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The Early Labor Market Impacts of Minnesota's 2023 Non-Compete Enforcement Ban

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Abstract

We assess the early impacts of Minnesota's 2023 non-compete enforcement ban on earnings and mobility outcomes. We use a variety of methodologies to study this ban covering all new non-compete contracts in the state, including synthetic differences-in-differences, synthetic control, and a triple-difference methodology based on the industry prevalence of non-competes. Across methods, we are unable to detect labor market impacts either for new hires or for workers overall. Our null results could be due to studying the policy too soon before labor market changes have taken place, imperfections in our methodologies, small effects not discernible with our current methods, or a true non-effect of the non-compete policy. Analysis of job postings data, though very noisy, shows limited change in employer use of non-competes.

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I. Introduction

Non-competes are restrictive covenants between employers and employees that impose limits on employees' abilities to work for a business in the same industry for some time after leaving their job. Survey evidence suggests that between 13 percent and 18 percent of employed Americans have non-compete contracts in their current job (Boesch et al. 2023; Starr et al. 2021).

In this research brief, we evaluate the early labor market impacts of Minnesota's 2023 ban on the enforcement of new non-noncompete contracts, the first categorical ban enacted since the 19th century. The law banning non-compete enforcement was passed on May 16, 2023, and came into effect on July 1, 2023. Contracts signed before July 1, 2023, would be still enforceable under prior Minnesota law, but new contracts would not be. Minnesota provides the first modern case study of a state wholly banning non-compete enforcement.

We are unable to consistently detect labor market impacts for any of the outcomes we study across our methodologies. Basic robustness and heterogeneity exercises likewise yield null effects. Our findings may reflect a true null effect of the policy, but they could also reflect other possibilities.[1] For example, the labor market response to the non-compete enforcement ban may be slow and not detectable with only about a year of post-ban data. Our confidence intervals are also somewhat wide given the limited availability of post-ban data, so we may not yet be able to detect small to moderate impacts.

The welfare implications of non-compete enforcement have been the focus of theoretical and empirical research. In principle, non-compete contracts could benefit both workers and

[1] There could be other factors uniquely affecting the Minnesota labor market, at the same time as the policy change, in industries with high non-compete shares. For example, Minnesota's relatively low unemployment rate, high share of remote work, high share of college graduates, or low but growing share of foreign-born residents could have shaped its labor market trajectory during our period of study, potentially masking the impacts of the non-compete ban. Such confounding factors could be affecting the validity of our methodologies and their ability to detect effects from the non-compete enforcement ban.

employers by protecting trade secrets and encouraging investment in human capital, with employers compensating workers in exchange for signing non-competes. However, non-compete contracts could also inefficiently reduce work mobility and lower earnings if employers use them to consolidate the leverage they have over workers at the beginning of an employment relationship. Moreover, non-competes can create negative spillovers for competing employers who consequently find it more difficult to hire.

Although non-compete contracts are prevalent across the country, states vary in how strictly they enforce them (Prescott et al. 2016). Some states like North Dakota and California have generally not enforced non-competes, while other states are stricter, with some even enforcing non-competes for workers terminated without cause or allowing courts to modify overly broad non-compete contracts and enforce them rather than ruling them entirely invalid. States have also varied in their enforceability of non-competes over time, with changes in legislation and judicial decisions increasing or decreasing non-compete enforceability. Scholars have used this variation to study the impacts of non-compete enforceability.

A growing body of research has investigated effects of enforceability on outcomes including entrepreneurship, investment, and innovation. Most studies indicate that non-competes and/or their stricter enforceability are associated with worse outcomes for young firms (Ewens and Marx 2018; Starr et al. 2018; Jeffers 2024) and diminished innovation (Belenzon and Schankerman 2013; Ewens and Marx 2018; Marx, 2022). Effects on investment, particularly in human capital, appear to be positive (Starr 2019).

Focusing on the labor market, researchers have examined labor mobility—the outcome that non-compete contracts most directly affect—and implications of mobility for job match quality (Shi 2023). Closely related are questions about how non-competes and non-compete

enforcement matter for workers' earnings. Most research has found negative effects on mobility and earnings (Marx et al. 2009; Lipsitz and Starr 2022; Balasubramanian et al. 2022; Johnson et al. 2025). Some studies have found more mixed results for executives (Garmaise 2011; Kini et al. 2021). Shi (2023) finds that initial wages for executives are higher with non-competes, but subsequent wage growth is lower.

Non-compete policy has changed in other states during the recent past, but these changes typically focused on specific industries, occupations, or wage groups. For example, Illinois and Washington recently banned non-compete enforcement for those earning less than roughly $75,000 and $123,000, respectively, but not for those earning above that level. Evaluating Minnesota's non-compete enforcement ban may thus provide unique insights into the labor market impacts of non-compete enforceability.

We employ a variety of methodologies to assess the impact of Minnesota's non-compete enforcement ban, including synthetic differences-in-differences, synthetic control, and a triple-difference methodology based on the industry prevalence of non-competes. To construct our outcome variables, we use earnings and mobility data from the Quarterly Workforce Indicators (QWI) that allow us to examine effects on both new hires, who may have been particularly affected by the policy, as well as workers overall. These data extend through 2025, providing about two years of post-treatment information. Previous research highlights that reducing non-compete enforceability for one group of workers, such as new hires, can spill over to other workers in equilibrium (Gottfries and Jarosch 2025; Starr, Frake, and Agarwal 2019). We also rely on job postings data from Lightcast to observe whether employers changed how they discussed non-competes in their job ads.

As more data become available, we plan to update our analysis to better understand how the non-compete enforcement ban is affecting the Minnesota workforce. The current research brief outlines our methodologies and presents our early results in the subsequent sections.

II. Data and Methodology

To study the effects of the Minnesota non-compete enforcement ban, we rely on data from the QWI. The QWI is produced by the Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program. The QWI combines administrative earnings and employment data from state unemployment insurance programs with demographic data provided by the Census Bureau. We construct four key outcome variables from the QWI: (logged) earnings for new hires deflated to 2024 dollars, (logged) earnings for all workers deflated to 2024 dollars, separation rates for new hires, and turnover rates for all workers. The separation rate for new hires is a short-term separation rate measuring the share of new hires who separate within a quarter of their hire. We adjust the timing of the published variables so that they correspond with the quarter of hire (or potential hire for overall variables). We use data from 2014 through 2025 as available. Details on how QWI variables are defined and how we construct our dataset are available in Appendix B.

We also examine data from the job posting firm Lightcast, which aggregates millions of job ads posted online, parsing information listed in the posting as well as providing the text of the posting. We analyze the text of the posting to see if it contains a discussion of non-compete contracts. We aggregate this data to create a variable that describes the number of postings with non-compete mentions for every 10,000 total postings in each state and month. Although postings mentioning non-competes are rare, they provide us with more specific information on

how employers are changing their practices in response to the law. We look both at overall non-compete mentions as well as at mentions specifically stating no non-compete is required, those discussing competitor non-competes, and other non-compete mentions that don't fall into the aforementioned categories and are presumably about the employer's own non-compete requirement. We use data on postings from 2014 through May 2025. Details about our Lightcast variables are also available in Appendix B.

We adjust all the outcome variables we study for seasonality. We do so by regressing the outcome on time indicators and subtracting the estimated quarterly or monthly effect centered so that the overall mean is preserved. We individually seasonally adjust each state or each state by industry or education cell provided in the data.

Table 1 provides summary statistics for our main outcomes of interest from the QWI and Lightcast data sources for all states and for Minnesota. The table shows that average 2022 monthly earnings were about $4,000 for new hires and $6,000 for all workers. These earnings are provided at the job rather than worker level and thus include separate values for individuals with multiple jobs, and they cover both part-time and full-time job holders. The subsequent rows show that about half of all new hires left by the next quarter, and that turnover in general was about 10 percent. In Minnesota, earnings were somewhat higher and turnover was slightly lower. Panel B shows that in 2022, about 20 posts out of every 10,000 mentioned non-competes in any way, with about one of those posts specifying no non-compete was required, 18 of them discussing competitor non-competes, and one describing non-competes in other ways presumably about the employer's own non-compete policy. Mentions of competitor non-competes were rarer in Minnesota. The final section of the table highlights that of the 51 states and District of Columbia, three states never enforced non-competes during our study period, nine

passed new non-compete enforcement rollbacks, and four had incomplete QWI data. We discuss in more detail below how these state characteristics impact our methodology.

Table 1-
Summary Statistics for Key Outcome Variables

	2022 Mean All States	2022 Mean Minnesota
	(1)	(2)
	A. QWI Data	
Earnings for New Hires (in 2024 Dollars)	4002	4370
Earnings for All (in 2024 Dollars)	5982	6541
New Hire Early Separations	0.5273	0.4705
Turnover for All	0.1047	0.0963
	B. Lightcast Data	
Any Non-Compete Mention Per 10k Posts	19.90	7.46
No Non-Compete Mention Per 10K Posts	0.70	0.71
Competitor Non-Compete Mention Per 10K Posts	17.87	5.06
Other Non-Compete Mention Per 10K Posts	1.33	1.69
	C. Counts	
States in United States	51	
Never Enforced Non-Compete States	3	
New Policy States	9	
Incomplete QWI Data States	2	

Notes: Data are from the Quarterly Workforce Indicators and Lightcast.

Our primary methodology for analyzing the Minnesota non-compete enforcement ban is the synthetic differences-in-differences methodology introduced by Arkhangelsky et al. (2021). Synthetic differences-in-differences is an estimator for causal effects with panel data that builds on insights from both difference-in-differences and synthetic control methods. The method matches pre-treatment outcome trends with an explicit weighting of comparison units like synthetic control, and it is invariant to additive unit-level shifts like difference-in-differences. In essence, the methodology selects and reweights a group of states and time periods to serve as the control group for Minnesota. This "synthetic Minnesota" is based on the best match to

Minnesota's trends leading up to the policy change among states that didn't experience a policy change. The post-July 2023 outcomes of the synthetic Minnesota approximate Minnesota's counterfactual in a scenario with no change in non-compete policy. The method improves the reliability of estimated effects by making sure the control group's pre-July 2023 trends are closely matched to those of Minnesota and limits any bias at the hand of the researcher in constructing that control group.

We carry out the synthetic differences-in-differences methodology using the default implementation in the *sdid* Stata package provided by Clarke et al. (2025) based on the estimation described in Clarke et al. (2023). The coefficient of interest $\hat{\tau}^{sdid}$ is obtained from the following minimization problem:

$$(1) \qquad \left(\hat{\tau}^{sdid}, \hat{\mu}, \hat{\alpha}, \hat{\beta}\right) = \arg\min_{\tau,\mu,\alpha,\beta} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} (Y_{it} - \mu - \alpha_i - \beta_t - W_{it}\tau)^2 \hat{\omega}_i^{sdid} \hat{\lambda}_t^{sdid} \right\}$$

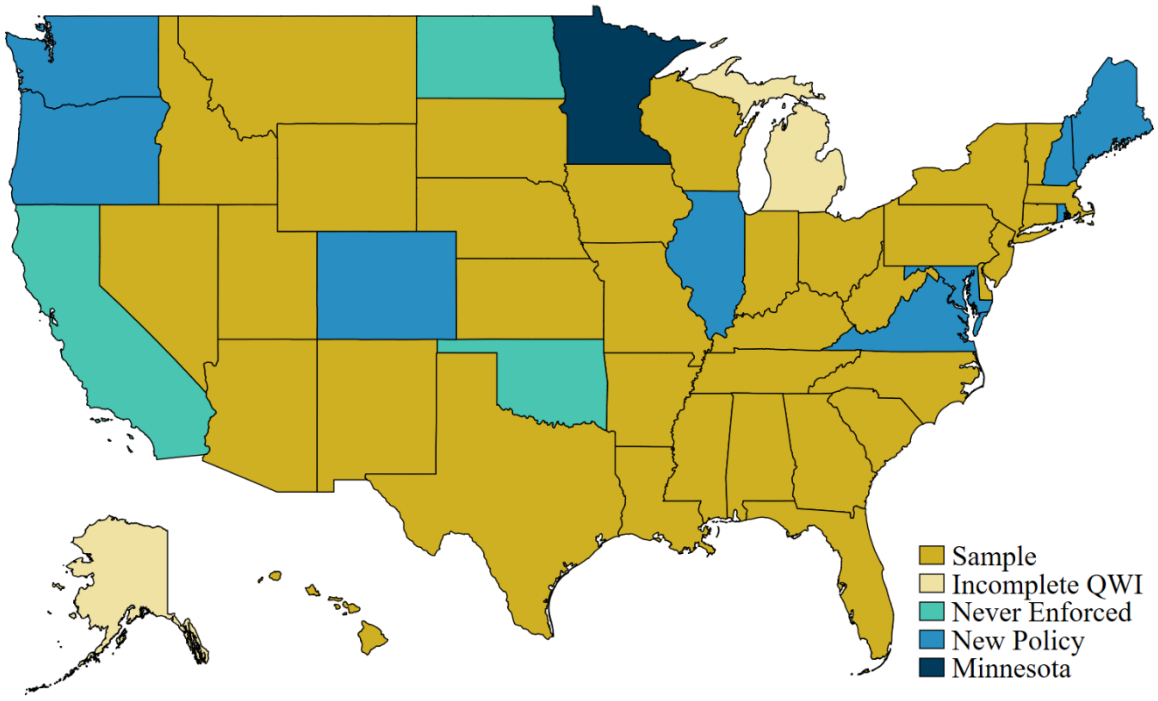
where $\hat{\omega}_i^{sdid}$ is obtained from minimization of the mean pre-treatment difference between the treated outcome and mean outcome of control units (weighted by $\hat{\omega}_i^{sdid}$). Similarly, $\hat{\lambda}_t^{sdid}$ is obtained by minimizing the difference between the mean pre-treatment outcome (each pre-treatment period weighted by $\hat{\lambda}_t^{sdid}$) and the mean post-treatment outcome. See Arkhangelsky (2021) for details.

We use the placebo method for conducting inference and constructing our standard errors. The placebo method essentially re-estimates the methodology for control units and assesses how likely it is that an estimate as extreme as our one for Minnesota in 2023 occurs.

Because synthetic differences-in-differences is not valid for always treated units, we exclude California, North Dakota, and Oklahoma—all states that have generally not enforced non-compete contracts since at least the 19th century. Because we are focused specifically on Minnesota's policy change, we also exclude states that have implemented related policy changes

during the analysis period of 2014 through 2025. This entails the exclusion of Colorado, Illinois, Maryland, Maine, New Hampshire, Oregon, Rhode Island, Virgina, and Washington due to their bans on non-competes for fairly wide sets of workers with earnings under certain wage thresholds between 2014 and 2025 (Economic Innovation Group 2025). We explore robustness to including these states with similar but non-identical policy changes in our analysis. Figure 1 provides a map depicting the states excluded from our main sample.

Figure 1. Map of Analysis Sample



We also exclude states with incomplete QWI data for our analysis of QWI outcomes because synthetic differences-in-differences requires a complete panel for estimation. Some states take longer to report their data to the QWI. Since the QWI data are only available for a limited amount of time after the non-compete enforcement ban went into effect, we opt to maximize the number of time periods we analyze rather than focusing on quarters where more states have reported their data to the QWI. Future iterations of this paper may expand the set of included states as their data become more available.

We examine robustness to our exclusion of states implementing income-based non-compete policy laws and explore heterogeneity based on industry and education. For our analysis of heterogeneity by industry, we rely on data from the 2022, 2023, and 2024 Surveys of Household Economics and Decisionmaking (SHED). These data contain information on whether respondents signed a non-compete contract and the industry in which they work. Non-compete prevalence ranges from 5 percent to 22 percent across industries, with a mean of 13 percent and a median of 12 percent. We merge these data with data by industry in the QWI, and we assign high non-compete industries to be those with above median rates of non-competes and low non-compete industries to be those with at or below median non-compete prevalence.[2] The QWI also disaggregates data by educational attainment for workers above the age of 25. We examine the effects of the non-compete enforcement ban for individuals with and without a college degree.

In addition to exploring the impacts of the Minnesota non-compete enforcement ban with the synthetic differences-in-differences methodology, we use two other methods to assess the effects and determine how sensitive the results are to the assumptions of the synthetic differences-in-differences methodology. First, we provide results for a synthetic control methodology using an analogous sample, implementation, and inference procedure. The synthetic control methodology builds a weighted combination of untreated states that best mimic Minnesota's pre-July 2023 outcomes and then compares the post-July 2023 differences to estimate the effect. Unlike synthetic differences-in-differences, synthetic control does not reweight time periods or accommodate level differences in pre-treatment trends of comparison units. Formally, synthetic control obtains the coefficient of interest $\hat{\tau}^{sc}$ is from the following minimization problem that is analogous to synthetic differences-in-differences with less

[2] We define the median non-compete prevalence without weighting by industry employment size, but the results are unchanged when using a weighted median.

flexibility due to the exclusion of the α_i unit fixed effects and $\hat{\lambda}_t$ time period weights (Clarke et al. 2023):

$$(2) \qquad \left(\hat{\tau}^{sc}, \hat{\mu}, \hat{\beta}\right) = \arg\min_{\tau,\mu,\alpha,\beta} \{\sum_{i=1}^{N} \sum_{t=1}^{T} (Y_{it} - \mu - \beta_t - W_{it}\tau)^2 \hat{\omega}_i^{sc}\}$$

Second, we estimate a triple-differences methodology that uses variation in non-compete prevalence across industry using the SHED data described above. This methodology has the benefit of netting out Minnesota specific trends that are constant across industries, but it has the weakness that non-compete prevalence is not necessarily tightly correlated with non-compete enforcement (Boesch et al. 2025). As with synthetic differences-in-differences and synthetic control, the triple-differences approach also requires a parallel trends assumption. We explore whether the parallel trends assumption is visibly violated in an analogous event study that is presented alongside the synthetic differences-in differences and synthetic control figures in Appendix A. Our primary triple differences methodology can be characterized by the following equation:

$$(3) \qquad outcome_{ist} = \beta_0 + \beta_1 MN \times post \times nc_share_{ist} + \sigma_s \times \eta_i + \sigma_s \times \tau_t + \eta_i \times \tau_t + \sigma_s + \eta_i + \tau_t + \varepsilon_{ist}$$

Here, β_1 represents our parameter of interest, which is the effect of being in the state of Minnesota after July 2023 for those with non-competes (i.e. having a nc_share_{ist} equal to one). To understand the effect for industries with the average non-compete share, the coefficient would need to be divided by a factor of about 8. σ_s, η_i, and τ_t, are our state, industry, and time fixed effects, respectively, and $\sigma_s \times \eta_i$, $\sigma_s \times \tau_t$, and $\eta_i \times \tau_t$, are their pairwise interactions. ε_{ist} represents the error term for our data that is structured at the industry, state, and time level. We cluster our standard errors at the industry level given that this is the primary level of

variation in our data. We continue to exclude always treated and policy change states but do not enforce a balanced panel.

Taken together, our methodologies have a host of strengths and weaknesses and varying assumptions that in aggregate can help us identify the impact of Minnesota's non-compete enforcement ban. We explore each of them for our QWI analyses, but we focus only on our primary synthetic differences-in-differences and synthetic control methodologies for our Lightcast analysis due to the noisiness of that data.

Results

The following tables present the results for the models described above, and Appendix A provides the figures that graphically show the patterns underlying each of the estimates. Table 2 presents the effect of Minnesota's non-compete enforcement ban for our new hire earnings outcome. Because the Minnesota ban exempted all non-competes signed prior to July 2023, its early effects may be most visible for labor market outcomes associated with new employment relationships. As shown in Table 2, all estimated effects are statistically insignificant, with most indicating small negative effects.

The estimate for our primary synthetic differences-in-differences model in Column 1 suggests a small, insignificant impact on new hire earnings. The 95 percent confidence interval spans earnings increases or decreases of about 4 percent. The synthetic control methodology in Column 2 suggests a negative point estimate with a wide confidence interval. The triple difference coefficient by industry share instead suggests an increase in new hire earnings, but the confidence interval is again wide. This estimate is for a non-compete share equal to 1, whereas the average industry non-compete share was around 13 percent. Scaling the confidence interval

to apply the estimates to the average industry non-compete share corresponds with estimates of earnings increases of 9 percent or decreases of 5 percent being in the interval. The industry and education breakdowns in Columns 5 through 8 do not show any meaningful heterogeneity in these characteristics, though the negative point estimates when disaggregating by non-compete industry prevalence are marginally significant.

Table 2-
Early Impact of MN Non-Compete Enforcement Ban on New Hire Earnings

	SDID Main Model	Synthetic Control	DDD by Industry Share	SDID Policy States	SDID High NC Ind	SDID Low NC Ind	SDID No College	SDID College
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Coefficient	-0.0124	-0.0325	0.138	-0.0165	-0.0317*	-0.0231*	-0.00652	-0.0226
Standard Error	(0.0146)	(0.0469)	(0.250)	(0.0161)	(0.0177)	(0.0136)	(0.0139)	(0.0258)
Observations	1,702	1,702	33,172	2,116	1,702	1,702	1,702	1,702
95% C.I.	[-.041 ; .0162]	[-.124 ; .0594]	[-.384 ; .66]	[-.0481 ; .0151]	[-.0663 ; .00293]	[-.0498 ; .00368]	[-.0338 ; .0208]	[-.0732 ; .0279]
2022 Mean	8.282	8.282	8.364	8.308	8.585	8.043	8.353	8.674

Notes: Model specifications are as noted in text. Data are from the Quarterly Workforce Indicators. Standard errors (in parentheses). ***p<.01, **p<.05, *p<.10.

Table 3 presents the same set of analyses for earnings overall. As with the earnings for new hires, the estimates are often negative and somewhat imprecise. Because the non-compete ban did not affect enforceability for workers who signed their contracts prior to July 2023, many of the individuals included in this overall earnings sample may not have been directly affected by the non-compete enforcement ban. However, to the extent that firms try to equalize earnings across new hires and existing hires, the non-compete ban could be placing pressure on earnings for existing hires as well (Gottfries and Jarosch 2025). The results from Table 3 could thus be interpreted as a type of control for general Minnesota earnings trends (if the policy affected only new hires) or as a spillover from the policy itself (either to workers with existing non-competes

or workers without non-competes). As more data become available and more individuals are hired after July 2023 in the sample, the interpretation of the results may become clearer.

Table 3-
Early Impact of MN Non-Compete Enforcement Ban on Overall Earnings

	SDID Main Model	Synthetic Control	DDD by Industry Share	SDID Policy States	SDID High NC Ind	SDID Low NC Ind	SDID No College	SDID College
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Coefficient	-0.0101	-0.00702	0.00683	-0.0111	-0.00608	-0.0145	-0.00451	-0.00835
Standard Error	(0.00829)	(0.0430)	(0.113)	(0.0123)	(0.00928)	(0.0118)	(0.00763)	(0.0138)
Observations	1,702	1,702	33,311	2,116	1,702	1,702	1,702	1,702
95% C.I.	[-.0263 ; .00617]	[-.0913 ; .0773]	[-.229 ; .243]	[-.0352 ; .013]	[-.0243 ; .0121]	[-.0377 ; .00869]	[-.0195 ; .0105]	[-.0353 ; .0186]
2022 Mean	8.683	8.683	8.680	8.704	8.907	8.362	8.619	9.070

Notes: Model specifications are as noted in text. Data are from the Quarterly Workforce Indicators. Standard errors (in parentheses). ***p<.01, **p<.05, *p<.10.

Table 4 shows the results of the non-compete enforcement ban on the short-term separation rate of new hires. On average, about half of new hires leave their jobs by or during the next quarter. Although many of these individuals may be temporary or seasonal workers, the ban on non-compete enforcement could make early transitions easier for some permanent workers. Overall, we see generally positive point estimates, wide confidence intervals, and limited heterogeneity in results for this outcome. The confidence intervals include increases in new hire separation rates at least as high as 2 percentage points across the specifications. The positive coefficient in the triple difference specification is marginally significant, suggesting about a 1 percentage point increase in new hire separation rates. For context, the mean separation rate in 2022 was roughly 50 percent.

Table 4-
Early Impact of MN Non-Compete Enforcement Ban on New Hire Separations

	SDID Main Model	Synthetic Control	DDD by Industry Share	SDID Policy States	SDID High NC Ind	SDID Low NC Ind	SDID No College	SDID College
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Coefficient	0.00474	-0.00386	0.0918*	0.00410	0.00377	0.00558	0.00581	0.0107
Standard Error	(0.00545)	(0.0109)	(0.0526)	(0.00593)	(0.00908)	(0.00742)	(0.00573)	(0.00779)
Observations	1,702	1,702	32,954	2,116	1,702	1,702	1,702	1,702
95% C.I.	[-.00595 ; .0154]	[-.0252 ; .0175]	[-.0183 ; .202]	[-.00752 ; .0157]	[-.014 ; .0216]	[-.00897 ; .0201]	[-.00543 ; .017]	[-.0046 ; .026]
2022 Mean	0.527	0.527	2.669	0.522	0.401	0.562	0.513	0.456

Notes: Model specifications are as noted in text. Data are from the Quarterly Workforce Indicators. Standard errors (in parentheses). ***p<.01, **p<.05, *p<.10.

Table 5 shows the results for overall turnover. As with overall earnings, it is not obvious whether the overall turnover results should be considered a type of control or if they capture spillover effects to workers whose non-competes are not directly affected by the policy and/or to workers without non-competes. In the table, we see negative point estimates for all specifications, though the confidence intervals are all sufficiently wide to include increases in turnover. The average 2022 turnover rate was roughly 10 percent, so the confidence intervals usually include increases of at least 1 percent of that base rate.

Table 5-
Early Impact of MN Non-Compete Enforcement Ban on Turnover

	SDID Main Model	Synthetic Control	DDD by Industry Share	SDID Policy States	SDID High NC Ind	SDID Low NC Ind	SDID No College	SDID College
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Coefficient	-0.00179	-0.00310	-0.00215	-0.00116	-0.000949	-0.00294**	-0.00156	-0.00240
Standard Error	(0.00202)	(0.00228)	(0.0185)	(0.00226)	(0.00334)	(0.00146)	(0.00191)	(0.00255)
Observations	1,665	1,665	32,217	2,070	1,665	1,665	1,665	1,665
95% C.I.	[-.00574 ; .00217]	[-.00757 ; .00138]	[-.0408 ; .0365]	[-.00558 ; .00326]	[-.0075 ; .00561]	[-.00579 ; .000]	[-.00531 ; .0022]	[-.00741 ; .0026]
2022 Mean	0.105	0.105	0.100	0.104	0.0813	0.131	0.0916	0.0812

Notes: Model specifications are as noted in text. Data are from the Quarterly Workforce Indicators. Standard errors (in parentheses). ***p<.01, **p<.05, *p<.10.

Next, Table 6 presents our results examining the Lightcast job postings data. These data help us look under the hood to see whether we can more directly observe changes in how employers are using non-compete agreements after the enforcement ban. We view this analysis as complementary to the analysis above because it speaks to the channel that presumably mediates the effect of Minnesota's policy on labor market outcomes. For the policy to have any effect, it must induce changes in employer and/or employee behavior. The text of job postings gives us an opportunity to see employer-side behavior that would plausibly change in the aftermath of the policy.

The postings data are noisy, as clearly observable in Appendix Figure 5, so the estimates are quite imprecise. Still, the table and figures demonstrate that the text of job postings in Minnesota has not dramatically changed in response to the enforcement ban. The point estimates, however, generally go in the expected directions: non-competes are generally less likely to be mentioned after July 2023, but postings specifying that no non-compete is required have increased, and postings that likely include a non-compete requirement declined. The results are suggestive of some but not complete change in how employers mention non-compete contracts in job postings. If employer recruiting strategies are changing on a similar timeline, it may take time before we can observe the broader impacts of the non-compete enforcement ban.

Table 6-
Early Impact of MN Non-Compete Enforcement Ban on Non-Compete Mentions in Job Postings

	Any Mention SDID	Any Mention SC	No Non-Compete SDID	No Non-Compete SC	Competitor NC SDID	Competitor NC SC	Other Non-Compete SDID	Other Non-Compete SC
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Coefficient	-0.265	-0.328	0.508	0.435	-0.000993	-0.0832	-0.675	-0.530
Standard Error	(3.738)	(1.221)	(1.101)	(1.137)	(0.437)	(0.506)	(1.043)	(0.702)
Observations	5,928	5,928	5,928	5,928	5,928	5,928	5,928	5,928
95% C.I.	[-7.59 ; 7.06]	[-2.72 ; 2.07]	[-1.65 ; 2.66]	[-1.79 ; 2.66]	[-.857 ; .855]	[-1.08 ; .909]	[-2.72 ; 1.37]	[-1.91 ; .847]
2022 Mean	19.90	19.90	0.702	0.702	17.87	17.87	1.332	1.332

Notes: Model specifications are as noted in text. Data are from Lightcast. Standard errors (in parentheses).
***p<.01, **p<.05, *p<.10.

Thus far, our results have failed to detect an early impact of Minnesota 2023 non-compete enforcement ban. As discussed above, this could be because we lack sufficient post-treatment data to form precise estimates, or because labor market participants are still adapting to the policy change and effects will take longer to occur. But it could also be that other events are uniquely affecting the Minnesota labor market at the same time as the policy change, making it challenging to identify effects. It is not possible to observe all such factors, but we can test to see if certain indicators of Minnesota's population and workforce changed starting in 2023. To do this, we use data from the Current Population Survey to assess whether Minnesota saw jumps in demographic characteristics such as age, sex, race, ethnicity, and nativity, in population traits relevant to workforce participation such as college-level educational attainment or presence of a disability, or in workforce attributes such as the share of individuals working remotely. Table 7 presents the results of such analyses using our primary synthetic differences-in-differences methodology. The table shows that most of these variables saw small and statistically insignificant changes after 2023 in Minnesota, relative to the weighted group of comparison states. The only exception is share with a college degree, where Minnesota's average was about 2

percentage points smaller than the synthetic group after 2023. The table suggests that it's unlikely that Minnesota experienced large changes in certain factors that could be masking the effects of the non-compete enforcement ban, but it's still possible that labor market conditions we are not able to measure could be influencing the results.

Table 7-
Population Characteristic Placebo Tests for MN's Non-Compete Enforcement Ban

	Age	Share Male	Share White	Share Hispanic	Share Citizen	Share College	Share Disability	Share Remote
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Coefficient	0.105	0.00125	-0.00121	-0.00778	0.00695	-0.0175*	0.0125	-0.0228
Standard Error	(0.172)	(0.00133)	(0.00697)	(0.0103)	(0.00633)	(0.00983)	(0.00791)	(0.0184)
Observations	5,889	5,889	5,889	5,889	5,889	5,889	5,889	1,794
95% C.I.	[-.233 ; .443]	[-.00135 ; .00385]	[-.0149 ; .0124]	[-.0279 ; .0124]	[-.00546 ; .0194]	[-.0367 ; .00181]	[-.00298 ; .028]	[-.0587 ; .0132]
2022 Mean	48.39	0.488	0.787	0.109	0.943	0.338	0.135	0.171

Notes: Model specifications are as noted in text. Data are from the Current Population Survey for adults over age 18. Standard errors (in parentheses). ***p<.01, **p<.05, *p<.10.

Discussion and Conclusion

In this research brief, we examine the early impacts of Minnesota's 2023 non-compete enforcement ban. We generally find null effects of the policy across methodologies and outcomes studied. However, the confidence intervals often include small to moderate positive impacts of the policy on earnings and mobility. Table 8 describes the existing literature on the labor market impacts of non-compete enforceability. Although many of the papers are able to detect effects in about a year, two closely related studies find that it can take about three years for the effects to be detectible in the data (Johnson et al. 2024; Lipsitz and Starr 2022). Moreover, many of the earnings and mobility effects uncovered in the literature fall within the confidence intervals estimated in our study. We thus may require more time and data to uncover the effects of Minnesota's non-compete enforcement ban.

Table 8-
Empirical Literature on Labor Market Impacts of Non-Compete Enforceability

	State's Policies Studied	Groups Affected	Policy Scope	Policy Direction	Impact on Wages	Impact on Mobility	Timing of Detectible Impacts
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Johnson et al. (2024)	Multiple	All workers	Often retroactive (many judicial)	Increase in enforceability	-1.7% moving from 25th-75th percentile	-3.8% within industry moving from 25th-75th percentile	2 to 3 years
Lipsitz and Starr (2021)	Oregon	Hourly workers	New contracts	Decrease in enforceability	+2-3%	+0.4p.p. or +17.3% in monthly mobility	3 years (mobility sooner)
Balasubramanian et al. (2022)	Hawaii	Tech workers	New contracts	Decrease in enforceability	+4.2% new hire; +0.7% overall	+11% separation rates	1 year
Marx et al. (2009)	Michigan	All workers (study inventors)	Retroactive after 2 years	Increase in enforceability	-	-8.1% mobility	1 year
Jeffers (2023)	Multiple	All workers	Often retroactive (many judicial)	Increase in enforceability	-	-7-11% departure rates for college grads	1 year
Garmaise (2011)	Multiple	All workers (study excecutives)	Often retroactive (many judicial)	Increase in enforceability	-8.2%	-47% within industry transfers	-
Kini et al. (2021)	Multiple	All workers (study CEOs)	Often retroactive (many judicial)	Increase in enforceability	+3-5%	-	1 year
This study	Minnesota	All workers	New contracts	Decrease in enforceability	TBD	TBD	TBD

Note: Table excludes studies using cross-sectional variation in non-compete enforceability or those exclusively examining non-labor market outcomes, as well as studies that are theoretical rather than empirical in nature.

Understanding the impacts of Minnesota's non-compete enforcement ban has implications for public policy and for our broader understanding of labor market functioning. A positive effect would be most aligned with the literature on non-compete enforceability and its labor market effects. Moreover, delay in the appearance of that effect might also provide insight into whether the non-retroactive nature of Minnesota's policy influenced its pattern of effects. For example, it could be that the impact of non-compete enforceability becomes more salient as

more workers become covered by the law.[3] Such a result would run counter to some existing research on non-compete enforceability but could be consistent with research studying job mobility in other contexts (Balasubramanian et al. 2022; Marx et al. 2009; Townsend and Allan 2024). A true null result, or a negative result, would suggest other possibilities. If labor market effects of non-competes are largely mediated by the existence of the contracts themselves rather than their legal enforceability, a policy change like Minnesota's might not leave a labor market footprint (Starr et al. 2020).

A negative earnings effect of banning non-compete enforcement would be inconsistent with most, if not all, of the prior empirical literature. However, it would be consistent with the presence of a compensating differential: In a setting where non-competes are freely negotiated, employers would need to offer higher initial earnings in exchange for a non-compete. Prior empirical studies have generally not found such an effect, attributing this to the lack of information on the part of workers and the rarity of reported bargaining over non-competes (Lipsitz and Starr 2022).

More data will help us make progress on some of the open questions left by this analysis, but additional research on non-competes may be required to answer all of them. This analysis examines only one state at a point in time. Although our study employs current state of the art methods to analyze a modern one-of-a-kind policy change, it is possible that a common shock to high non-compete industries in Minnesota or something unique about the Minnesota labor market is influencing our results. How non-compete enforceability influences labor market outcomes as a whole may require more research in other contexts. Still, the analysis presented

[3] Note that the lack of retroactivity in Minnesota's policy is shared by some other recent changes in enforceability, notably in Oregon's 2008 policy (Lipsitz and Starr 2022).

here helps to build understanding of how non-compete enforceability shapes labor market outcomes.

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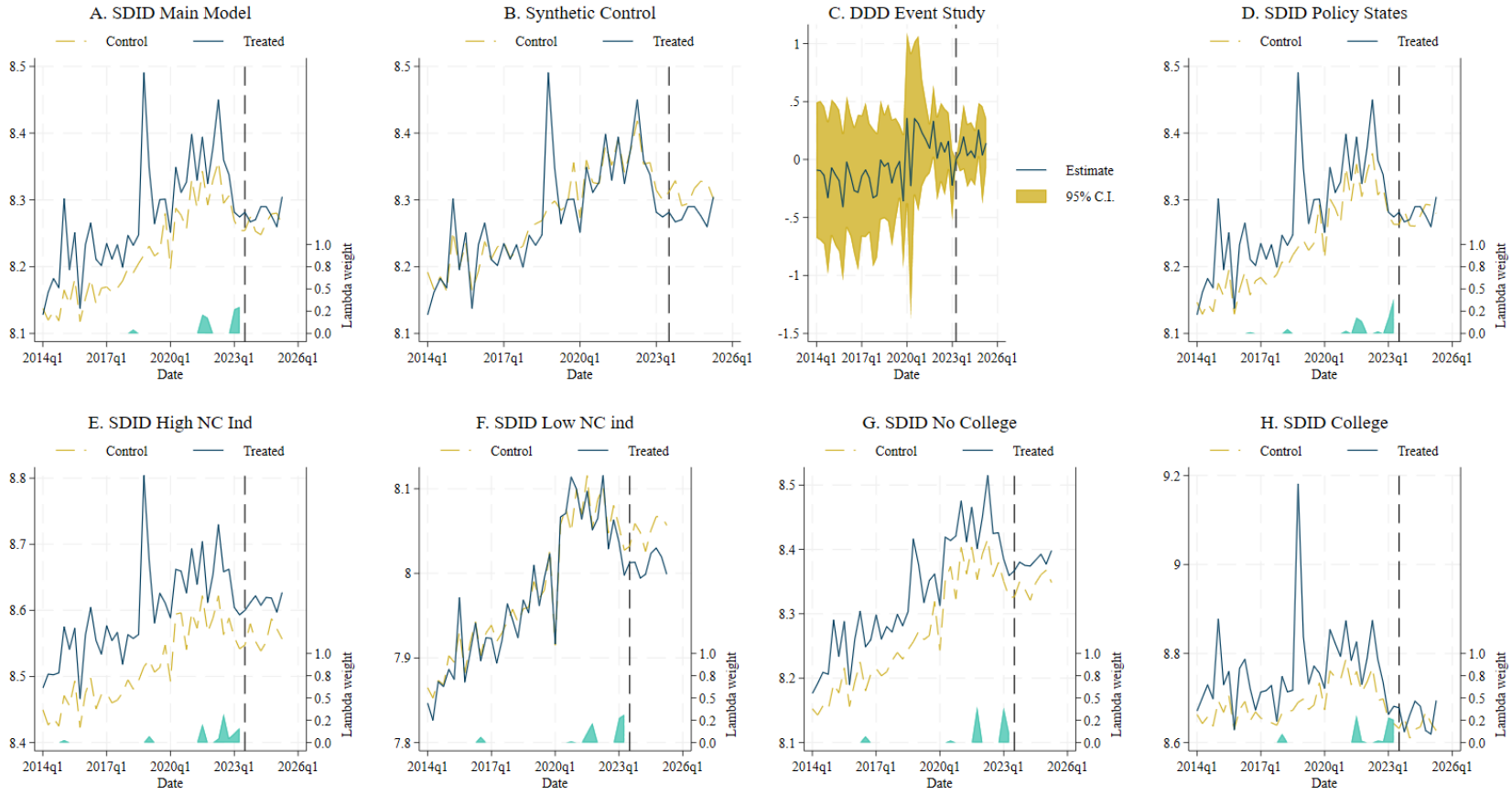
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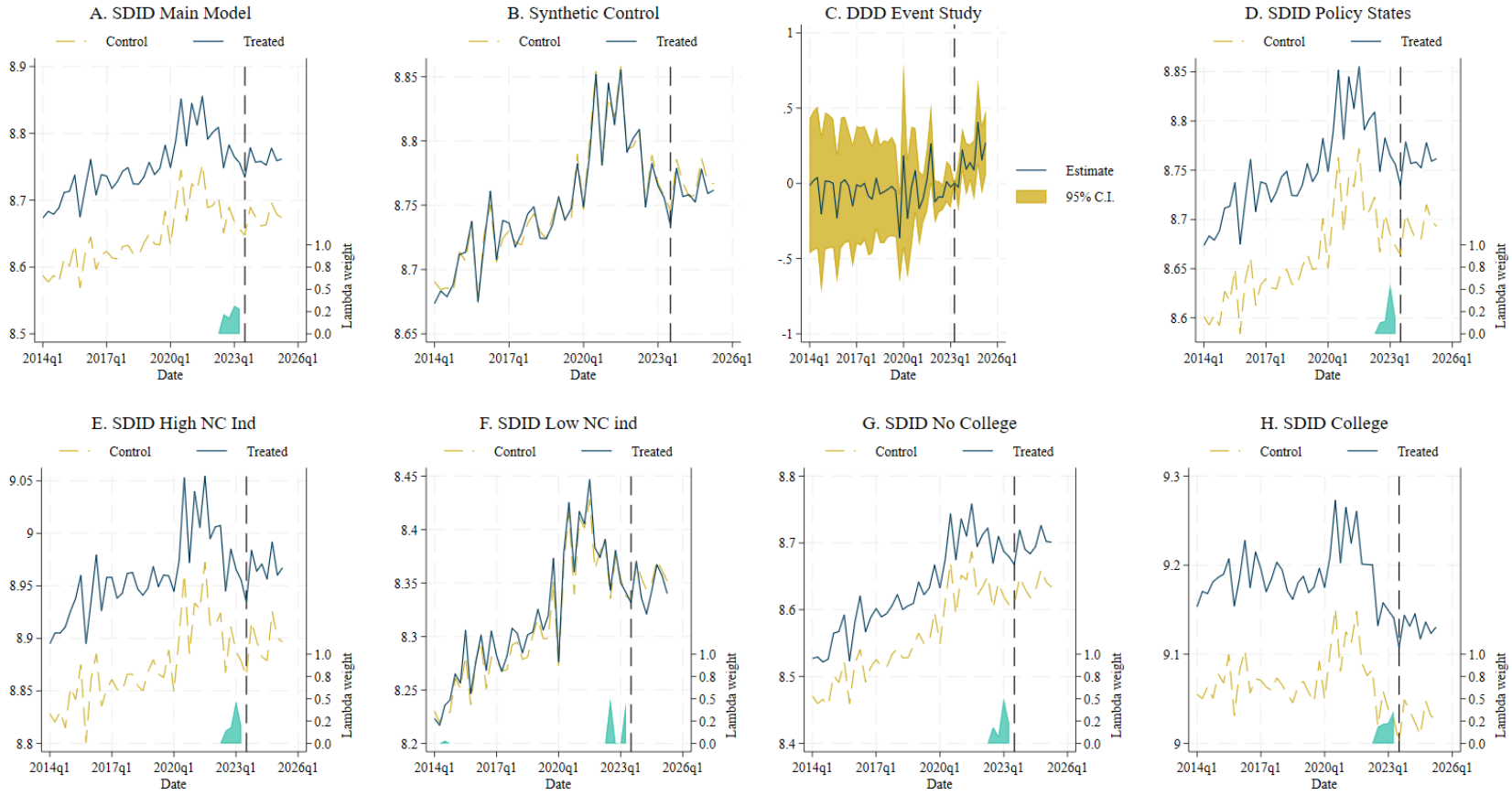
Appendix A. Additional Figures

Appendix Figure 1. Graphical Results for Early Impact on New Hire Earnings



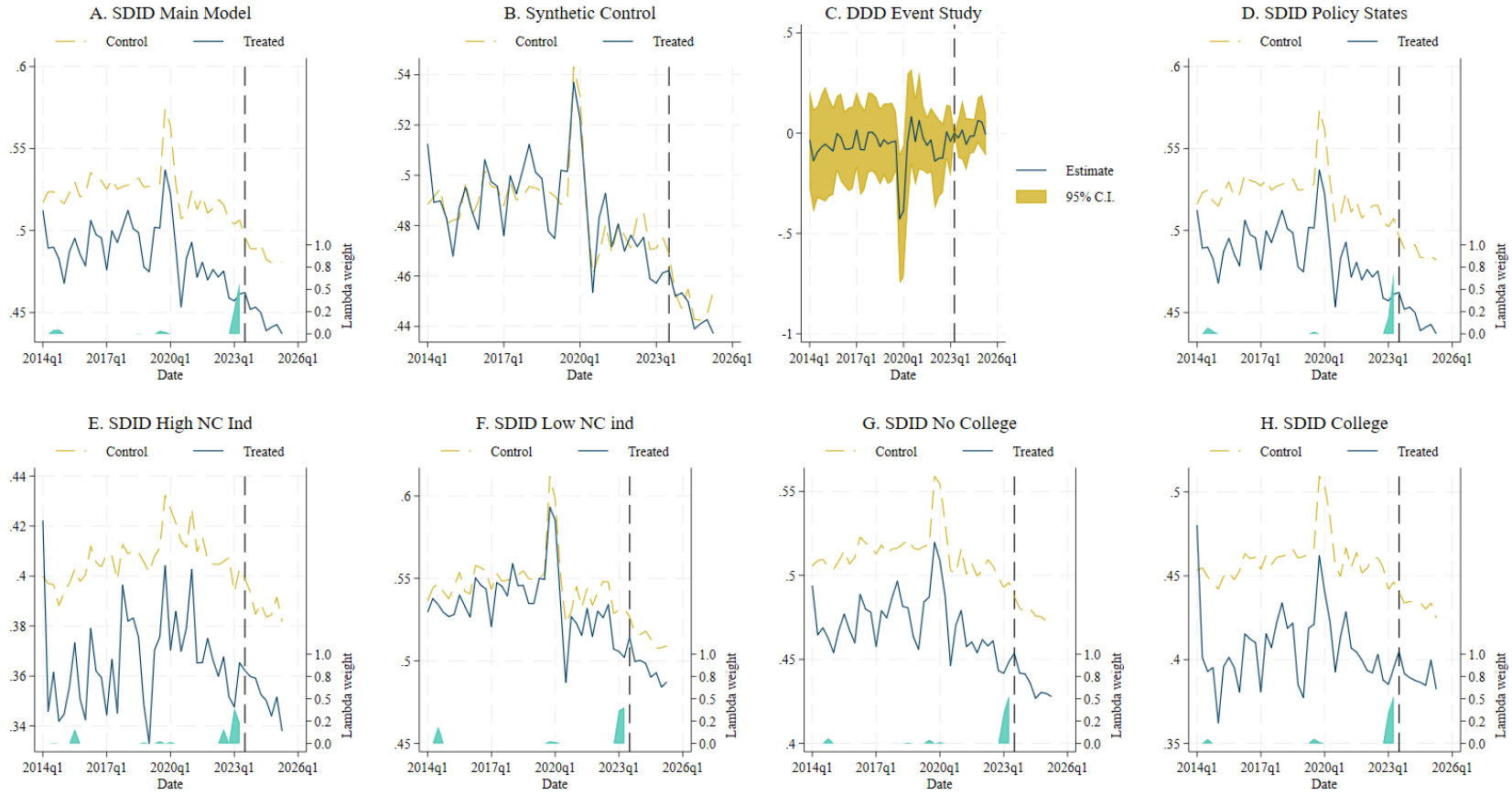
Notes: Model specifications are as noted in text. Data are from the Quarterly Workforce Indicators.

Appendix Figure 2. Graphical Results for Early Impact on Overall Earnings



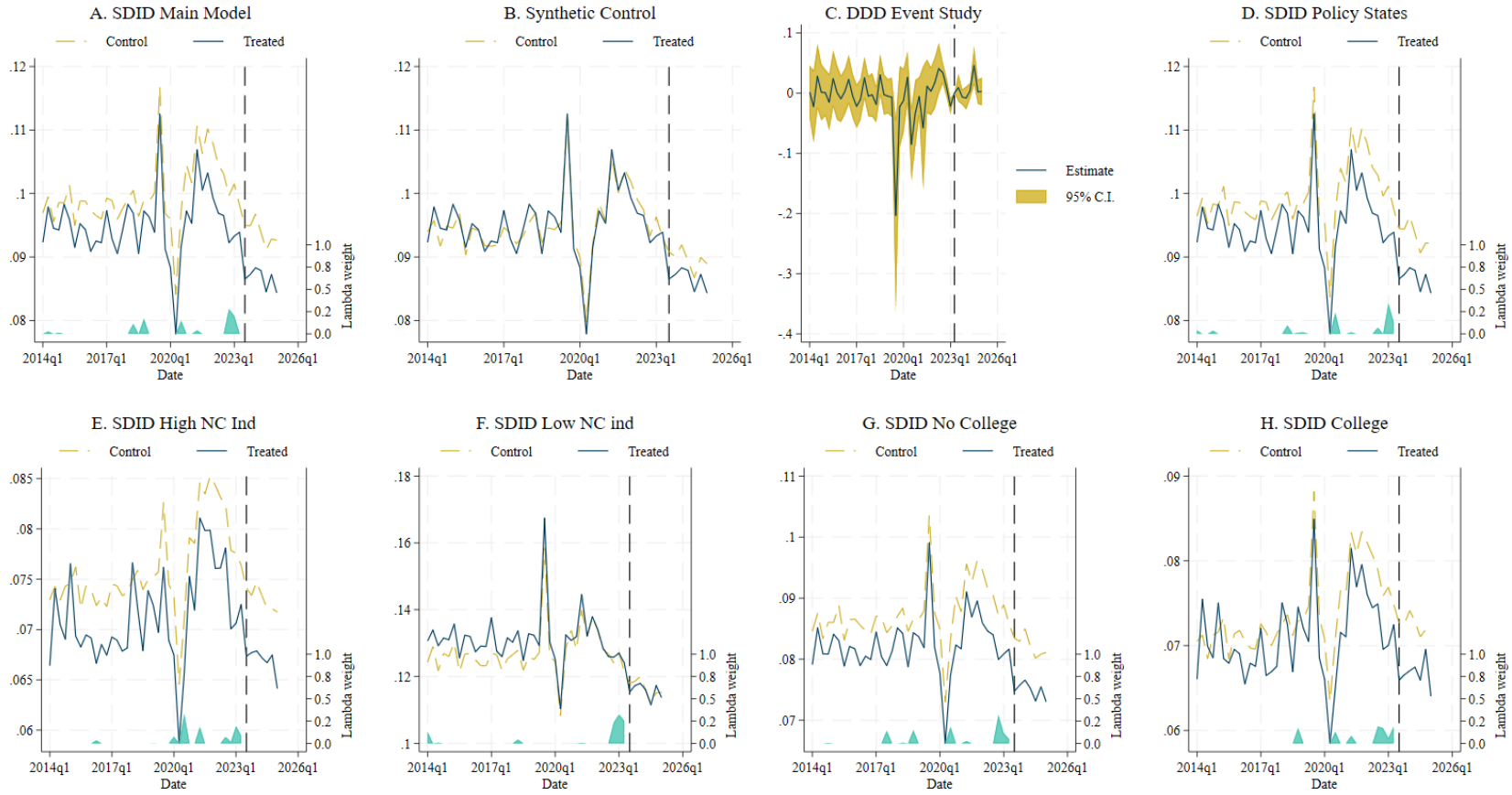
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Appendix Figure 3. Graphical Results for Early Impact on New Hire Separations



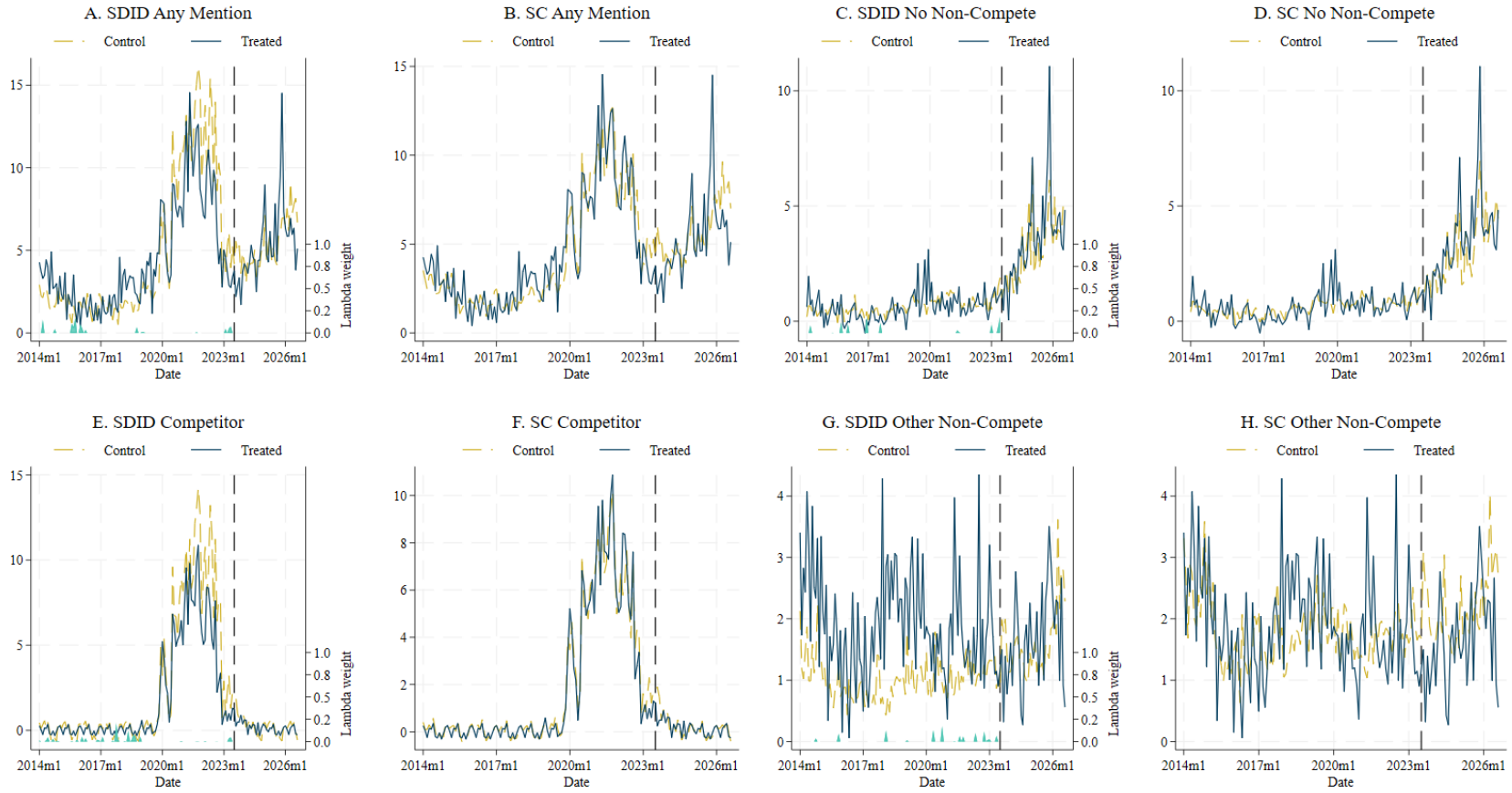
Notes: Model specifications are as noted in text. Data are from the Quarterly Workforce Indicators.

Appendix Figure 4. Graphical Results for Early Impact on Turnover



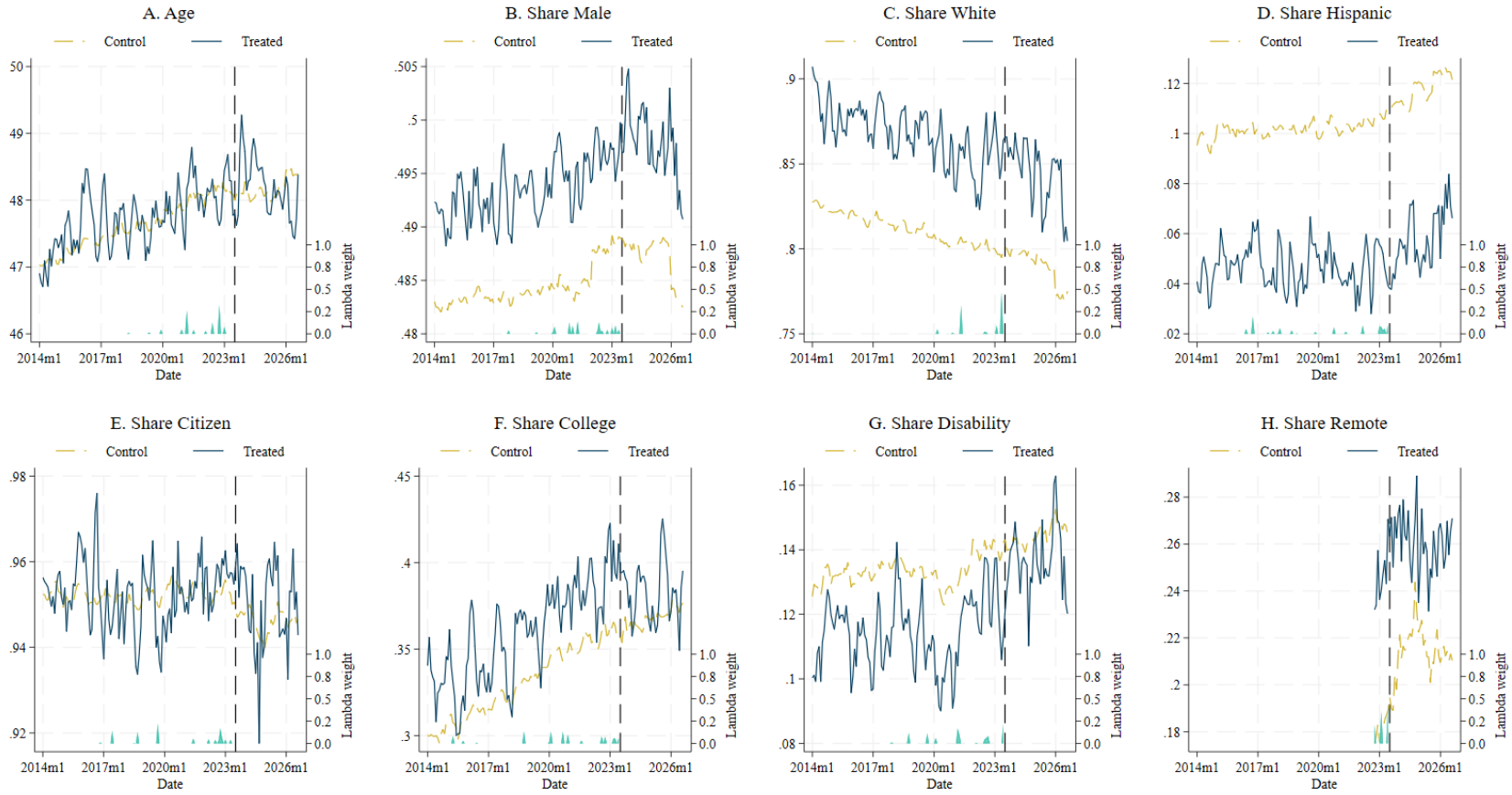
Notes: Model specifications are as noted in text. Data are from the Quarterly Workforce Indicators.

Appendix Figure 5. Graphical Results for Early Impact on Job Posting Outcomes



Notes: Model specifications are as noted in text. Data are from Lightcast.

Appendix Figure 6. Graphical Results for Early Impact on Population Characteristics



Notes: Model specifications are as noted in text. Data are from the Current Population Survey.

Appendix B. Data Construction Details

Our QWI variables have the following definitions and construction details:

- Earnings for new hires (EarnHirNS)
    - Average monthly earnings of newly stable employees (i.e., full-quarter employees who were new hires with a firm in the previous quarter)
    - Lagged to align with quarter of hire
    - Logged for ease of interpretation
- Earnings for all workers (EarnS)
    - Average monthly earnings of employees with stable jobs (i.e., worked with the same firm throughout the quarter)
    - Lagged to align with quarter of potential hire
    - Logged for ease of interpretation
- Separation rate for new hires (1- HirNS_lagged/HirN)
    - Estimated number of workers who started a job that they had not held within the past year, and the job turned into a job that lasted at least a full quarter with a given employer / estimated number of workers who started a job that they had not held within the past year
    - Lagged stable variable to align with quarter of hire
- Turnover (TurnOvrS)
    - Rate at which stable jobs begin and end calculated by summing the number of stable hires in the reference quarter and stable separations in the next quarter, and dividing by the average full-quarter employment
    - Lagged to align with quarter of potential hire

Our Lightcast variables have the following definitions and construction details:

- Any non-compete mention in posting
    - non-compete, noncompetition, non-competition, not to compete, noncompete, restrictive covenant, postemployment restraint
    - Exclude HR and legal occupations
    - Scale by total postings x 10000
- Specifies no non-compete required
    - no [phrase], no[phrase], without [phrase], zero [phrase], [phrase]: no, [phrase]:no, or look 40 characters back and see if one of the following phrases shows up: do not, does not, don't, doesn't
    - Scale by total postings x 10000
- Related to competitor non-compete
    - must not be subject to (noted anywhere in sentence with phrase), have you signed a [phrase], do you have a [phrase], not bound by a [phrase], enforceable [phrase]
    - Scale by total postings x 10000
- All other non-compete postings
    - Roughly corresponds to those postings stating job requires signing non-compete
    - Scale by total postings x 10000