-recession windows defined by NBER business cycle dates. This is done to isolate localized labor market shocks from periods of nationwide economic contraction. During national recessions, macroeconomic conditions deteriorate broadly across counties, limiting meaningful cross-sectional variation in treatment timing. By focusing on expansion periods, the analysis exploits differences in the incidence and timing of local economic shocks while holding fixed the broader national economic environment through time fixed effects. This design ensures that identifying variation is driven primarily by cross-county differences in local labor market conditions rather than aggregate downturns. To estimate the causal effect of local economic shocks on voter registration behavior, the analysis employs a panel difference-in-differences framework with two-way fixed effects (TWFE). The baseline specification is given by:

$$Y_{c,t} = \beta \cdot Shock_{c,t} + \alpha_c + \gamma_t + \varepsilon_{c,t}$$

where $Y_{c,t}$ denotes voter registration outcomes in county c during year–month t.[21] The terms α_c and γ_t represent county and time fixed effects, respectively. $\varepsilon_{c,t}$ is an idiosyncratic error term capturing unobserved determinants of voter registration that vary across counties and over time. The variable $Shock_{c,t}$ captures the onset of local economic distress, measured using WARN layoff announcements and Sahm-rule unemployment triggers

[21] t indexes year–month observations (e.g., January 2005), and the associated time fixed effects capture shocks common to all counties at that level. This differs from specifications that include month-of-year indicators as seasonal controls, which would capture recurring calendar effects rather than common time shocks.

as binary indications of treatment, alongside the continuous unemployment rate per county. β captures the average within-county change in registration outcomes associated with the onset of local economic shocks, relative to counties that have not yet been treated.

The analysis focuses on three primary measures of local economic conditions: Sahm rule triggers, WARN events,[22] and county-level unemployment rates. Sahm and WARN measures are implemented as binary indicators,[23] capturing discrete shocks to local economic conditions, while the unemployment rate provides a continuous and more granular measure of underlying labor market conditions. Each measure captures a different dimension of economic stress and maps differently to the theoretical framework outlined earlier. Identification in this framework comes from comparing within-county changes in registration behavior over time relative to counties that have not yet experienced a shock in the same period.

The TWFE specification relies on several identifying assumptions. First, the parallel trends assumption requires that, in the absence of treatment, treated and untreated counties would have experienced parallel changes in voter registration over time. This does not require outcomes to be at the same level, but rather that they would have evolved with comparable underlying trends. In the context of the inter-recession windows, this implies that, absent local economic shocks, counties would have followed analogous recovery trajectories in registration behavior. Second, the model assumes no systematic anticipatory responses to treatment. Third, treatment timing must be conditionally exogenous, such that the onset of local economic shocks is not driven by unobserved factors that also affect registration outcomes. Finally, the specification assumes no interference across units, implying that shocks in one county do not directly affect outcomes in another.[24] While these assumptions

[22]WARN indicators are primarily binary, but continuous measures of mass layoff activity, including counts of notices and the number of affected workers, are addressed in chapter 6.

[23]Local economic shocks are constructed using WARN layoff announcements and Sahm-rule unemployment triggers; detailed construction of these measures is provided in Section 3.3

[24]The no-interference assumption—also known as the Stable unit treatment value assumption

are standard, they are not directly testable and may be violated in the present setting. Accordingly, the empirical strategy incorporates a series of diagnostic and robustness exercises, most notably event study analyses and alternative estimators, to evaluate the plausibility of these assumptions and to assess the stability of the results under weaker identifying conditions.

The timing of treatment reflects the arrival of information about local economic conditions. WARN notices represent advance announcements of mass layoffs, providing workers with information about impending job loss prior to separation. As a result, behavioral responses may occur either at the time of announcement or with a delay as economic conditions are realized. In contrast, Sahm-rule triggers are constructed from realized unemployment dynamics and are not forward-looking. This distinction motivates a flexible empirical approach that allows for delayed responses to treatment while placing less emphasis on anticipatory behavior.

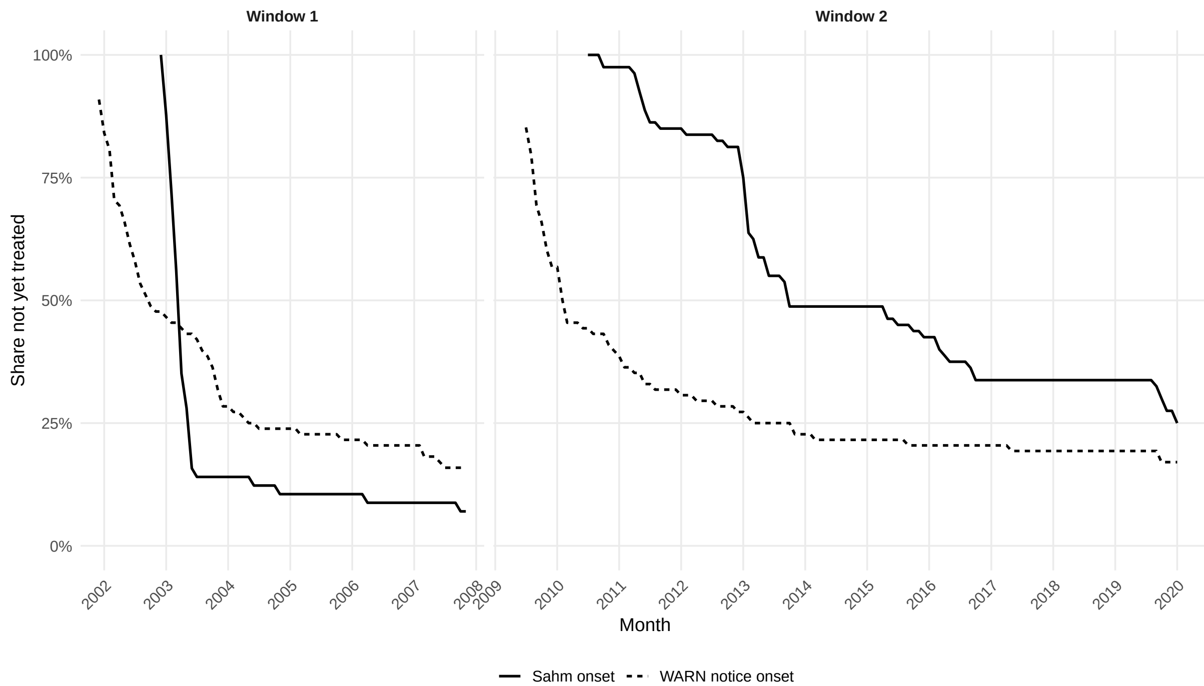
A key limitation of the TWFE framework in this context arises from the staggered timing of treatment. Counties experience economic shocks at different points in time, and once a county is treated, it remains exposed for the remainder of the window. When treatment effects are heterogeneous across cohorts or evolve dynamically over time, the TWFE estimator combines comparisons between treated and not-yet-treated units with comparisons between earlier- and later-treated units. This can potentially generate bias in the estimated treatment effect, particularly if the impact of economic shocks changes over time.[25] In this setting, such concerns are especially relevant, as the effects of local economic distress may attenuate or evolve following the initial shock. This feature of the data is

(SUTVA)—may be violated if labor markets span county boundaries, particularly in the case of WARN notices where workers may reside in different counties than their place of employment. This issue is discussed further in the interpretation section.

[25]Recent work demonstrates that in staggered treatment settings with heterogeneous treatment effects, two-way fixed effects estimators combine multiple treatment comparisons with non-convex weights, including comparisons between already-treated and newly treated units, which can lead to biased estimates (Goodman-Bacon, 2021; Sun & Abraham, 2021).

illustrated in Figure 3, which reports the share of counties that have not yet experienced treatment within each inter-recession window. The figure shows a steady decline in the untreated share over time, indicating that the pool of valid comparison units becomes progressively smaller as the window evolves. Early in each window, a large fraction of counties remains untreated, allowing for comparisons between treated and not-yet-treated units. In contrast, later periods rely increasingly on comparisons across already-treated counties, which can introduce bias in the presence of heterogeneous treatment effects. This pattern highlights why identification is strongest in earlier periods and motivates the use of estimators that explicitly restrict comparisons to not-yet-treated units. This also implies that event-study estimates at longer horizons rely on a shrinking and compositionally changing set of comparison units, a consideration that is important when interpreting the dynamic estimates reported in Figures 4 and 5.

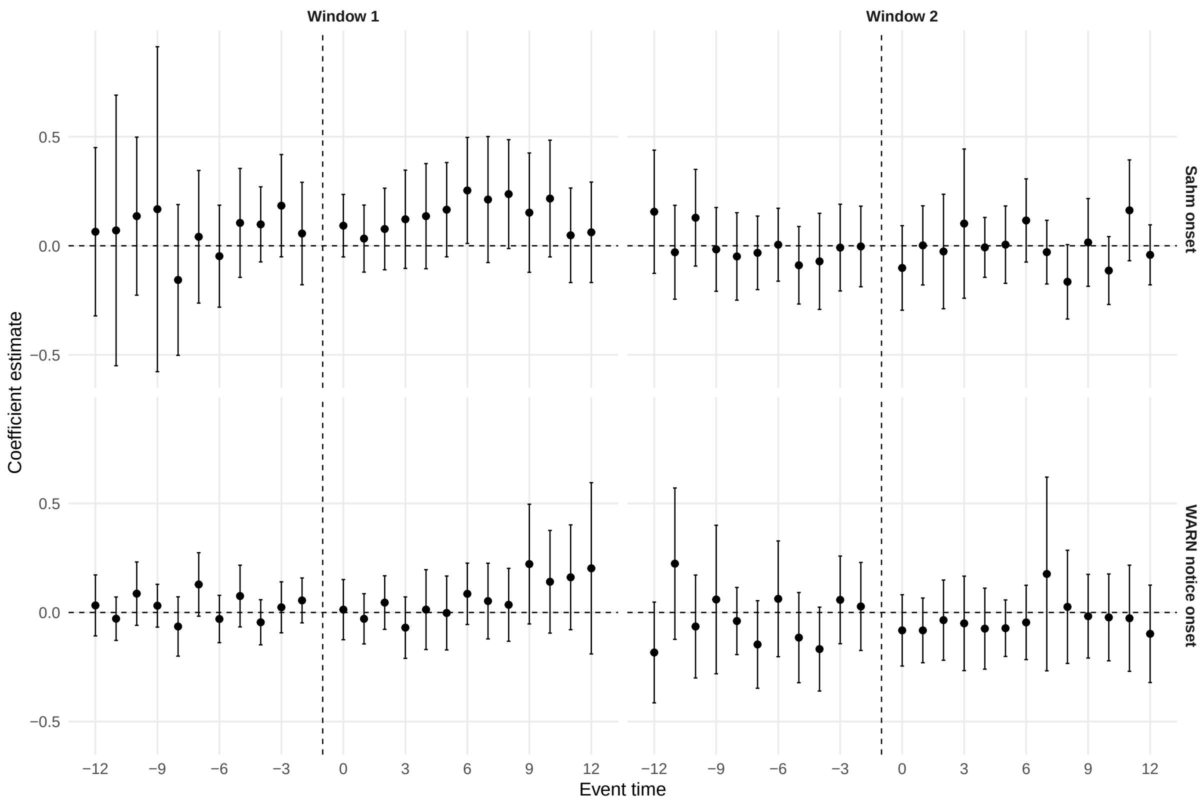
Figure 3: Share of Counties Not Yet Treated over Time



Notes: This figure reports the share of counties that have not yet experienced treatment in each month of the analysis windows. Sahm panels use the Sahm-trimmed estimation windows, while WARN panels use the full inter-recession windows. Declining values indicate shrinking untreated comparison groups over time. This pattern illustrates why identification in staggered-adoption designs is strongest earlier in each window and weakens later as more counties become treated.

To assess the validity of the identifying assumptions and to characterize the dynamic response of voter registration to economic shocks, the analysis extends the baseline specification to an event study framework. This approach replaces the single treatment indicator with a series of leads and lags relative to the timing of treatment, allowing outcomes to be traced before and after the onset of economic shocks. Figure 4 presents the event study estimates from the two-way fixed effects specification. The pre-treatment coefficients are generally centered around zero, providing suggestive evidence in support of the parallel trends assumption and indicating limited evidence of systematic anticipatory behavior. Post-treatment coefficients exhibit a pattern characterized by relatively muted immediate effects followed by somewhat larger estimates at positive event times. However, these estimates are not statistically distinguishable from zero and should be interpreted as suggestive rather than causal evidence of dynamic responses.

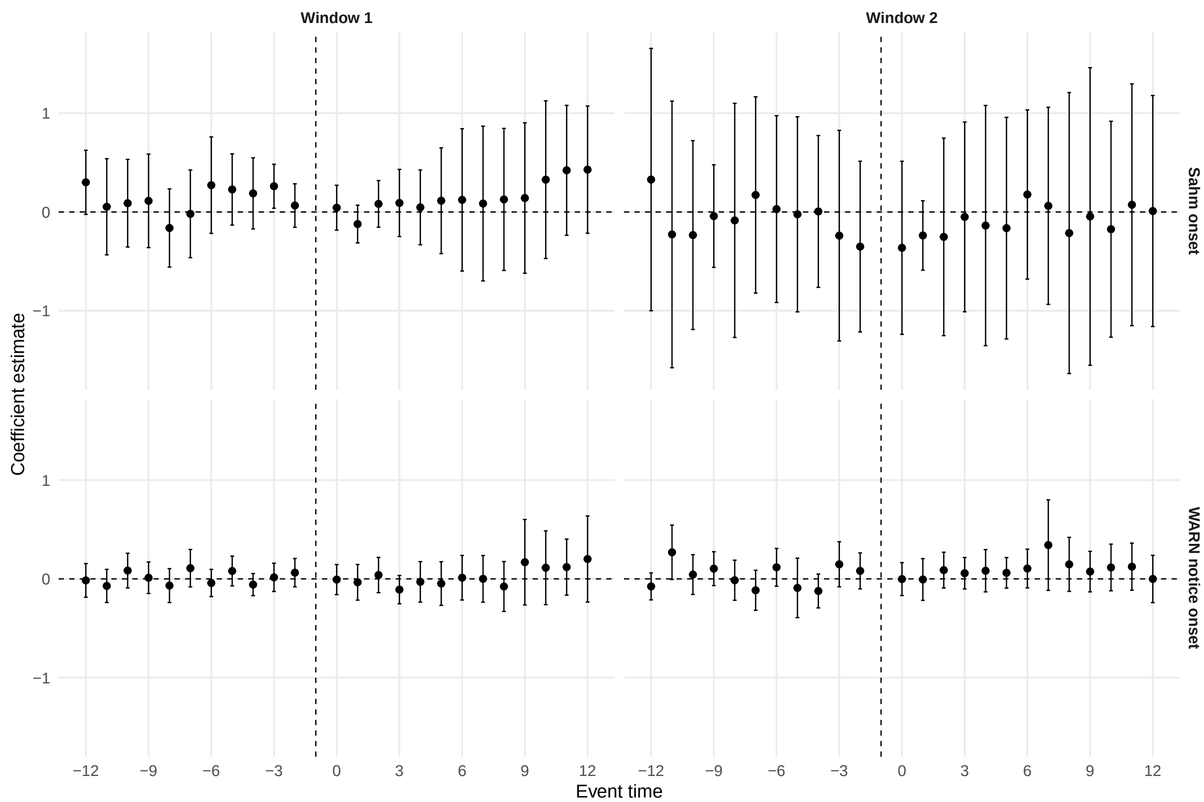
Figure 4: TWFE Event-Study Estimates of Registration Effects



Notes: This figure reports dynamic treatment-effect estimates from two-way fixed-effects event-study specifications. The outcome is registrations per 1,000 voting-age population. Event time is measured in months relative to treatment onset, with month -1 omitted as the reference period. The plotted estimates include 95 percent confidence intervals based on county-clustered standard errors. Sahm panels use the Sahm-trimmed estimation windows, while WARN panels use the full inter-recession windows.

However, event study estimates obtained within the TWFE framework do not fully resolve the identification concerns associated with staggered treatment timing. When treatment effects are heterogeneous, these estimates may still suffer from the same weighting issues as the baseline specification. To address this concern, the analysis implements the estimator proposed by Sun and Abraham (2021), which is specifically designed for settings with staggered treatment adoption. This approach constructs cohort-specific treatment effects and aggregates them using appropriate weights, ensuring that treated units are compared only to units that have not yet been treated at the same point in time. By avoiding comparisons across already-treated units, this estimator produces consistent estimates in the presence of heterogeneous and dynamic treatment effects. Figure 5 reports the corresponding event-study coefficients using the Sun and Abraham estimator. By restricting comparisons to not-yet-treated units, this approach eliminates the contamination arising from comparisons across already-treated cohorts. As a result, these estimates provide a more credible characterization of dynamic treatment effects under staggered adoption, particularly in later periods where Figure 3 shows that the set of valid comparison units becomes limited. As with the TWFE estimates, the coefficients are not statistically distinguishable from zero across event time. While the pattern of estimates is broadly consistent with the dynamics observed in the TWFE specification, the Sun and Abraham estimates provide the preferred evidence on dynamic treatment effects under staggered adoption.

Figure 5: Sun and Abraham Event-Study Estimates of Registration Effects



Notes: This figure reports dynamic treatment-effect estimates from Sun and Abraham event-study specifications. The outcome is registrations per 1,000 voting-age population. Event time is measured in months relative to treatment onset, with month -1 used as the reference period. The plotted estimates include 95 percent confidence intervals based on county-clustered standard errors. Sahm panels use the Sahm-trimmed estimation windows, while WARN panels use the full inter-recession windows.

It is important to note that moving from two-way fixed effects to event study to Sun and Abraham is not an apples-to-apples comparison, as each estimator answers a slightly different question and involves distinct tradeoffs. The TWFE model provides a simple benchmark but may be biased under staggered treatment timing and heterogeneous effects. The event study framework adds transparency by illustrating dynamic responses and enabling diagnostic checks of identifying assumptions but still inherits the limitations of the TWFE estimator. The Sun and Abraham approach provides the most credible causal estimates under staggered adoption, though at the cost of reduced precision and a smaller effective comparison set. In this sense, the progression across methods should be understood as a sequence of validation exercises: TWFE establishes a baseline, event studies assess identifying assumptions and dynamics, and the Sun and Abraham estimator provides the preferred causal estimates.

3.3 Treatment Variables

As described in the previous section, the analysis incorporates three primary measures of economic distress: Sahm-rule triggers, WARN-based indicators, and the county-level unemployment rate. Each measure is constructed at the county–month level and is designed to capture a distinct dimension of local labor market conditions, corresponding to different interpretations of the estimated treatment effects.

The Sahm-rule trigger is implemented as a binary indicator equal to one when a county experiences a clear and sustained worsening in labor market conditions. The rule identifies this point by comparing recent unemployment levels to the county's recent low: specifically, it activates when the three-month average unemployment rate rises by at least 0.5 percentage points above its lowest value in the past year. In this way, the measure captures when unemployment has not only increased, but increased enough, and for long enough, to signal a meaningful downturn rather than normal month-to-month variation. For the purposes of the analysis, once a county meets this condition, it is treated as having experienced an economic shock and remains classified as treated for the remainder of the inter-recession window. In other words, the first time the Sahm rule is satisfied marks the onset of a downturn in that county, and all subsequent months in the window are considered part of the post-shock period. This construction allows the Sahm indicator to capture the timing of when a county first enters a period of worsening labor market conditions. The estimated coefficient can therefore be interpreted as the average change in voter registration following the onset of this local downturn, rather than a response to short-term fluctuations in unemployment.[26]

An important implementation issue arises from the behavior of the Sahm rule following national recessions. Because the trigger depends on sustained increases in unemployment, many counties remain in a triggered state even after the official recovery date used to define the inter-recession windows. As a result, at the beginning of each window, a substantial share of counties may already be classified as treated. This feature is illustrated in Figure 6, which displays the distribution of Sahm triggers across counties for the second inter-recession window. Many counties appear in a triggered state immediately following the end of the preceding recession, reflecting lingering labor market weakness rather than new localized shocks.

[26]Alternative specifications to limit the duration of the treatment period yield similar conclusions and do not change the interpretation of the results. See chapter 6.

Figure 6: County Sahm Trigger Timing, Window 2

Notes: This heatmap displays the monthly timing of county-level Sahm-rule triggers in Window 2. A highlighted cell indicates that a county-month satisfies the Sahm trigger definition. Counties already triggered at the selected trimmed start month are shown with reduced opacity. The vertical line marks the start month chosen for the Sahm-trimmed analysis window. Counties are displayed in FIPS order.

To address this issue, the start of each estimation window is defined endogenously based on the evolution of the treatment distribution. Within the first twelve months of each inter-recession period, the month with the lowest share of triggered counties is identified. The estimation window is then redefined to begin at this point, and counties that remain in a triggered state at the new start date are excluded from the analysis. This can be thought of as adjusting the start date of our window to more accurately reflect when counties have stopped feeling the impacts of a national recession, which ensures that the sample begins with a set of counties that are plausibly untreated and therefore capable of serving as valid comparison units. Following this adjustment, treatment is defined relative to the first within-window onset of the Sahm trigger for each county, and the absorbing structure ensures that counties remain treated for the remainder of the window. Without this adjustment, early-period comparisons would rely heavily on counties already experiencing persistent economic distress, limiting the ability to interpret estimated effects as responses to newly realized shocks.

WARN-based measures capture discrete job displacement events using data on mass layoff announcements filed under the Worker Adjustment and Retraining Notification (WARN) Act. Under this federal requirement, large employers must provide advance notice—specifically 60 days—before implementing mass layoffs or plant closures. These notices are collected and published by state agencies, providing a high-frequency record of impending job loss at specific locations. Using state-level WARN filings, which are aggregated to the county–month level based on the reported location of each event, the sample is restricted to the study period and to observations that can be reliably assigned to a county. From this, a binary indicator is constructed equal to one in county-months where at least one WARN notice is issued. This indicator captures the arrival of new information about impending job loss and represents a salient, localized economic shock that is both observable and temporally precise.[27] In specifications that focus on treatment timing, WARN exposure is implemented as an absorbing indicator based on the first observed layoff announcement within each inter-recession window. That is, once a county experiences a WARN event, it is treated as having been exposed to a negative labor market shock for the remainder of the window. This ensures consistency with the event-study framework and allows for

[27] Alternative specifications using WARN counts and measures of affected workers relative to the voting-age population are explored as robustness checks but are not included in the baseline results.

comparisons across counties based on the timing of initial exposure.

The county-level unemployment rate enters the analysis as a continuous measure of local labor market conditions. Unlike the binary treatment indicators, the unemployment rate captures incremental variation in economic distress over time. This allows the specification to account for gradual changes in labor market conditions that may not be captured by discrete triggers. In this context, the estimated coefficient on the unemployment rate reflects the marginal effect of changes in local labor market conditions on voter registration, holding fixed county and time effects. This measure is not expected to have a large independent effect on political participation, as modest month-to-month changes in unemployment are unlikely to constitute a salient economic shock. Instead, it serves as a proxy for underlying local economic conditions, helping to isolate the effects of discrete shocks captured by the Sahm and WARN indicators from broader fluctuations in the labor market.

Taken together, these treatment variables allow for a comparison between diffuse and acute forms of economic distress. The Sahm trigger captures sustained and broad-based labor market deterioration, while WARN indicators reflect discrete and localized job loss events. The unemployment rate provides a continuous measure of underlying labor market conditions, capturing incremental variation between these extremes. As a result, estimated effects should be interpreted in the context of the underlying shock type: coefficients on Sahm and WARN indicators reflect responses to the onset of discrete economic shocks, while coefficients on the unemployment rate reflect responses to gradual changes in local economic conditions.

CHAPTER FOUR

RESULTS

The empirical results indicate that local economic shocks do not produce a statistically detectable effect on voter registration behavior. Across specifications and outcome measures, estimated effects are small in magnitude, and the corresponding 95% confidence intervals are centered around zero, providing no evidence of a systematic causal relationship between local economic distress and registration activity.

Table 2: Main Results for Total Registrations

	Sahm	**WARN**	**U3**
Window 1			
Coefficient	0.053	0.001	-0.003
Std. error	(0.122)	(0.041)	(0.028)
95% CI	[-0.192, 0.297]	[-0.081, 0.082]	[-0.059, 0.052]
Observations	3,420	6,336	6,336
Window 2			
Coefficient	-0.092	0.013	0.008
Std. error	(0.063)	(0.096)	(0.026)
95% CI	[-0.219, 0.034]	[-0.178, 0.203]	[-0.043, 0.059]
Observations	9,200	11,176	11,176

Notes: The outcome is total registrations per 1,000 voting-age population. All models include county and month fixed effects. Standard errors are clustered by county. Sahm models use trimmed inter-recession windows. WARN and unemployment-rate models use full inter-recession windows. 95% confidence intervals are calculated as estimate $\pm$ 1.96 $\times$ standard error. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

For total registrations, the estimated effects of economic shocks remain close to zero across both windows. In Window 1, the Sahm trigger corresponds to an increase of approximately 5 registrations per 100,000 individuals, while the WARN indicator and unemployment rate imply changes that are effectively negligible in magnitude. In Window 2, the Sahm estimate shifts to a decrease of roughly 9 registrations per 100,000 people, while WARN and unemployment again produce very small positive effects. Although the point estimates vary across specifications and windows, all implied changes remain in the order of single-digit registrations per 100,000 individuals. The standard errors are large relative to these point estimates. In most cases, the estimated coefficients are small compared to the surrounding uncertainty, indicating that the data are consistent with both modest positive and modest negative effects. This lack of precision is reflected in the instability of the estimates across windows, particularly for the Sahm measure, where the sign reverses despite similar magnitudes. The 95% confidence intervals further indicate that all estimates include zero and are centered in a narrow range around it, ruling out economically large effects in either direction. Such variation suggests that the observed differences are not indicative of a stable underlying relationship but rather reflect noise in the estimation. This interpretation is reinforced by the event study results presented previously, where coefficients fluctuate around zero with no clear post-treatment pattern and no evidence of anticipatory effects.

Table 3: Main Results for First-Time Registrations

	Sahm	**WARN**	**U3**
Window 1			
Coefficient	0.013	0.002	0.009**
Std. error	(0.017)	(0.008)	(0.004)
95% CI	[-0.021, 0.047]	[-0.014, 0.017]	[0.001, 0.018]
Observations	3,420	6,336	6,336
Window 2			
Coefficient	-0.010	-0.003	0.009**
Std. error	(0.012)	(0.013)	(0.004)
95% CI	[-0.033, 0.013]	[-0.028, 0.022]	[0.002, 0.017]
Observations	9,200	11,176	11,176

Notes: The outcome is first-time registrations per 1,000 voting-age population. All models include county and month fixed effects. Standard errors are clustered by county. Sahm models use trimmed inter-recession windows. WARN and unemployment-rate models use full inter-recession windows. 95% confidence intervals are calculated as estimate $\pm$ 1.96 $\times$ standard error. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

A similar pattern emerges for first-time registrations. In Window 1, the estimated effects imply increases of approximately 1–2 registrations per 100,000 individuals for the discrete shock measures, while the unemployment rate produces an effect of just under 1 additional registration per 100,000 people. In Window 2, these estimates either shrink toward zero or reverse sign, with all effects again remaining extremely small in absolute terms. Across both windows, the magnitude of the estimated effects is consistently limited, with most coefficients implying changes well below even modest shifts in registration activity. As with total registrations, the standard errors are large relative to the size of the coefficients for most specifications, limiting the precision with which these effects can be estimated. Consistent with this, the 95% confidence intervals generally include zero for the discrete shock measures and remain tightly bounded, indicating that any true effects are small in magnitude. The combination of small coefficients and comparatively large uncertainty implies that the data do not support a clear directional relationship between economic shocks and first-time registration behavior. This conclusion aligns with the event study evidence, where estimated dynamic effects remain centered around zero and show no consistent pattern following the onset of economic shocks.

Taken together, the results across both outcome measures present a consistent pattern. Estimated effects are small in magnitude, imprecisely estimated, and sensitive to specification and sample window. The relative size of the standard errors compared to the coefficients indicates that the estimates are not distinguishable from zero in a meaningful sense, and the occasional sign reversals further suggest a lack of stability in the relationship. The event study analysis corroborates this interpretation, showing no clear dynamic response of registration behavior to economic shocks. Overall, the evidence indicates that local economic conditions are not associated with meaningful changes in voter registration activity within this setting.

CHAPTER FIVE

INTERPRETATION

The empirical results indicate that local economic shocks have no consistent effect on voter registration behavior. Across specifications, outcome measures, and identification strategies, estimated effects are small and not distinguishable from zero. This raises a central question: why do economic shocks appear to have limited impact on registration, despite strong theoretical predictions in both directions?

A natural starting point is the competing forces of withdrawal and mobilization. The theoretical framework suggests that economic shocks can simultaneously reduce participation through resource constraints while increasing participation through grievance-driven engagement. However, the structure of voter registration implies that these forces should not be symmetric. Registration is a costly, forward-looking administrative act that must be completed prior to participation. As such, it is more likely to be sensitive to constraints than to short-run increases in political motivation. Individuals facing economic hardship must allocate time and resources toward immediate concerns, raising the opportunity cost of completing registration. Under this framework, one would expect the withdrawal effect to dominate, producing a negative relationship between economic distress and registration.

The absence of such a directional effect in the data is therefore informative. Rather than observing a clear decline in registration following economic shocks, the estimates fluctuate around zero, with no consistent sign across specifications or windows. One possible explanation is that withdrawal and mobilization effects are offsetting at the aggregate level, with some individuals disengaging while others become more politically active. However, given the cost structure of registration, this explanation would require mobilization responses that are large enough to counteract the withdrawal. The lack of a clear negative effect therefore suggests that neither mechanism translates strongly into registration behavior, which suggest that registration is not a particularly responsive or salient margin of political participation.

This interpretation is reinforced by the temporal and institutional features of registration. Unlike voting, which occurs at a well-defined and highly salient moment, registration decisions are diffuse and often disconnected from immediate political stimuli. Individuals must act in advance of elections, frequently outside periods of peak political engagement, and may not perceive registration as an urgent or necessary response to changing economic conditions. These institutional frictions weaken the link between short-run shocks and observable behavior, even when underlying political attitudes are shifting. A related factor is the mismatch between the timing of economic shocks and the electoral cycle. Economic conditions evolve continuously, while elections—and the incentives to engage—occur at discrete intervals. When shocks occur outside of politically salient periods, individuals may delay or ignore the registration decision altogether. This timing disconnect further attenuates any potential response, particularly in high-frequency data where shocks and opportunities for action are not aligned.

The event study evidence is consistent with this interpretation. Estimated coefficients remain centered around zero both before and after treatment, with no clear post-shock adjustment or anticipatory behavior. Combined with the relatively large standard errors observed in the regression results, this pattern suggests that the data do not contain a meaningful signal linking economic shocks to registration behavior. Instead, the observed variation appears largely driven by noise rather than a systematic underlying relationship. It is important to acknowledge that aggregate null results may mask heterogeneous responses. Different individuals may respond to economic shocks in opposing ways, with some withdrawing from participation and others becoming more politically engaged. However, even if such offsetting behavior is present, the lack of a dominant withdrawal effect—despite the cost structure of registration—suggests that these responses do not translate strongly into registration decisions at the aggregate level.

More broadly, these findings align with the distinction between political preferences and participation decisions. A large body of research demonstrates that economic conditions influence how individuals evaluate incumbents and choose between candidates. However, the decision to participate—particularly at the registration stage—is governed by additional constraints, including institutional barriers, habit formation, and the costs of administrative action.[28] In this context, economic shocks may meaningfully affect political attitudes without producing observable changes in registration behavior.

Taken together, the evidence points toward a consistent conclusion. While economic theory predicts that participation should respond to changes in economic conditions, the margin at which participation is measured is critical. In the case of voter registration, the combination of administrative costs, institutional timing, and limited salience appears to dampen responsiveness to economic shocks. As a result, registration does not provide a sensitive measure of how economic conditions influence political engagement, even when underlying preferences may be changing.

[28]The distinction between political preferences and participation decisions is well established. Economic conditions have been shown to influence vote choice and incumbent evaluation (e.g., Kramer, 1971; Bloom and Price, 1975). In contrast, participation is shaped by institutional and resource constraints (Wolfinger and Rosenstone, 1980; Rosenstone, 1982). This would include registration requirements and administrative costs. As a result, changes in political attitudes do not necessarily translate into changes in participation behavior.

CHAPTER SIX

SENSITIVITY AND ROBUSTNESS

This chapter evaluates whether the preceding findings are sensitive to alternative specifications, dependent on the way periods of analysis are defined, or plausibly driven by remaining threats to identification. The core result of the paper is that local economic shocks do not produce a consistent or robust change in voter registration. That conclusion is strongest if it survives reasonable changes in how economic distress is measured, how political time is structured, and how potential confounds are interpreted. The purpose of this chapter is therefore not to search for a preferred alternative specification, but to show that the main interpretation does not hinge on a narrow modeling choice.

6.1 Sensitivity to Alternative WARN Specifications

The baseline WARN specification treats exposure as an absorbing binary indicator: once a county experiences a WARN event within the relevant window, it is considered treated for the remainder of that period. That approach is appropriate if the announcement of a major layoff is best understood as a salient local shock whose political effects may persist beyond the initial month. At the same time, WARN data can be operationalized in other plausible ways. Rather than emphasizing the presence of a shock, one may instead emphasize the number of WARN notices filed or the scale of disruption relative to county population. Table 4 reports results using all three approaches.

Table 4: Alternative WARN Specifications for Total Registrations

Specification	**Window 1**	**Window 2**
Binary	0.001	0.013
	(0.041)	(0.096)
Count	0.001	-0.039
	(0.027)	(0.036)
Intensity	0.001	-0.002
	(0.007)	(0.002)

Notes: The outcome is total registrations per 1,000 voting-age population. Entries are coefficients from county and month fixed-effects regressions, with county-clustered standard errors in parentheses. The binary specification uses the absorbing WARN treatment indicator. The count specification uses the number of WARN notices in the county-month. The intensity specification scales affected workers by voting-age population. Window 1 and Window 2 refer to the two inter-recession analysis windows.

Across both windows, the substantive conclusion remains unchanged. For all registrations, the baseline absorbing WARN indicator produces coefficients very close to zero: 0.001 in Window 1 and 0.013 in Window 2. Replacing the binary measure with WARN count, a measure of how many notices were issued in a county, likewise produces no meaningful effect with estimates of 0.001 in Window 1 and -0.039 in Window 2. The intensity specification, which scales affected workers relative to county voting-age population, yields similarly small estimates of 0.001 in Window 1 and -0.002 in Window 2. None of these results suggests a stable or substantively important relationship between WARN-based economic shocks and the registration margin. The same pattern holds for first-time registrations.[29] The estimates remain small in magnitude and unstable in sign across operationalizations. The absence of a systematic result across these measures is important. If the baseline null finding were merely an artifact of coding WARN as a binary absorbing treatment, one would expect at least one of the alternative measures to reveal a clearer pattern. Instead, the results in Table 4

[29]See Appendix B.

indicate that the broader conclusion is not driven by the particular functional form used to summarize WARN exposure. This stability across measures also informs interpretation. A binary absorbing indicator emphasizes the arrival of a salient political signal, whereas WARN count and WARN intensity place more weight on the frequency or magnitude of local layoff events. The fact that none of these formulations produces a clear effect suggests that the registration response is weak not only to the presence of a layoff announcement, but also to reasonable alternative representations of layoff severity.

6.2 Robustness to Alternative Period Definitions

The main analysis is organized around broad inter-recession windows. That structure is motivated by the desire to isolate local economic variation within national non-recessionary periods while avoiding contamination from major aggregate downturns. Still, one could argue that voter registration is fundamentally tied to electoral timing, and that local economic conditions may matter primarily within more explicitly political periods. To evaluate that possibility, Table 5 reports results under two alternative period definitions: election-reset periods and a pre-election sample.

Table 5: Alternative Period Definitions for Total Registrations

Specification	Estimate	Std. error
Window 1		
Sahm: Baseline	0.053	(0.122)
Sahm: Election reset	0.005	(0.097)
Sahm: Pre-election	-0.574	(0.564)
WARN: Baseline	0.001	(0.041)
WARN: Election reset	0.077*	(0.044)
WARN: Pre-election	0.303	(0.278)
Window 2		
Sahm: Baseline	-0.092	(0.063)
Sahm: Election reset	-0.068*	(0.038)
Sahm: Pre-election	-0.329	(0.244)
WARN: Baseline	0.013	(0.096)
WARN: Election reset	0.033	(0.036)
WARN: Pre-election	0.184	(0.224)

Notes: The outcome is total registrations per 1,000 voting-age population. Entries are county and month fixed-effects coefficients, with county-clustered standard errors in parentheses. Baseline specifications use the main inter-recession windows. Election-reset specifications redefine treatment timing within periods that restart after each November general election and remove counties already treated at the beginning of the period. Pre-election specifications restrict the sample to the months immediately preceding November general elections. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

The first alternative treats the post-election environment as the start of a new political period. Under this design, treatment timing is redefined within periods that restart after each general election[30], and counties already treated at the beginning of a period are removed before constructing the absorbing treatment. This is a stronger assumption than the baseline design. In effect, it assumes that the political relevance of prior shocks does not carry through the election and that a new electoral cycle resets the comparison. That assumption is not preferred as a baseline identifying framework, but it is useful as a robustness exercise

[30] Throughout the analysis, "general elections" refers specifically to November general elections in the United States electoral calendar.

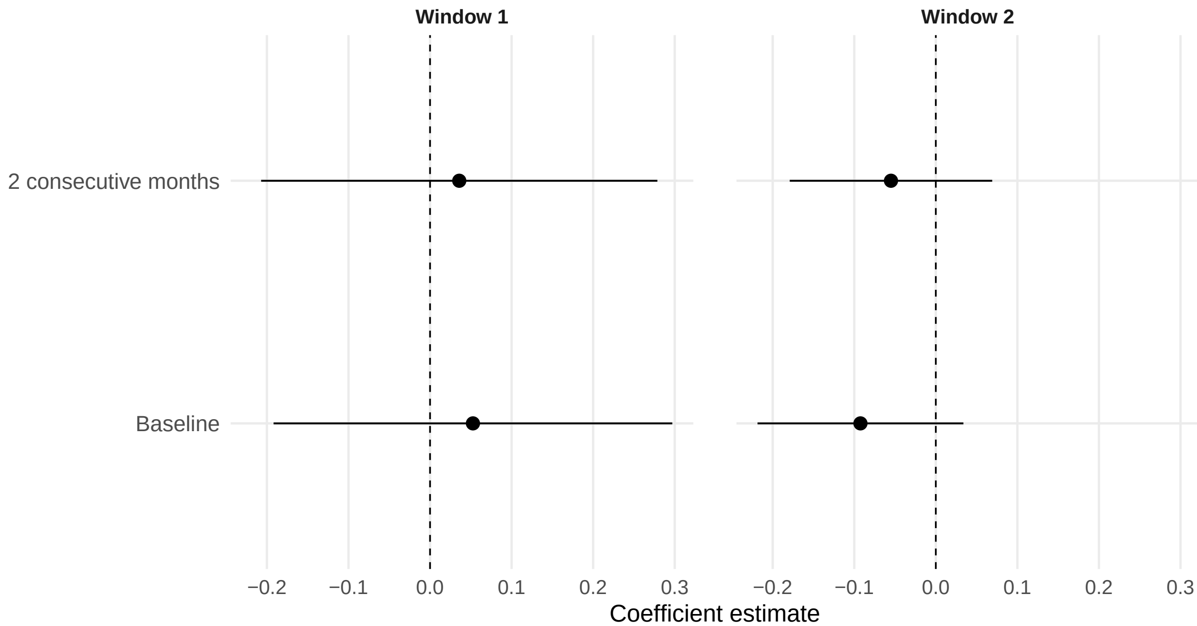
because it asks whether the earlier results are being driven by the long horizon over which treatment is allowed to persist. The second alternative restricts the analysis to the months immediately preceding general elections. This specification focuses on the period in which registration activity is most salient and most likely to matter for actual turnout. The logic is straightforward: if economic shocks affect registration at all, the effect should be most visible when registration decisions are closest to an upcoming election and therefore most politically consequential. The results in Table 5 continue to show little evidence of a durable and consistent registration response.

Taken together, the alternative period definitions reinforce the interpretation that the absence of strong effects is not merely a byproduct of how the sample is partitioned. Narrowing the analysis to explicitly political periods does not materially alter the substantive conclusion. Registration appears largely unresponsive to these forms of local economic distress even when the analysis is centered more tightly on the electoral calendar.

6.3 Additional Specification and Identification Considerations

Several remaining issues are more appropriately handled through discussion, because they concern interpretation of the identifying variation rather than a competing preferred specification. One additional sensitivity concern is whether the SAHM trigger is too permissive when defined off a single month of activation. To address this, I also consider an alternative trigger that requires two consecutive months of SAHM activation before treatment turns on. This stricter definition reduces the chance that the estimated effect is responding to short-lived fluctuations or noise in county-level unemployment dynamics. Requiring persistence makes treatment more conservative and shifts the interpretation of exposure from emerging distress to sustained distress.

Figure 7: Baseline and Two-Month Sahm Trigger Specifications



Notes: This figure compares the baseline Sahm trigger specification with an alternative definition that requires two consecutive months of Sahm activation before treatment begins. The outcome is total registrations per 1,000 voting-age population. Estimates are shown separately by inter-recession window. Vertical intervals represent 95% confidence intervals.

This alternative does not materially alter the interpretation of the results. The main implication is that tightening the trigger does not reveal a previously hidden, strong registration response. Instead, the broad conclusion remains that registration is not consistently responsive to these local labor market shocks.

A more consequential omitted variable concern is the possibility that registration drives coincide with economic downturns. If outside organizations, campaigns, parties, or advocacy groups intensify registration efforts in distressed areas, then registration activity may rise for reasons that are correlated with economic shocks but not caused by them. In that case, observed registration levels would partly reflect mobilization efforts rather than the direct effect of economic distress. The key issue is the direction of any resulting bias. Registration drives are generally organized around election timing and campaign mobilization, not around local unemployment shocks per se.[31] To the extent that such efforts do become more intense in economically distressed places, they would work against a withdrawal mechanism by increasing registrations precisely where economic hardship might otherwise reduce them. That means the bias from omitted registration drives is toward zero. In other words, if countercyclical mobilization exists, it will attenuate any negative relationship between economic shocks and registration rather than mechanically generate one.

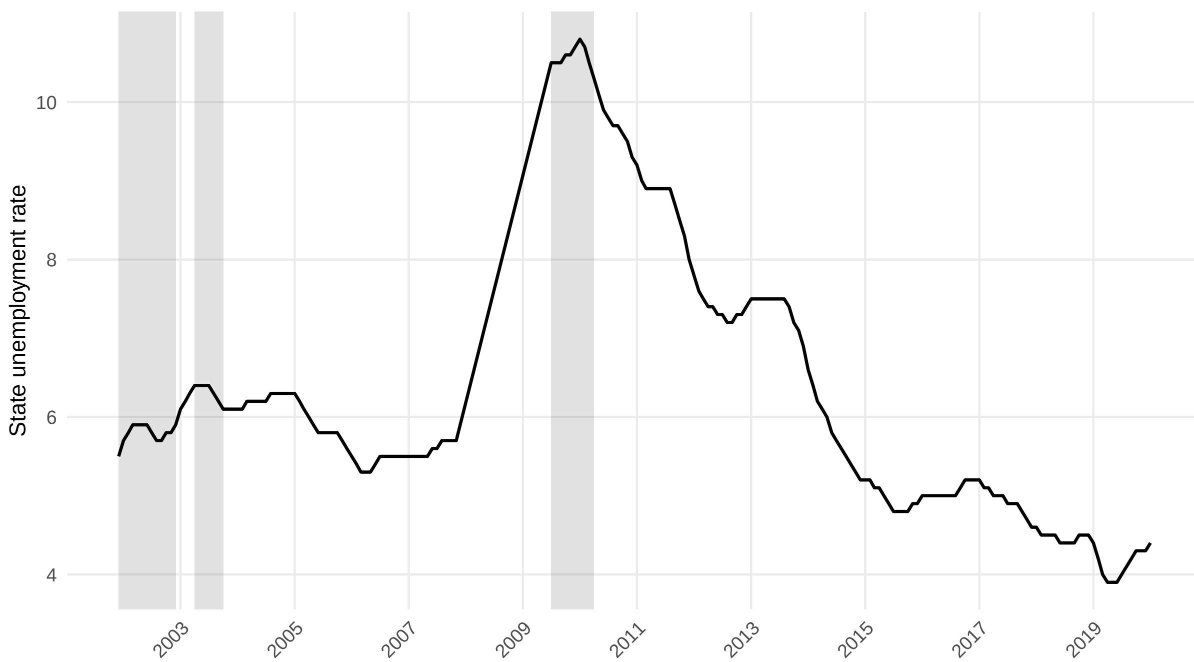
Another distinct identification concern arises for WARN-based specifications because WARN notices are tied to establishment location rather than worker residence. A layoff notice filed in one county may affect workers who live in neighboring counties and commute across county lines for employment. In that case, counties classified as untreated may still contain affected residents, while the treated county may not contain the full set of economically exposed households. This creates a potential violation of the stable unit treatment value assumption. The practical implication is attenuation rather than spurious discovery. If treatment exposure spills across county lines, then the contrast between treated and untreated counties is weakened. The treated group no longer cleanly contains the exposed population, and the untreated group is partially contaminated. That structure biases the estimated WARN effect toward zero. Weak or null WARN estimates therefore should not be interpreted too literally as evidence that local mass layoff announcements have no political relevance whatsoever. They may instead reflect that county-of-filing is an imperfect proxy for county-of-exposure. This concern is much less severe for SAHM-based specifications. The unemployment rate is measured at the household level and tied to place of residence, making it more appropriate for capturing the local labor market conditions faced by county residents. For this reason, null results using WARN require somewhat more caution in interpretation than comparable results using SAHM. Even so, the broader pattern

[31]Consistent with institutional features of voter registration systems and observed seasonal patterns in registration data, registration drives are primarily concentrated around election periods and statutory deadlines, rather than being systematically aligned with local economic fluctuations.

across specifications remains the same: the data do not reveal a large, stable response of voter registration to local economic distress.

A final concern is that broader state-level labor market conditions may be driving both county-level treatment and registration outcomes. If so, county-level shocks might simply be reflecting statewide economic cycles rather than genuinely local variation. Figure 8 helps place the county analysis in this broader context by showing Ohio's state unemployment rate together with periods in which the state SAHM trigger is active.

Figure 8: Ohio State Unemployment and State Sahm Trigger Timing



Notes: This figure plots Ohio's state unemployment rate and highlights periods in which the state-level Sahm trigger is active. The figure is descriptive and is intended to place the county-level analysis within the broader state labor-market environment. Month fixed effects in the regression specifications absorb statewide shocks common to all counties in a given month.

The figure makes clear that county-level analysis takes place against a common statewide macroeconomic environment. State-level Sahm triggers coincide primarily with national recession periods and do not provide the within-window county-level treatment variation used for identification. This is useful descriptively, but it does not undermine the identifying strategy. Statewide economic conditions are common to all counties in a given month and are therefore absorbed by the month fixed effects included in every specification. The identifying variation in the regressions comes from differences in county-level exposure within these broader state-level cycles. Figure 8 is therefore best interpreted as contextual. It shows that the local treatment measures are embedded in recognizable statewide economic episodes, but it also clarifies why the empirical design is still able to focus on within-state local variation. Counties are compared against one another while sharing the same broader macroeconomic environment.

Taken together, these sensitivity checks and identification discussions suggest that the paper's main result is not an artifact of a narrow modeling choice. The absence of a strong and consistent effect of local economic distress on voter registration appears to reflect a genuine feature of the registration margin rather than a fragile empirical construction.

CHAPTER SEVEN

CONCLUSION

While existing research has largely emphasized voter turnout, this paper shifts attention to the decision to enter the electorate, providing a new perspective on how economic conditions influence political behavior. As a means to examine the relationship between local economic shocks and political participation, voter registration represents a distinct and underexplored margin of engagement. The results indicate that local economic shocks—whether gradual and diffuse, as captured by Sahm rule indicators, or acute and salient, as captured by WARN notices—have little consistent effect on voter registration. Across multiple specifications, outcome measures, and identification strategies, estimated effects are small and statistically indistinguishable from zero. This finding contrasts with parts of the turnout literature and suggests that the impact of economic conditions on political participation depends critically on the stage of participation being considered.

The primary contribution of this paper is to show that voter registration is not a highly responsive margin of political participation in the context of local economic shocks. While economic theory predicts that participation should respond to changes in economic conditions, the results demonstrate that this relationship does not extend uniformly across all stages of engagement. By focusing on registration, this analysis highlights that earlier-stage participation decisions may be substantially less sensitive to economic shocks than downstream behaviors such as turnout. From a policy perspective, these findings suggest that economic distress does not necessarily translate into increased civic engagement through voter registration. This has important implications for how participation is measured and for how policymakers interpret the relationship between economic conditions and democratic responsiveness. If registration is relatively unresponsive to economic shocks, then efforts to increase participation may need to focus more directly on reducing institutional barriers and lowering the costs of engagement.

The results also highlight the importance of considering multiple dimensions of participation when evaluating political behavior. Future research could build on this work by examining the interaction between registration and turnout, exploring whether economic shocks affect the transition from registration to voting, and investigating heterogeneous effects across individuals and regions. Additionally, further analysis using individual-level data could provide more detailed insights into how different groups respond to economic conditions.

This paper demonstrates that the relationship between economic conditions and political participation depends critically on the margin being examined. Focusing on voter registration reveals that not all forms of engagement respond to economic shocks, even when theory predicts a directional effect. The lack of a negative response—despite the cost structure of registration—suggests that this margin is not highly sensitive to short-run economic conditions. This highlights the role of institutional constraints and measurement choice in shaping conclusions about political behavior.

REFERENCES CITED

[1] Alamo, C. and B. Uhler (2023) "Uptick in Unemployment Rate Triggered a Recession Indicator," EconTax Blog, `https://lao.ca.gov/LAOEconTax/article/Detail/786`.

[2] Bloom, Howard S. and H. Douglas Price (1975) "Voter Response to Short-Run Economic Conditions: The Asymmetric Effect of Prosperity and Recession," *American Political Science Review*, 69 (4), 1240–1254, 10.2307/1955284.

[3] Burden, Barry C. and Amber Wichowsky (2014) "Economic Discontent as a Mobilizer: Unemployment and Voter Turnout," *Journal of Politics*, 76 (4), 887–898, 10.1017/S0022381614000437.

[4] Cantoni, Davide, Vincent Pons, and Johannes Schäfer (2024) "Voting Rules, Turnout, and Economic Policies,"Technical Report 32941, National Bureau of Economic Research, 10.3386/w32941.

[5] Charles, Kerwin Kofi and Melvin Stephens Jr. (2013) "Employment, Wages, and Voter Turnout," *American Economic Journal: Applied Economics*, 5 (4), 111–143, 10.1257/app.5.4.111.

[6] Dhillon, Amrita and Susana Peralta (2002) "Economic Theories of Voter Turnout," *Economic Journal*, 112 (480), F332–F352.

[7] Downs, Anthony (1957) "An Economic Theory of Political Action in a Democracy," *Journal of Political Economy*, 65 (2), 135–150.

[8] Feddersen, Timothy J. and Wolfgang Pesendorfer (1996) "The Swing Voter's Curse," *American Economic Review*, 86 (3), 408–424.

[9] Feddersen, Timothy and Alvaro Sandroni (2006) "A Theory of Participation in Elections," *American Economic Review*, 96 (4), 1271–1282.

[10] Federal Reserve Bank of St. Louis (2026) "Real-time Sahm Rule Recession Indicator," `https://fred.stlouisfed.org/series/SAHMREALTIME`.

[11] Feng, S. and J. Sun (2020) "Misclassification-Errors-Adjusted Sahm Rule for Early Identification of Economic Recession," *SSRN Electronic Journal*, 10.2139/ssrn.3584931.

[12] Fiorina, Morris P. (1976) "The Voting Decision: Instrumental and Expressive Aspects," *Journal of Politics*, 38 (2), 390–413, 10.2307/2129541.

[13] Goodman-Bacon, Andrew (2021) "Difference-in-Differences with Variation in Treatment Timing," *Journal of Econometrics*, 225 (2), 254–277, 10.1016/j.jeconom.2021.03.014.

[14] Harsanyi, John C. (1980) "Rule Utilitarianism, Rights, Obligations and the Theory of Rational Behavior," *Theory and Decision*, 12 (2), 115–133, 10.1007/BF00154357.

[15] Kramer, Gerald H. (1971) "Short-Term Fluctuations in U.S. Voting Behavior, 1896-1964," *American Political Science Review*, 65 (1), 131–143, 10.2307/1955049.

[16] Krolikowski, Pawel M. and Kurt G. Lunsford (2024) "Advance Layoff Notices and Aggregate Job Loss," *Journal of Applied Econometrics*, 39 (3), 462–480, 10.1002/jae.3032.

[17] Labonte, Marc and Lida R. Weinstock (2024) "The Sahm Rule Trigger: Is the United States in a Recession?," Congressional Research Service, https://www.congress.gov/crs-product/IN12410.

[18] Liu, H., W. B. McCartney, R. Ramcharan, C. Zhang, and X. Zhang (2025) "Relieving Financial Distress Increases Voter Turnout: Evidence from the Mortgage Market,"Technical Report 2025-2517, Federal Reserve Bank of Dallas, 10.24149/wp2517.

[19] Martins, Rodrigo and Francisco J. Veiga (2013) "Economic Performance and Turnout at National and Local Elections," *Public Choice*, 157 (3-4), 429–448.

[20] National Bureau of Economic Research, "US Business Cycle Expansions and Contractions," https://www.nber.org/research/data/us-business-cycle-expansions-and-contractions, Retrieved April 12, 2026.

[21] National Cancer Institute (2025) "SEER*Stat Database: Populations – Total U.S. (1969–2023)," Dataset.

[22] National Conference of State Legislatures, "Same Day Voter Registration," https://www.ncsl.org/elections-and-campaigns/same-day-voter-registration, Retrieved April 12, 2026.

[23] Ohio Secretary of State, "Secretary of State Data Portal," https://data.ohiosos.gov/portal/past-election-results, Retrieved April 14, 2026.

[24] ——— , "Statewide Voter Files Download Page," https://www6.ohiosos.gov/ords/f?p=VOTERFTP:STWD, Retrieved April 15, 2026.

[25] Radcliff, Benjamin (1992) "The Welfare State, Turnout, and the Economy," *American Political Science Review*, 86 (2), 444–454, 10.2307/1964232.

[26] Rector, A. and D. S. Peter, "Maine Department of Administrative and Financial Services Office of the State Economist," n.d.

[27] Rosenstone, Steven J. (1982) "Economic Adversity and Voter Turnout," *American Journal of Political Science*, 26 (1), 25–46, 10.2307/2110837.

[28] Sahm, Claudia (2019) "Direct Stimulus Payments to Individuals," Brookings.

[29] Schuessler, Alexander A. (2000) "Expressive Voting," *Rationality and Society*, 12 (1), 87–119, 10.1177/104346300012001005.

[30] Sun, Liyang and Sarah Abraham (2021) "Estimating Dynamic Treatment Effects in Event Studies with Heterogeneous Treatment Effects," *Journal of Econometrics*, 225 (2), 175–199, 10.1016/j.jeconom.2020.09.006.

[31] Swartz, Teresa T., Amy Blackstone, Christopher Uggen, and Heather McLaughlin (2009) "Welfare and Citizenship: The Effects of Government Assistance on Young Adults' Civic Participation," *Sociological Quarterly*, 50 (4), 633–665, 10.1111/j.1533-8525.2009.01154.x.

[32] Wolfinger, Raymond E. and Steven J. Rosenstone (1980) "The Effect of Registration Laws on Turnout," in *Who Votes?*, 61–88: Yale University Press.

APPENDICES

APPENDIX A

ADDITIONAL TABLES

This appendix reports supplementary regression tables referenced in the main text but not reproduced there.

Table 6: Alternative WARN Specifications for First-Time Registrations

Specification	**Window 1**	**Window 2**
Binary	0.002	-0.003
	(0.008)	(0.013)
Count	-0.002	-0.014*
	(0.007)	(0.007)
Intensity	-0.001	-0.002**
	(0.002)	(0.001)

Notes: The outcome is first-time registrations per 1,000 voting-age population. Entries are coefficients from county and month fixed-effects regressions, with county-clustered standard errors in parentheses. The binary specification uses the absorbing WARN treatment indicator. The count specification uses the number of WARN notices in the county-month. The intensity specification scales affected workers by voting-age population. Window 1 and Window 2 refer to the two inter-recession analysis windows. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table 7: Alternative Period Definitions for First-Time Registrations

Specification	Estimate	Std. error
Window 1		
Sahm: Baseline	0.013	(0.017)
Sahm: Election reset	-0.003	(0.011)
Sahm: Pre-election	-0.029	(0.074)
WARN: Baseline	0.002	(0.008)
WARN: Election reset	0.008	(0.005)
WARN: Pre-election	-0.030	(0.040)
Window 2		
Sahm: Baseline	-0.010	(0.012)
Sahm: Election reset	-0.013	(0.013)
Sahm: Pre-election	-0.009	(0.088)
WARN: Baseline	-0.003	(0.013)
WARN: Election reset	0.013*	(0.007)
WARN: Pre-election	-0.008	(0.060)

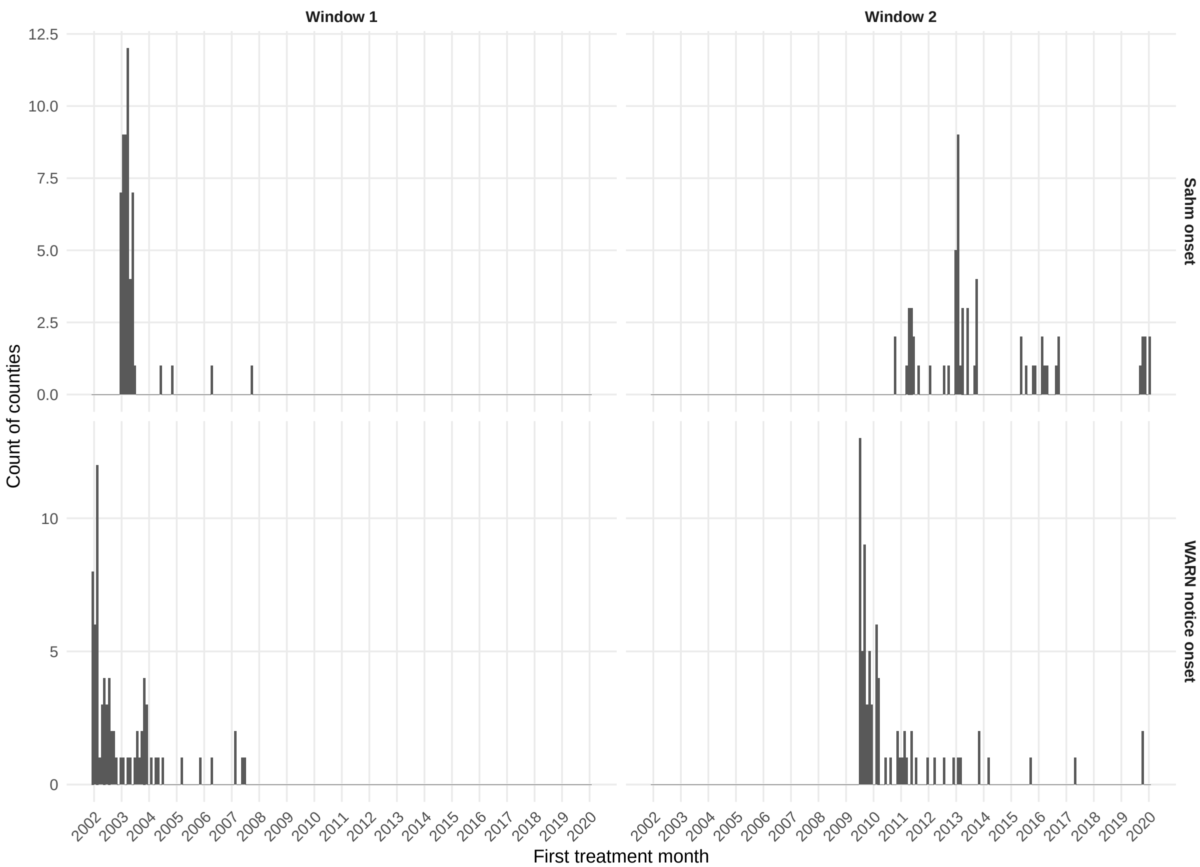
Notes: The outcome is first-time registrations per 1,000 voting-age population. Entries are county and month fixed-effects coefficients, with county-clustered standard errors in parentheses. Baseline specifications use the main inter-recession windows. Election-reset specifications redefine treatment timing within periods that restart after each November general election and remove counties already treated at the beginning of the period. Pre-election specifications restrict the sample to the months immediately preceding November general elections. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

APPENDIX B

ADDITIONAL FIGURES

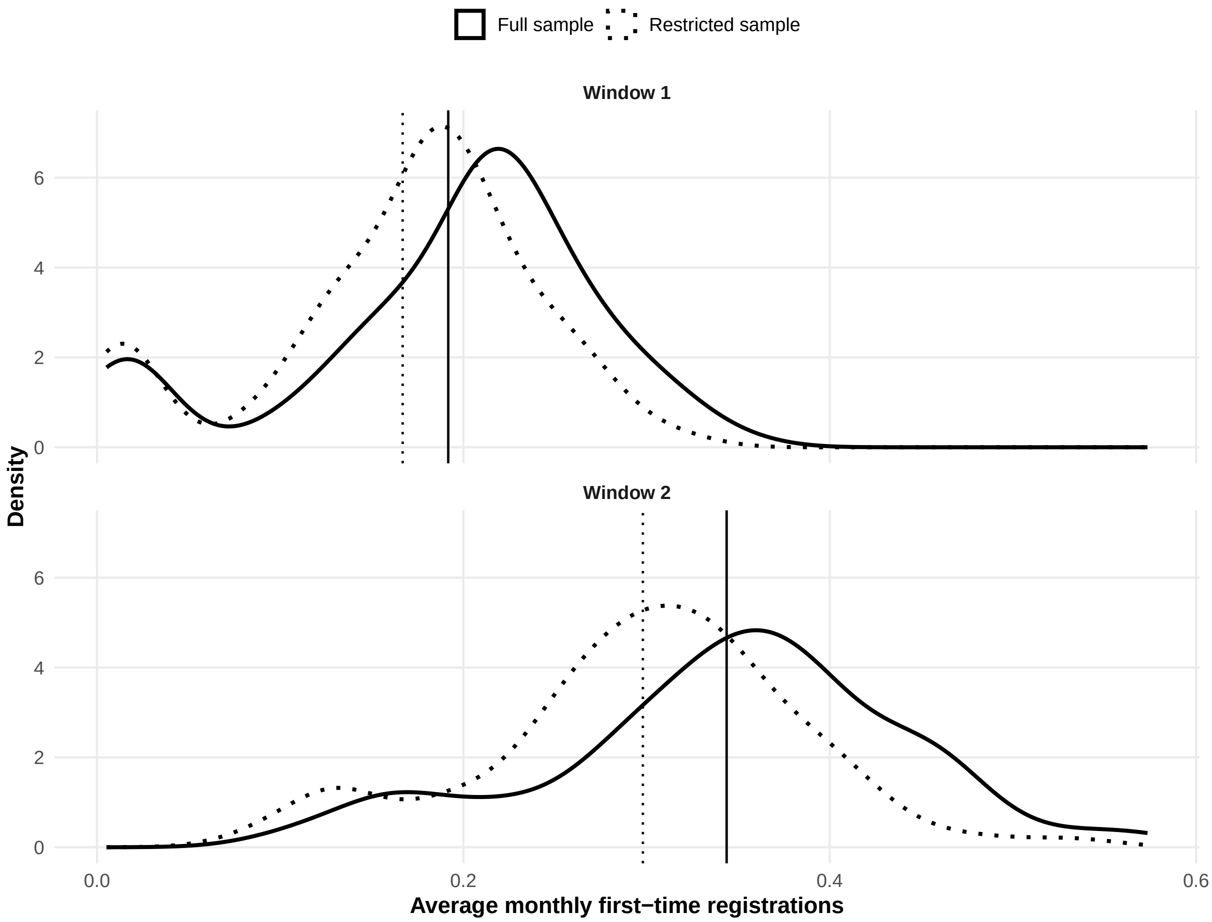
This appendix reports supplementary figures that support the descriptive and event-study analysis in the main text.

Figure 9: Distribution of First Treatment Onset by County



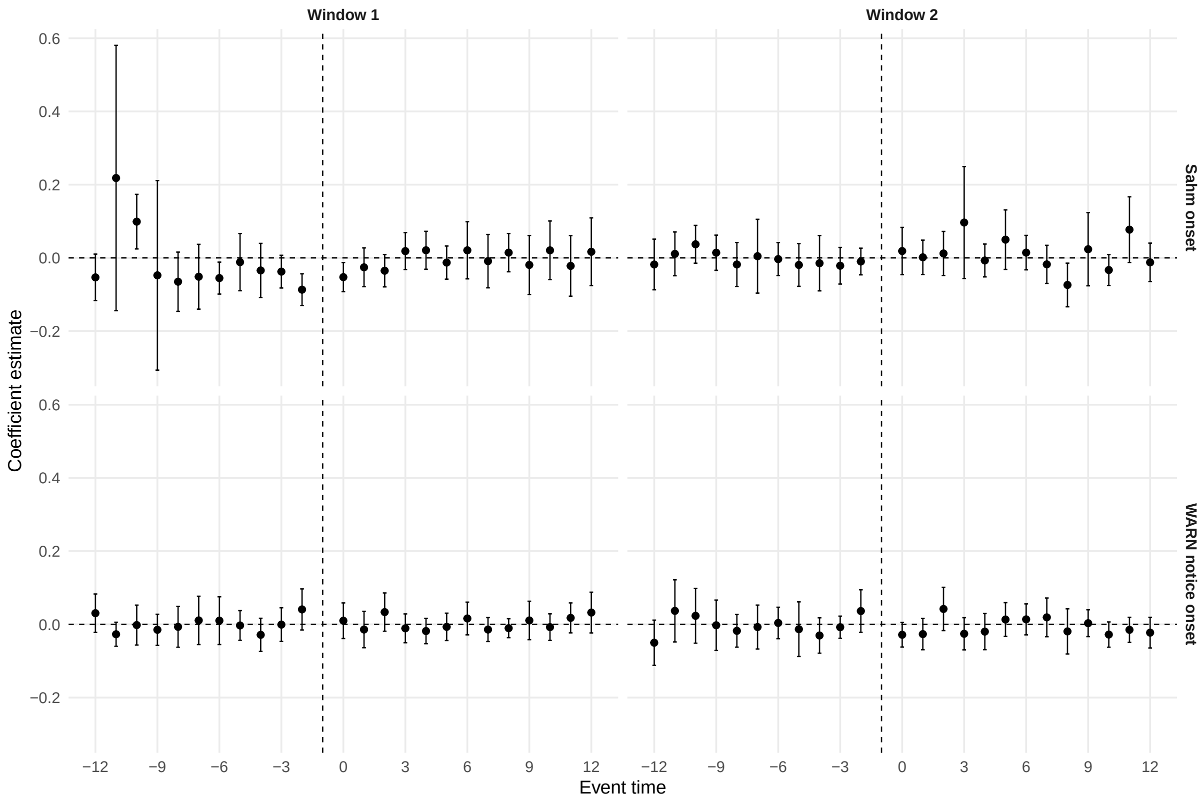
Notes: This figure shows the distribution of first observed treatment onset by county, separately for Sahm-rule onsets and WARN notice onsets. Each county contributes at most one observation for each treatment type within the relevant window. Sahm panels use the Sahm-trimmed estimation windows, while WARN panels use the full inter-recession windows.

Figure 10: Cross-County Distribution of Average Monthly First-Time Registration Rates



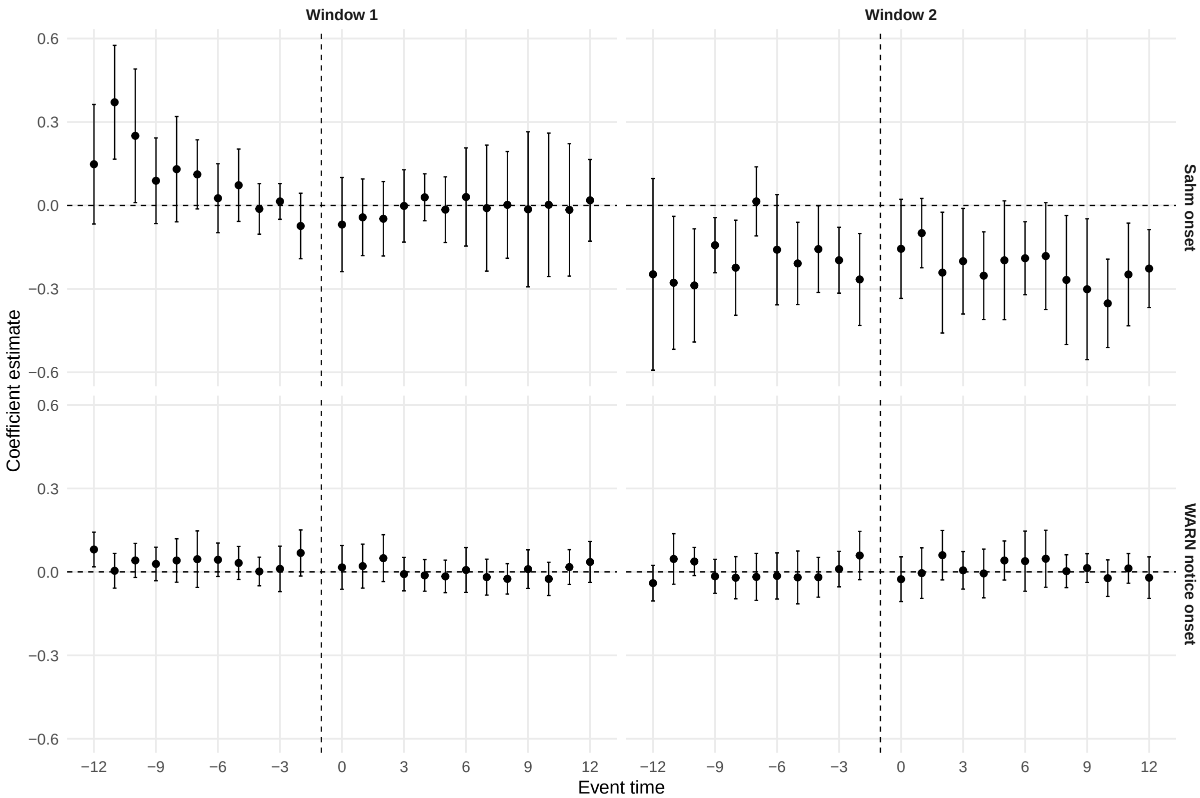
Notes: This figure shows the cross-county distribution of average monthly first-time registration rates separately by inter-recession window and sample definition. Registration rates are measured per 1,000 voting-age population. Full samples include all months in the relevant window. Restricted samples exclude September and October in election years. Vertical lines mark the corresponding sample means.

Figure 11: TWFE Event-Study Estimates for First-Time Registrations



Notes: This figure reports dynamic treatment-effect estimates from two-way fixed-effects event-study specifications. The outcome is first-time registrations per 1,000 voting-age population. Event time is measured in months relative to treatment onset, with month -1 omitted as the reference period. The plotted estimates include 95% confidence intervals based on county-clustered standard errors. Sahm panels use the Sahm-trimmed estimation windows, while WARN panels use the full inter-recession windows.

Figure 12: Sun and Abraham Event-Study Estimates for First-Time Registrations



Notes: This figure reports dynamic treatment-effect estimates from Sun and Abraham event-study specifications. The outcome is first-time registrations per 1,000 voting-age population. Event time is measured in months relative to treatment onset, with month -1 used as the reference period. The plotted estimates include 95% confidence intervals based on county-clustered standard errors. Sahm panels use the Sahm-trimmed estimation windows, while WARN panels use the full inter-recession windows.

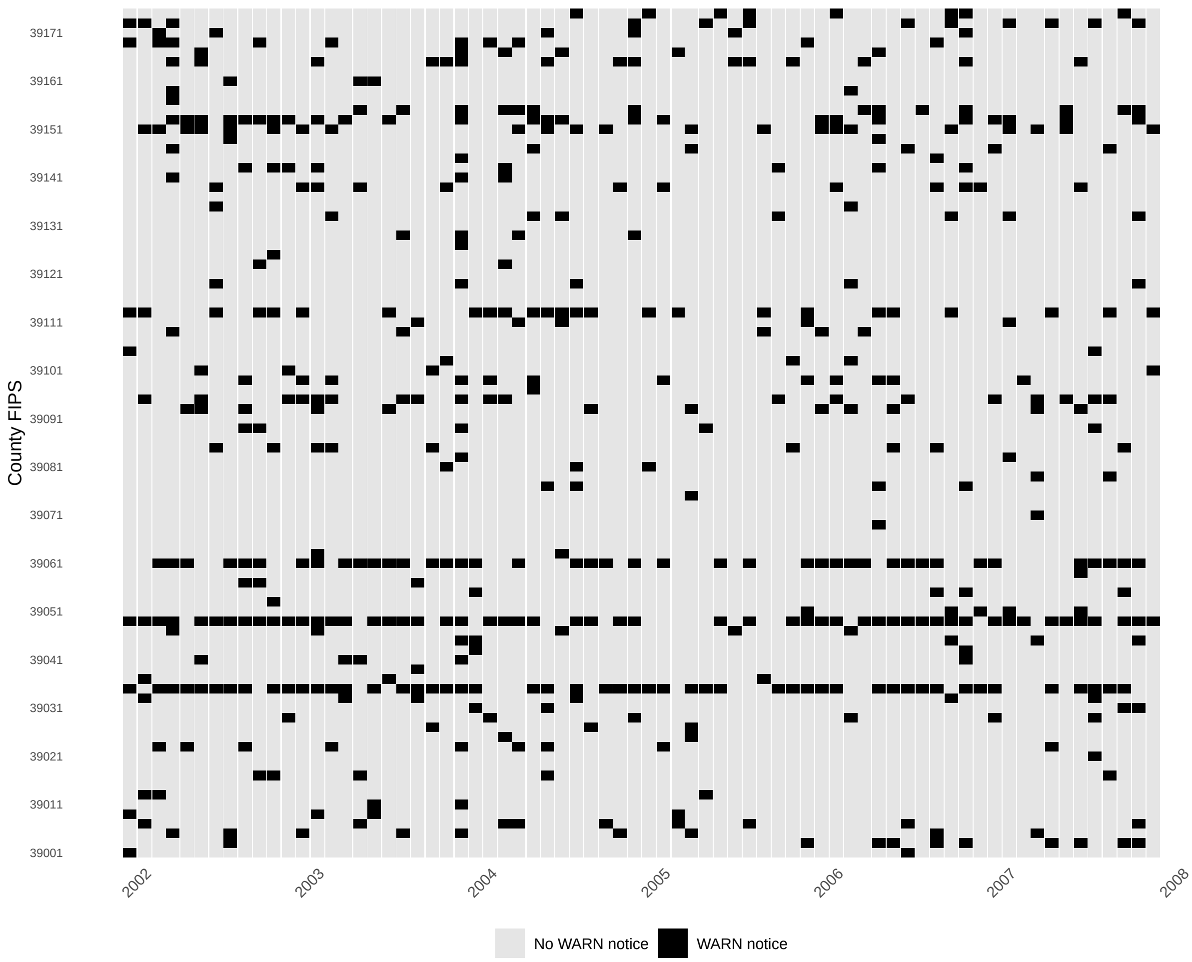
Figure 13: County Sahm Trigger Timing, Window 1

Notes: This heatmap displays the monthly timing of county-level Sahm-rule triggers in Window 1. A highlighted cell indicates that a county-month satisfies the Sahm trigger definition. Counties already triggered at the selected trimmed start month are shown with reduced opacity. The vertical line marks the start month chosen for the Sahm-trimmed analysis window. Counties are displayed in FIPS order.

Figure 14: County Sahm Trigger Timing, Window 2

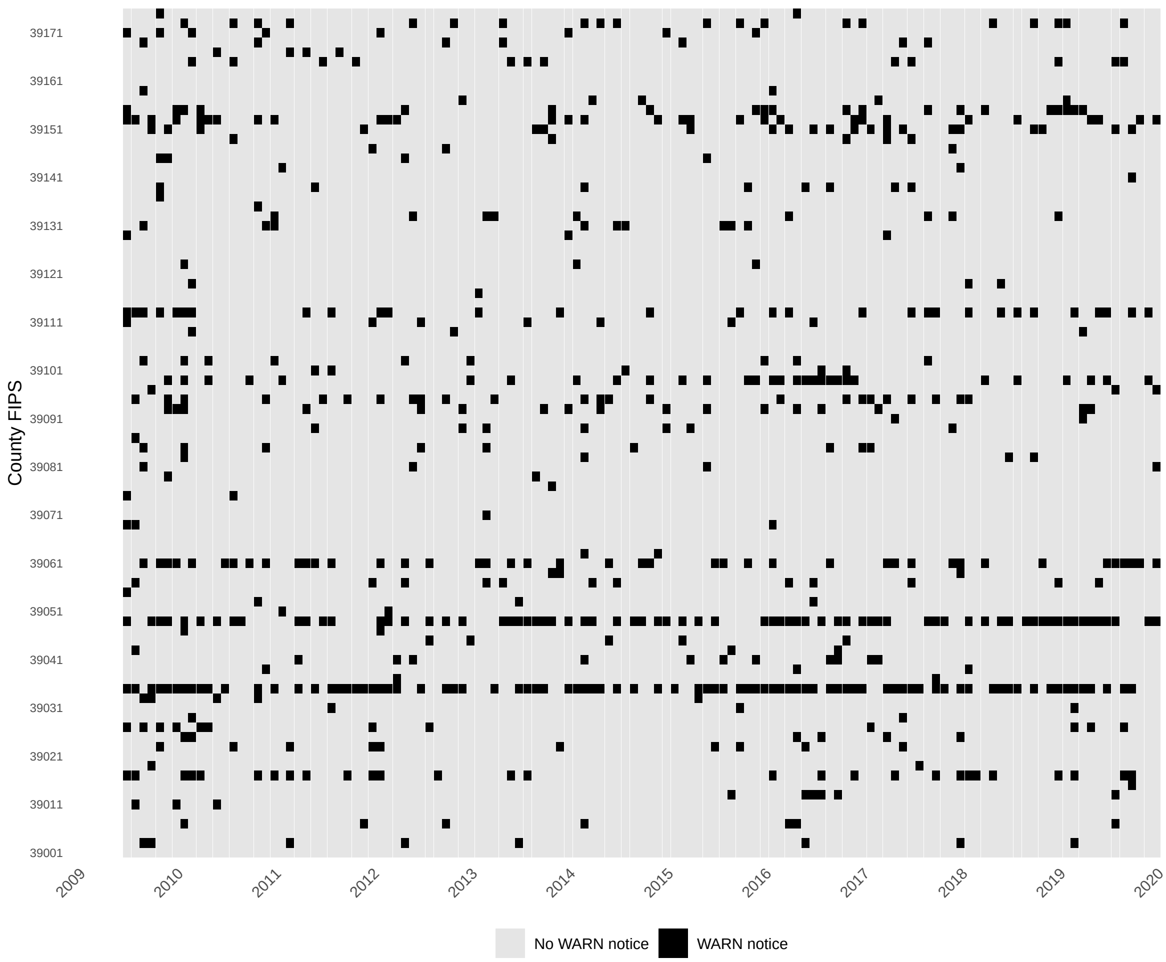
Notes: This heatmap displays the monthly timing of county-level Sahm-rule triggers in Window 2. A highlighted cell indicates that a county-month satisfies the Sahm trigger definition. Counties already triggered at the selected trimmed start month are shown with reduced opacity. The vertical line marks the start month chosen for the Sahm-trimmed analysis window. Counties are displayed in FIPS order.

Figure 15: WARN Notice Timing by County, Window 1



Notes: This heatmap displays the monthly timing of WARN notices by county in Window 1. A highlighted cell indicates that at least one WARN notice is observed in the county-month. Counties are displayed in FIPS order. WARN panels use the full inter-recession windows.

Figure 16: WARN Notice Timing by County, Window 2



Notes: This heatmap displays the monthly timing of WARN notices by county in Window 2. A highlighted cell indicates that at least one WARN notice is observed in the county-month. Counties are displayed in FIPS order. WARN panels use the full inter-recession windows.

APPENDIX C

OHIO INSTITUTIONAL DETAILS

Ohio is a useful setting for this analysis because voter registration is a distinct administrative step that occurs before voting. Unlike states with Election Day registration, Ohio requires voters to register in advance of an election. This matters for the thesis because the dependent variable measures registration, not turnout. Registration is therefore interpreted as an upstream participation decision: it is the cost a person must pay before being able to vote, but it is not itself the final act of political participation.

Several Ohio election institutions changed between 2000 and 2020. The most relevant changes were the expansion of no-excuse absentee voting beginning in 2006, changes to early voting and the "Golden Week" period during the 2010s, the introduction of online voter registration beginning in 2017, and continued voter-list maintenance procedures. These changes matter because they shaped the broader voting environment in which registration decisions occurred.

Most of these institutional changes are not central to the empirical analysis because they do not create the type of county-level variation used for identification. The thesis relies on differences across Ohio counties in the timing of local economic shocks, measured through county-level Sahm triggers and WARN notices. By contrast, major election-administration changes generally occurred statewide. Since the regressions include month fixed effects, statewide changes common to all counties in a given month are largely absorbed by the empirical design.

The most direct registration-cost change is online voter registration, which began in 2017. This likely made registration easier for some voters. However, because it was implemented statewide at a single point in time, it does not provide county-specific treatment variation. It may affect the overall level of registrations after implementation, but it is not well suited for explaining why one county's registration rate changes relative to another county's in response to local economic distress.

Other reforms are more closely connected to voting than registration. No-excuse absentee voting and early-voting changes affected how and when registered voters could cast ballots. These rules may influence turnout or election-period mobilization, but they are less directly tied to the monthly decision to register. For that reason, they are best treated as institutional background rather than primary explanatory variables.

Election timing remains important. Registration activity is likely to rise before major November general elections, especially presidential elections. The analysis addresses this concern by using month fixed effects and by checking robustness to excluding election-adjacent months. This is preferable to modeling each institutional rule change separately, because the main research question is not whether Ohio election reforms affected registration, but whether local economic shocks affected registration within Ohio's institutional framework.

Overall, Ohio's institutional details support the interpretation of registration as an advance, preparatory form of political participation. The key legal changes between 2000 and 2020 are worth noting, but they do not substantially alter the identification strategy because they are mostly statewide, downstream of registration, or already handled through fixed effects and election-month robustness checks.

APPENDIX D

USE OF THE SAHM RULE IN POLICY AND GOVERNMENT ANALYSIS

This appendix documents the use of the Sahm Rule across federal policy institutions and applied economic analysis. The evidence demonstrates that the rule has been formally implemented in federal data systems, referenced in Congressional policy analysis, and used as a practical monitoring tool in broader economic applications.

D.1 Federal-Level Implementation and Policy Use

The Sahm Rule was originally developed as part of a broader proposal for automatic fiscal stabilization. In her policy work, Sahm (2019) proposed that the rule could serve as a trigger mechanism for direct stimulus payments, allowing fiscal policy to respond rapidly to deteriorating economic conditions without requiring new legislative action. This design highlights a key feature of the Sahm Rule: it is not merely descriptive, but explicitly constructed as a policy tool for real-time macroeconomic stabilization.

The most direct institutional implementation of the Sahm Rule appears in the Federal Reserve Bank of St. Louis database. The Federal Reserve Bank of St. Louis maintains a publicly available time series titled "Real-time Sahm Rule Recession Indicator." This series operationalizes the rule exactly as defined in the original research, using monthly unemployment data to calculate the threshold condition. The indicator is updated in real time and presented alongside other core macroeconomic variables, reflecting its integration into standard economic monitoring practices (Real-Time Sahm Rule Recession Indicator, 2026).

In a 2024 report, the Congressional Research Service evaluates whether recent economic conditions meet the criteria for recession and explicitly incorporates the Sahm Rule into its analysis. The report discusses the rule's trigger condition, its historical performance, and its interpretation in the context of current labor market data. Importantly, the Congressional Research Service notes that the Sahm Rule is widely used by economists and policymakers as a real-time diagnostic tool, particularly in contrast to retrospective recession dating methods (Labonte & Weinstock, 2024).

D.2 Broader Applied and Subnational Use

At the state level, the use of the Sahm Rule appears most clearly in official economic monitoring publications, where it is incorporated as part of a broader set of recession indicators. A particularly strong example comes from the Maine Office of the State Economist. In its monthly Maine Economic Indicators reports, the state includes a dedicated subsection titled "Real-time Sahm Rule Recession Indicator," where it defines the rule using the standard 0.5 percentage point threshold (Rector & Peter, 2026).

Evidence of state-level usage also appears in fiscal and policy-oriented analysis. The California Legislative Analyst's Office explicitly references the Sahm Rule in its economic commentary, noting that economists and federal policymakers regularly track the so-called Sahm Rule to help identify the beginning of a recession and describing its 0.5 percentage point threshold definition. In this context, the rule is used as part of a broader framework for interpreting labor market conditions and recession risk, indicating that it has diffused into state-level fiscal analysis and policy discussion (Alamo & Uhler, 2023).