The Differences in Preventing Nurse Burnout

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Abstract

I investigate whether state laws that restrict mandatory overtime for nurses affect registered nurse outcomes. I develop a simple model of nurse burnout that illustrates how reducing burnout, in theory, increases nurse income by increasing tenure length and thus wages. In a difference-in-differences framework using ACS data, I test this hypothesis by estimating the laws' effect on average income, wage, and hours worked among nurses. I estimate that these laws increase total income and hourly wage for registered nurses by 6.4 percent and 5.5 percent respectively, with no significant effect for time outcomes. These results suggest that reducing nurse burnout increases worker income through increased tenure and wage growth, even without an increase in time worked.

I Introduction

It is well documented that healthcare workers in general, and registered nurses in particular, often suffer from "burnout": a loss of motivation and reduced commitment to one's occupation (Dall'Ora et al., 2020). In some recent estimates, more than half of nurses experience at least a moderate level of burnout (Kelly, Gee and Butler, 2021). The reasons that each nurse experiences burnout are, by nature, personal, but there are some common threads. Some of the main reasons nurses give for burnout include shift patterns, inherent psychological demand, poor work environment, emotional exhaustion, and depersonalization (Getie et al., 2025; Dall'Ora et al., 2020). Perhaps the most common reason for burnout is low personal accomplishment (Getie et al., 2025). There are also correlations between burnout and non-work related attributes of the nurses. These include being younger, being male, not having children and being single (Li et al., 2024) This phenomenon does not just affect nurses; it impacts everything about the healthcare system. High nurse burnout is associated with lower safety culture, more frequent infections, more frequent patient falls, more missed medications, and more missed care (Li et al., 2024).

The culmination of burnout problems for the nursing profession is turnover, something that affects both the nurses that quit and the healthcare systems that lose skilled workers. Approximately one third of nurses leave their place of employment within one to two years of starting their job (Unruh and Zhang, 2014). While there might be many reasons a nurse leaves their position, burnout is a significant factor. Increases in emotional exhaustion, an aspect of burnout, significantly increase nurse turnover (Kelly, Gee and Butler, 2021). Turnover is a very costly event, costing as much as $90,000 per nurse, in addition to disrupting patient care (Halter et al., 2017). The cost has prompted efforts by hospitals and other healthcare organizations to try to decrease burnout by creating programs to build resilience and reduce stress (Kelly, Gee and Butler, 2021).

In this paper I explore the cost of burnout for the nurses themselves. I investigate the effects of decreasing mandatory overtime on nurse outcomes, most importantly time and wage outcomes. We know from Getie et al. (2025) that schedule is a driver in nurse burnout, so decreasing additional required work hours has the potential to decrease nurse burnout and thus nurse turnover. Nurses already experience burnout at high levels, and additional required hours likely exacerbate the crisis.

I develop a simple burnout model in which nurses begin with a finite emotional reserve. As they work, mandatory overtime depletes more emotional reserves than normal work hours, which depletes a nurse's supply. When the reserve falls below a certain level, the nurse quits and starts a new job at a different location. Over a finite time horizon the model shows that the more often a nurse has to quit and restart at another healthcare facility, the lower their income will be. The model predicts that decreasing burnout increases income in the long-run, even when total hours worked does not change. In my empirical section, I utilize the staggered adoption of laws and regulations at the state level that ban mandatory overtime for nurses to investigate the effect of such laws on nurse income and work hours. In a canonical difference-in-differences model using data from the American Community Survey (ACS), I estimate that these laws increase total nurse take-home pay and hourly wage. Additionally, I find no evidence that these regulations significantly affect nurses' time worked. However, prior work by Sun and Abraham (2021) and Goodman-Bacon (2021) shows that with variation in treatment timing, the canonical two-way fixed effects estimator can be biased because it implicitly assigns negative weights to some cohort-specific treatment effects. Because my treatment timing is staggered and I allow for heterogeneous treatment effects across time and space, this initial specification is likely biased.

I instead use the difference-in-differences estimator developed by Callaway and Sant'Anna (2021), which identifies group-time average treatment effects on the treated (ATTs) using never-treated states in addition to not-yet-treated states as comparison units where available and relies on a conditional parallel trends assumption. Using this estimator, I find that the laws increase nurse total income by an aggregated ATT of 6.4 percent for nurses in treated states. This appears to be driven primarily by wage growth rather than changes in time worked, as I estimate no significant effects on hours per week, weeks per year, or total annual hours for nurses in treated states, whereas I find an aggregate ATT of 5.5 percent for hourly wage. The validity of these estimates hinges on the conditional parallel trends assumption, which requires that, absent treatment, trends for treated units would have evolved similarly to never-treated units conditional on covariates. The event-study results for the outcomes of income, hourly wage, and time worked show some statistically significant differences in the early pre-treatment periods, suggesting long-run differential trends across states. However, in the years immediately preceding treatment, the pre-treatment coefficients are small in magnitude, and most are statistically indistinguishable from zero, which provides more relevant support for the parallel trends assumption in the short-run. Another concern is that in my sample nurses in never-treated states differ significantly from nurses in not-yet-treated states in age, race, and marital status conditional on year. In order to try to handle this difference, I also report estimates using inverse probability weights along with my covariates to make the conditional parallel trends assumption more plausible.

There is a small but convincing literature on measuring the effect of state laws and regulations that prohibit mandatory overtime for nurses. These papers focus on two main questions. First, how do these laws affect the quality of nursing care? Lu and Lu (2016) shows that these mandatory overtime laws decreased quality of care at nursing homes, as nursing homes shifted from regular full-time nurses to contract nurses to meet staffing needs in the face of the laws. Similarly, Dong (2022) shows that contract nurses became more common, and that these policies caused an increase in Medicare patients using nursing home facilities. Second, how did such regulations impact the hours that nurses ended up working? The aforementioned papers, along with Bae and Yoon (2014) and Bae, Brewer and Kovner (2012) show that these laws were in fact effective at decreasing mandatory overtime. Although measured mandatory overtime did decrease, my paper adds to the nuance of this result. I show that there was no impact on the overall average hours worked by nurses, confirming what Bae and Yoon (2014) found: mandatory overtime hours decreased, but overall average nurse hours stayed constant as nurses increased voluntary overtime. The literature is relatively silent on outcomes for nurses besides hours worked. I fill part of that void by investigating the laws' impact on nurse income and wages. More broadly, my paper adds to a literature on the impacts of overwork. Berniell and Bietenbeck (2020) shows that reducing weekly work hours from 39 to 35 decreased smoking and BMI among French workers. Similarly using Danish data, Hummels, Munch and Xiang (2025) finds that increases in workload resulted in higher instances of depression, stress, heart disease, and strokes. When it comes to burnout specifically, Nekoei, Sigurdsson and Wehr (2024) finds that burnout permanently decreases earnings. My findings support this result by showing that decreased mandatory overtime, which likely reduces burnout, increases earnings.

II Model of Nurse Burnout

I develop a model of nurse burnout in the spirit of Maestas and Li (2007) to show the potential mechanisms through which burnout reduction increases income. This model is intended to highlight those mechanisms rather than capture the equilibrium wage determination of nurses.

II.A Model Set-Up

Suppose a nurse works for T periods, where each period can be thought of as a week. Each nurse starts with an emotional reserve $E_0 = R$ where $R \in \mathbb{N}$. For every nurse there exists an E_c such that if $E_t < E_c$, the nurse immediately quits. Every week, each nurse gains or loses additional emotional reserves:

$$A_t(s_t, m_t, v_t) = \gamma - s_t - \alpha m_t - \beta v_t \tag{1}$$

Where s_t is the number of normal shifts worked at week t, m_t is the number of mandatory overtime shifts the nurse works at week t and v_t is the number of voluntary overtime shifts the nurse works at week t. s_t and m_t are chosen by the hospital, and v_t is chosen by the nurse. γ is the additional emotional reserve the nurse gets from leisure every week, and α and β are emotional costs of overtime shifts with $\alpha > \beta$. The nurse's emotional reserves at week t are then given by:

$$E_t(s_t, m_t, v_t) = A_t + E_{t-1}$$

that is

$$E_t(s_t, m_t, v_t) = \gamma - s_t - \alpha m_t - \beta v_t + E_{t-1} \tag{2}$$

Wages at week t are given by:

$$w_t(s_t, m_t, v_t) = w_t^s s_t + w_t^m m_t + w_t^v v_t \tag{3}$$

Where

$$w_t^s = \begin{cases} \eta w_{t-1}^s, & \text{if } E_{t-1} \geq E_c \\ w_0^s, & \text{otherwise} \end{cases} \qquad w_t^m = \begin{cases} \theta w_{t-1}^m, & \text{if } E_{t-1} \geq E_c \\ w_0^m, & \text{otherwise} \end{cases} \qquad w_t^v = \begin{cases} \theta w_{t-1}^v, & \text{if } E_{t-1} \geq E_c \\ w_0^v, & \text{otherwise} \end{cases}$$

η and θ are greater than 1, and $\theta > \eta$. A nurses total wages by T is thus

$$W(T) = \sum_{t=1}^{T} w_t \tag{4}$$

II.B Algebraic exploration

Assume a nurse faces the same schedule every week: $s_t = \tilde{s}, m_t = \tilde{m}, v_t = \tilde{v}$. Also assume that $A_t \leq 0$, so that nurses steadily deplete their emotional reserve without replenishment. Let τ be the tenure at the current job for the nurse. Tenure at a job is then

$$\tau = \lfloor \frac{E_0 - E_c}{\tilde{s} + \alpha\tilde{m} + \beta\tilde{v} - \gamma} \rfloor$$

τ is increasing in E_0 and γ, and is decreasing in E_c, $\tilde{s}$, $\tilde{m}$, $\tilde{v}$, β, α. Since $\tilde{s}, \tilde{m}, \tilde{v}$ are constant, per period wages can be given by $w_t = \tilde{s}w_0^s\eta^{t-1} + \tilde{m}w_0^m\theta^{t-1} + \tilde{v}w_0^v\theta^{t-1}$. Total cumulative wages during τ are

$$W(\tau) = \sum_{t=1}^{\tau} w_t = \tilde{s} w_0^s \sum_{t=1}^{\tau} \eta^{t-1} + (\tilde{m} w_0^m + \tilde{v} w_0^v) \sum_{t=1}^{\tau} \theta^{t-1}$$

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Suppose the nurse quits at the beginning of the week if $E_t < E_c$ will occur sometime throughout the week and re-hiring is instantaneous. Assume the nurse is rehired at a different hospital at the original base wage. Additionally assume the nurse's emotional reserves are reset to E_0. Suppose for a given nurse this happens $n-1$ times, so there are n different τ periods of total time T. Because $W(\tau)$ is a convex function of τ when $\eta > 1$ or $\theta > 1$, splitting a given total working time T into shorter interrupted spells reduces cumulative earnings:

$$\sum_{i=1}^{n} W(\tau_i) < W\left(\sum_{i=1}^{n} \tau_i\right).$$

Intuitively, each interruption resets the multiplicative growth of w_t^s, w_t^m, and w_t^v to their initial values. Continuous tenure allows compounding to operate over a longer horizon, producing a disproportionately higher total income even when total weeks worked are identical. Since τ is decreasing in $\tilde{m}$, reducing mandatory overtime increases tenure, increasing their wages and thus accumulating more income by time T. Ultimately the model predicts that reducing mandatory overtime increases tenure and cumulative earnings, even if total hours remain constant.

III Mandatory Overtime Laws Background

Although there is no law at the federal level that prohibits mandatory overtime for healthcare workers, from 2001 to 2012, 18 different states passed laws or created regulations prohibiting overtime requirements for nurses (Healthy Work Campaign, 2020; Lu and Lu, 2016). These 18 states still currently have these restrictions in place. As seen in Figure 1, the states with overtime bans are relatively spread across the country, although there are none in the mountain west and only one in the south. They represent four out of the five most populous states: Texas, California, New York, and Pennsylvania. Although all of these states have sought to ban overtime in some way, two out of the 18 have regulations instead of outright laws: California and Missouri (Lu and Lu, 2016). The motivation for the mandatory overtime laws was observations about nurse stress, fatigue and burnout, but simply banning extra required shifts was not the only strategy. In addition to provisions making mandatory overtime illegal, most states added rules concerning acceptable work conditions for nurses. For example, Illinois requires that employers allow at least eight hours between shifts, and California prohibits working more than 12 hours in a 24-hour period (Lu and Lu, 2016). Additionally, almost all have exceptions for emergencies, although there are still some restrictions attached in those cases, such as not being required to work more than 16 hours in a 24-hour time period.

IV Data

I use data from the American Community Survey (ACS) to estimate the impact of these state level laws (Ruggles et al., 2025). Unlike other papers that use specialized nurse survey data and data from care center facilities, I use these broad samples to get a more generalized view of the nursing population and the effects of the laws on it. This also allows me to use nurses in all treated states in the sample, and utilize those in all never-treated states as a counterfactual. The ACS is intended to capture broad demographic, housing, social, and economic data. It surveys a large sample of households, with more than 1.3 million households responding in later years. These data are gathered on a rolling basis, so data for the individuals in the survey includes information from the year leading up to the survey event. So if someone received the survey in August 2005, their income is reported from August 2004 to August 2005.

The ACS has been performed nationally by the US Census Bureau since 1999, although during 1999-2004 questions and methods were still being refined. The final state to adopt a law prohibiting mandatory overtime for nurses was Massachusetts in 2012, and the first states to adopt these laws (California and Maine) did so at the beginning of 2001. I use ACS data from 2000-2016. This allows me to capture a pre-period for Maine and California as well as access medium- to long-term impacts for New York, Alaska, and Massachusetts.

Across all years of the data I use, there are 449,283 people who self-identify as registered nurses. I do not include licensed practical nurses (LPNs) in my baseline estimates, but I do include them in robustness analysis. Additionally for my baseline analysis I only include nurses that are in the labor force: 396,895 nurses. Since a different set of nurses is surveyed every year, ACS data is repeated cross-sectional instead of a true panel. From 2005-2016, my years when the ACS is in its standardized, final state, there are between 26,000 and 30,000 registered nurses in the labor force in every year. In 2001 - 2004 there are around 10,000 nurses in every year, and in 2000 there are only 3,246, reflecting smaller national sample sizes in those years. In every year, nurses in the labor force represent just under 1 percent of all observations. I weight my regressions using the Census-created person-weight variable in order to adjust for these different sample sizes across years.

Generally speaking, the nurses included in this sample have largely unsurprising demographic characteristics. For example, as seen in Table 1, registered nurses are 91 percent female. We can also see that they are largely white, with 82 percent self-identifying into that category. On average, their age is as expected, 45. The oldest nurse in the sample is 95 and the youngest is 16. 67 percent are married and they have a low unemployment rate of 1 percent (although this is likely biased as it is possible unemployed nurses may not identify as nurses in the survey). We can also see that approximately half of the entire sample live in states that get treated eventually, with around 40 percent of the sample actually being treated.

Nurses in treated states differ in a number of ways from nurses in never-treated states, as can be seen in Table 2, a raw comparison of basic covariates. They are older, less female, more non-white and non-black, and less likely to be married. This does raise concerns about the comparability of the two groups. I use the estimator from Callaway and Sant'Anna (2021) (CS), which allows for conditioning on covariates and uses never-treated and not-yet-treated units as appropriate counterfactuals. The need for this conditioning is further seen in Table 3, where nurses in never-treated states and nurses in states before treatment still differ in some categories conditional on year; nurses in not-yet-treated states are older, less white, and more likely to be married. This suggests that there might be some important differences in the nursing labor market between treated and never-treated states. I attempt to make the groups as comparable as possible by controlling for these things, in addition to using the CS estimator, which utilizes inverse probability weights.

V Strategy

I define treatment as the implementation of a law or regulation that prohibits mandatory overtime. Since the ACS data are collected on a rolling basis, I adjust the timing of treatment based on the implementation date of the law. If the law was put into place at some other time besides January 1st of the year, I say treatment began at the beginning of the next year so that all of the recorded nurses' data come from a year with at least partial treatment. Only three of the states have laws or regulations that became effective at the beginning of the year (California, Oregon, and New Hampshire), so this applies to fifteen of the treated states.

I seek to estimate the effects of these laws on various nurse outcomes, most importantly income, wage and work-time outcomes. My outcomes measuring total income for the year are directly found in the ACS: total income, total earned income, total income from wages and total income from welfare. In addition to understanding how these laws might affect overall annual income, I use additional outcomes that shed light on possible mechanisms for any increase in overall income from work. These include hourly wage, weekly hours worked, total yearly hours worked and total weeks worked. Like my yearly income outcomes, weekly hours and total weeks worked are reported directly in the ACS data, although the exact form varies in some years. I calculated total hours worked by taking the self-reported weekly hours and multiplying them by the reported total weeks worked. I then calculated hourly wage by taking total wage income and dividing by the calculated total hours worked.

V.A Estimation

I use a difference-in-differences framework in order to utilize the pattern of adoption of these laws to estimate their impact. The estimating equation that I first use is

$$Y_{ist} = \beta\,(G_i \times \text{Post}_t) + X_{ist}'\gamma + \alpha_s + \lambda_t + \varepsilon_{ist}, \tag{5}$$

In this equation, Y_{ist} is the outcome for nurse i in state s and year t. The term G_i is an indicator for whether the nurse is in a state that ever passes a mandatory overtime law, and Post_t is an indicator for post-treatment periods. The coefficient β measures the average treatment effect on the treated for the mandatory overtime law for the specific outcome, under standard conditional parallel trends assumptions. The vector X_{ist} contains observed covariates (e.g., age, sex, race, marital status, employment) with corresponding coefficients γ. The terms α_s and λ_t represent state and year fixed effects.

Since treatment happens at different times and I allow for heterogeneous effects, the standard difference-in-differences framework yields biased estimates for the laws' effects on my outcomes. Therefore I use the CS difference-in-differences approach. This method yields group-time average treatment effects on the treated. A weighted average of these group-time effects can then be taken, resulting in an aggregated ATT, which can be viewed as an overall effect. This method also uses both not-yet-treated and never-treated groups as comparisons when estimating group-time ATT. My estimating equation for aggregate ATT is:

$$Y_{it} = \alpha_i + \lambda_t + \sum_g \mathbf{1}\{G_i = g\}\mathbf{1}\{t \geq g\}\,\tau_{g,t} + X_{it}'\gamma + \varepsilon_{it}, \tag{6}$$

The indicator $\mathbf{1}\{G_i = g\}$ identifies the treatment cohort g to which nurse i belongs, and $\mathbf{1}\{t \geq g\}$ indicates periods that occur on or after that cohort's treatment year. The term $\tau_{g,t}$ represents the group-time average treatment effect for cohort g in year t.

V.B Assumptions

In order for the canonical and CS difference-in-differences frameworks to yield the ATT and aggregate ATT respectively, the standard parallel trends assumption is required. In this case that means that conditional on covariates, treated and comparison states would have experienced the same average period-to-period evolution in nurse outcomes. For the

CS framework this parallel trends assumption is applied cohort-by-cohort. In the results section, I evaluate the potential validity of this assumption by observing the various event studies and using pre-trend tests. For every outcome there is some suggestive evidence from these methods that the conditional parallel trends assumption might be invalid, although this might be attributable to early ACS data being sparse on sample size and less-standard in data gathering methods. The difference in observables between nurses in never-treated states and those in not-yet-treated states also brings up concerns about parallel trends validity. Although there are statistically significant differences in age, racial make-up, and marital status, these differences are likely not large and the characteristics not important enough to make a meaningful difference in the evolution in average nurse outcomes in treated states versus comparison states. In addition to the important parallel trends assumption, I need to assume that there are no anticipatory effects. In the case of nurses and mandatory overtime laws, it seems plausible that hospitals and other healthcare organizations started to change their shift assignment practices in anticipation of the laws so as to not be caught off-guard. Although this might be the case, in my event studies I find no evidence for an effect immediately before the time of treatment. I also need to assume that there are no spill-over effects from nurses in treated states and nurses in comparison states. As there is no way of tracking these nurses across time without specialized data, there is no way to explore if nurse migration could make this assumption less reasonable.

VI Results

VI.A Baseline Results

Table 4 reports the estimates for the effect of mandatory overtime laws on various measures of income. All income measures are in log terms, so the estimates can be interpreted as percent changes since they are smaller in magnitude. In the canonical difference-in-differences estimates there are significant, but small increases in total income, total earned income, and total wage income of around 2 percent on average for nurses in treated states. Unsurprisingly, the estimate for the effect on welfare income is not significant. An effect on welfare income would only occur if the laws pushed hospitals to stop hiring registered nurses and instead hired many more LPNs, certified nurse assistants, or similar workers. There is no evidence for this. Since the traditional difference-in-differences framework introduces bias in staggered adoption situations even with the assumption of conditional parallel trends, I also present aggregate ATT estimates from the CS difference-in-differences. Using this method, I estimate larger effects for non-welfare income: I find an aggregate ATT of positive 6.4 percent for total income, with similar estimates for earned and wage income. Nurses in not-yet-treated states earn an average of $53,749.05, so a 6.4 percent increase corresponds to roughly $3,493.69 in additional annual income. To see the magnitude of this, consider a nurse who is 45 years old - the average age in the sample. If the level difference persists and there is modest nominal wage growth of 2 percent per year, the cumulative additional earnings between ages 45 and 65 would be approximately $84,887. In Table 5, I find an aggregate ATT of positive 5.5 percent for average nurse hourly wages. Additionally, there is no evidence, in the canonical framework or in the CS estimates, for a change in hours or weeks worked. Thus, under these assumptions, although mandatory overtime was prohibited, there was no measurable impact on weekly or yearly average time worked. This implies that on average nurses working mandatory overtime switched to voluntary overtime, which is in line with what Bae and Yoon (2014) finds.

My simple model of nurse burnout predicts that decreasing burnout among nurses increases income through an increase in wages as a result of longer tenure. These empirical results are in-line with the prediction from that model. I estimate a significant increase in income, which is an obvious benefit for nurses. There is no evidence, however, that this increase occurs because of an increase in time-worked, whether at the weekly or yearly level. Rather, there is evidence that the increase in income results from a corresponding increase in hourly wage. If nurses switched to voluntary overtime, my model also predicts higher lifetime earnings as it costs less emotional reserve to voluntarily work extra. Altogether, my model presents a mechanism that can explain these empirical results: longer tenure due to reduced burnout that stems from either working less overtime overall or simply working less mandatory overtime.

VI.B Assessment of Parallel Trends

The above results rely heavily on the assumption of conditional parallel trends. To asses this, I present event study figures and perform pre-trend tests for all baseline CS difference-in-differences estimates. The pre-trend test uses a chi-square test with the null-hypothesis that all pre-treatment coefficients are equal to zero. For all eight of my outcomes, the chi-square statistic has a p-value of less than .05, indicating that at least one pre-treatment coefficient is statistically different from zero, which can be seen visually in Figures 2 and 3. These findings raise questions about the validity of the conditional parallel trends assumption; if taken at face value, they imply that the CS difference-in-differences estimates are invalid and do not represen the causal effect.

Additionally, the covariate imbalance shown in Table 3 could provide evidence that there are fundamental differences in the nurse labor markets between states that pass laws prohibiting mandatory overtime and those that do not. There are significant differences in age, race, and marital status. As is standard in CS difference-in-differences, I use inverse probability weights to re-weight the counterfactual groups to be on average slightly older, less white, and more likely to be married. Conditioning and weighting should reduce bias from observable differences, but residual imbalance - particularly in racial composition - could still threaten the parallel trends assumption, especially if different racial groups experience different wage trajectories. However, the magnitudes of differences are small. Nurses in not-yet-treated states are on average one year older, five percentage points less white and almost two percent more likely to be married. These differences are unlikely to change trends on their own, but warrant caution.

Taking another look at the event studies also reveals something important. For all outcomes besides welfare income, the pre-trend violations happen in the first three pre-treatment periods: 13, 12, and 11 years respectively before treatment. In every case, the three estimates are imprecisely estimated, and show no clear pattern. Small sample sizes in the earlier ACS years, as well as non-standard survey procedures until 2005 could be driving this pattern with early coefficients. For this reason I exclude these years in my robustness section. In contrast to the first three pre-periods, the nine coefficients immediately preceding treatment are statistically indistinguishable from zero and are fairly precise for all non-welfare income and time outcomes. These coefficients provide the most relevant support for the estimate of the treatment effect, since the CS estimator weights observations based on available pre-periods. This suggests that identification rests on short-run conditional parallel trends, which are reasonably well supported by the event studies. With this interpretation, the results for income, wage, and time worked should be viewed as suggestive, but fairly credible.

VI.C Robustness

I make additional sample and specification choices to test the robustness of the baseline CS difference-in-differences results. I first remove the pre-2005 ACS data from the sample, which also removes seven states since they are considered "always treated". I do this because the ACS was not in its current form until 2005, so the data might be fundamentally different, potentially biasing results. Next, I add state-specific linear time trends to control for things such as differences in long-term wage growth, development, demographic changes, and institutional differences. I next remove California and Missouri from the sample as they do not have formal laws, but rather have regulations prohibiting mandatory overtime. Next, I change the specification so that only the never-treated states' nurses are used as a comparison instead of both the never-treated and not-yet-treated states. Next, my main analysis only includes registered nurses, although the language of the laws also provides protection from mandatory overtime for LPNs. I include LPNs to check the robustness of these estimates to other related occupations. I also present estimates that include no covariates. In all of these cases - although with less certainty for the estimates using state-specific linear time trends - the same story emerges as in the baseline analysis. As seen in Tables 6 and 7 there are positive, significant effects on average income and wage measures, whereas there is no evidence for a change in the weekly or yearly time worked. There are some differences in the magnitude of the income effects, with estimates from Panels A and C showing statistically significantly lower estimates, but I still find that laws prohibiting mandatory overtime for nurses increase average income, assuming conditional parallel trends hold in every case. In Panel B of tables 6 and 7, which includes adjustments for state-specific linear trends, much of the long-run cross-state variation is absorbed, leading to generally imprecise estimates. Only the result of hourly wage increasing remains significant, with an aggregate ATT of 2 percent. I also find in this setting that overall income increases by the same amount as wage on average, although the standard errors are slightly larger (.011 vs .010). This strategy might overcontrol in this situation, as reducing nurse burnout likely changes outcomes in the medium- to long-run since it takes time for burnout to occur instead of small year-to-year deviations from a trend. Even if including these trends does not overcontrol, there still is some evidence that nurses benefit a little monetarily from these laws with the estimates on income and wage of about 2 percent.

VII Conclusion

Effective healthcare is an incredibly important part of any thriving society. Nurses are on the front lines of this work, and the nature of the job is such that nurses often experience the psychological burden of burnout, which can result in lower job satisfaction, poorer health outcomes for those being taken care of, and high turnover. This hurts everyone involved. Patients suffer from lower quality of care, hospitals have to spend time, effort, and money hiring nurses, and nurses are negatively affected in many areas of their lives. This burnout among nurses is the result of many different aspects of nursing care, some of which occurs because it is inherently difficult to take care of others' healthcare needs constantly, and some comes from hospital environment and policies.

Laws and regulations banning mandatory overtime are one way that policy makers have tried to step in to improve outcomes for nurses, which in turn improves outcomes for healthcare facilities and patients. The staggered adoption of these laws between 2001 and 2012 has allowed me to estimate the impact these laws have on nurse income and work time. In this paper I show evidence that income increased, on average, for the nurses in treated states, while also showing that time worked did not measurably change on average. Since the canonical difference-in-differences estimation biases results when there is staggered adoption and heterogeneous treatment effects, I use the difference-in-differences estimator from Callaway and Sant'Anna (2021) to reach my conclusions. If these results are, in fact, valid under the assumption of parallel trends, then the mandatory overtime laws were successful in at least one regard: nurses are financially better off. Moving forward, these results may be supportive of future laws and regulations designed to protect workers in high-stress environments.

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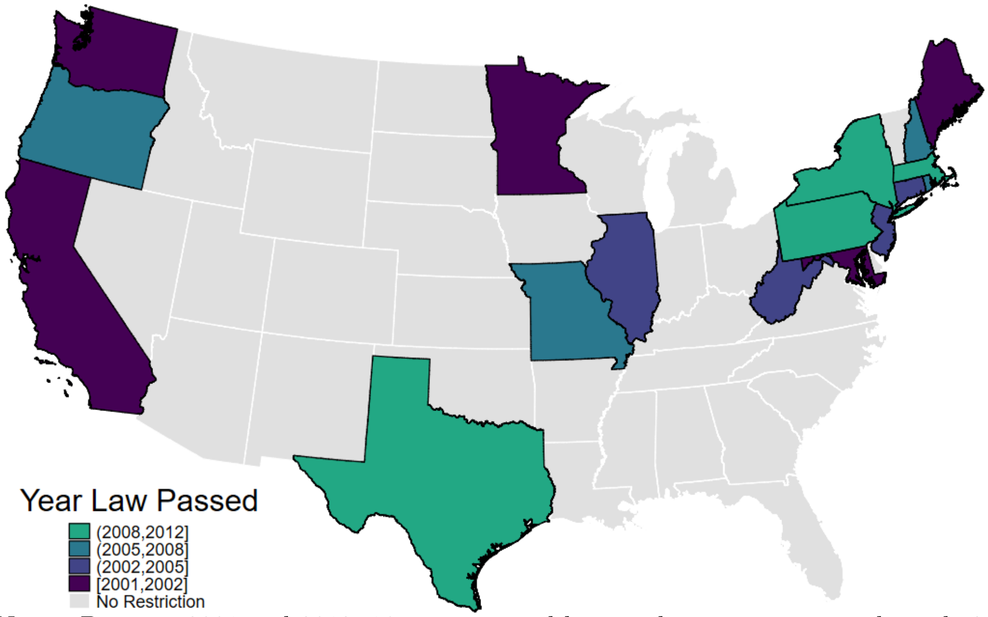
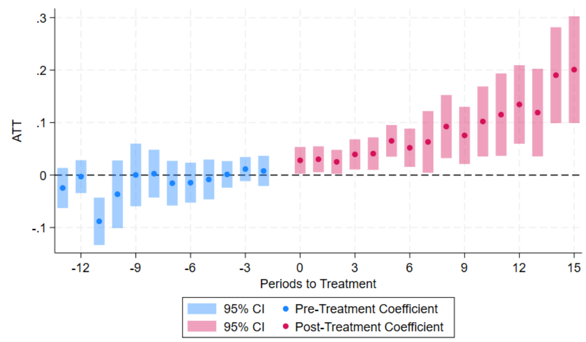
Figure 1. Adoption of Overtime Laws and Regulations by Year



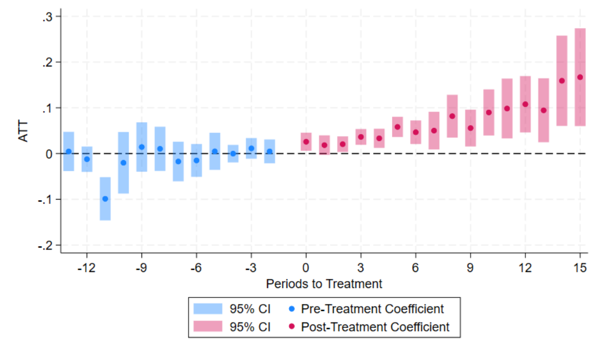
Notes: Between 2001 and 2012, 16 states passed laws and two states created regulations designed to prohibit mandatory overtime for nurses. This map shows states in the contiguous United States that adopted such policies and in what time frame they were adopted. Alaska (not-shown) adopted such a law in 2001.

Figure 2. Event Studies for Income Measures

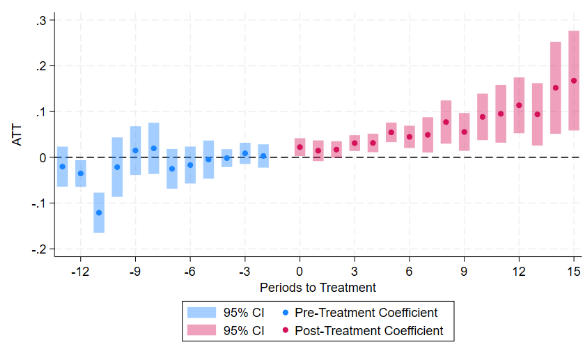
Panel A. Total Income



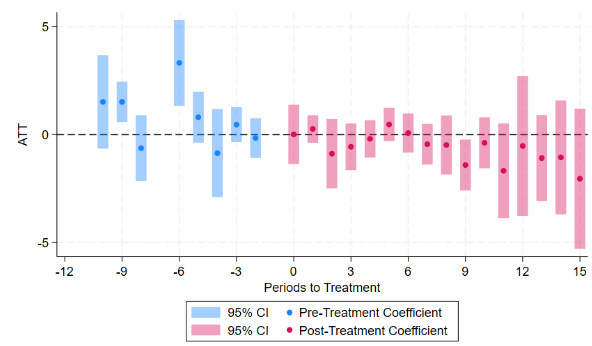
Panel B. Total Earned Income



Panel C. Total Wage Income



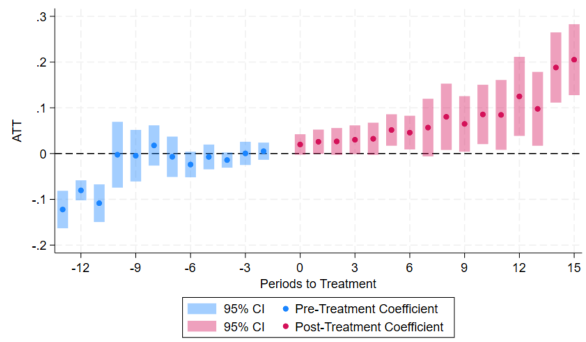
Panel D. Total Welfare Income



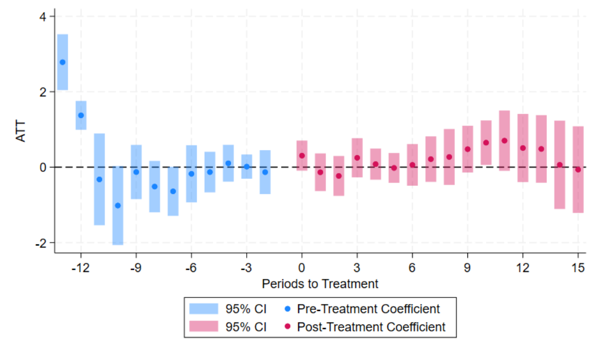
Notes: These figures plot event study coefficients from the Callaway and Sant'Anna (2021) differences-in-differences estimator for the effect of laws prohibiting mandatory overtime for various income measures: total income, total earned income, total wage income, and total welfare income. All income is in log terms. Shaded regions represent 95% confidence intervals for the estimates. Data from the ACS, 2000-2016.

Figure 3. Event Studies for Wage and Time-Worked Outcomes

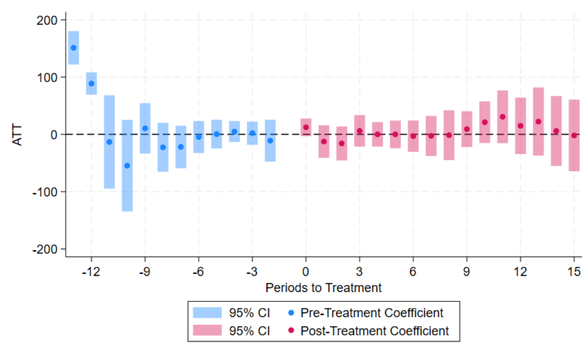
Panel A. Hourly Wage



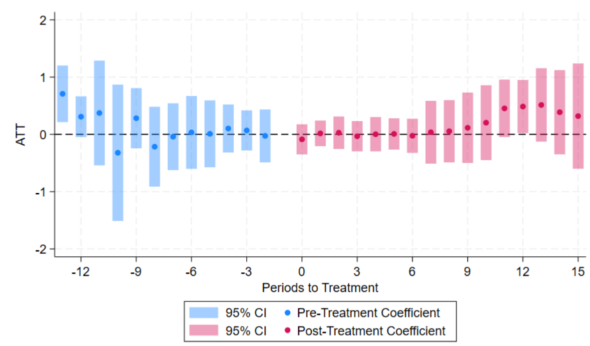
Panel B. Weekly Hours Worked



Panel C. Total Hours Worked



Panel D. Total Weeks Worked



Notes: These figures plot event study coefficients from the Callaway and Sant'Anna (2021) differences-in-differences estimator for the effect of laws prohibiting mandatory overtime for various outcomes: hourly wage, weekly hours, total yearly hours, and total yearly weeks. Wage is in log terms. Shaded regions represent 95% confidence intervals for the estimates. Data from the ACS, 2000-2016.

Table 1. Summary Statistics of Nurses Sample

Variable	Mean	SD	Min	Max
Employed	0.99	0.12	0.00	1.00
Age	45.22	11.96	16.00	95.00
Female	0.91	0.28	0.00	1.00
White	0.82	0.39	0.00	1.00
Black	0.08	0.27	0.00	1.00
Married	0.67	0.47	0.00	1.00
Total Income	57202.52	34179.79	0.00	873000.00
Hourly Wage	31.45	30.68	0.00	6071.43
In Treated State	0.51	0.50	0.00	1.00
Treated	0.38	0.48	0.00	1.00

Notes: This table presents summary statistics for the sample of registered nurses from the ACS, 2000-2016. The sample includes 396,895 registered nurses in the labor force. Variables include some covariates used in estimation (age, gender, race, marital status, employment status) and key outcomes (total income and hourly wage). "In Treated State" indicates the nurse resides in a state that eventually adopted a mandatory overtime law or regulation. "Treated" indicates the nurse resides in such a state after the policy's implementation.

Table 2. Comparison between Treated and Non-Treated Nurses

	Non-Treated		Treated		Difference	
Variable	Mean	SD	Mean	SD	Diff	p-value
Employed	0.985	0.120	0.985	0.122	0.000	0.512
Age	44.827	11.787	45.860	12.205	1.036***	0.000
Female	0.916	0.278	0.903	0.296	-0.016***	0.000
White	0.854	0.353	0.752	0.432	-0.114***	0.000
Black	0.081	0.273	0.076	0.265	-0.003**	0.048
Married	0.681	0.466	0.661	0.473	-0.026***	0.000
Total Income	52696.709	30592.638	64660.417	38257.280	13274.985***	0.000
Hourly Wage	28.899	28.920	35.678	32.965	7.300***	0.000

Notes: *** p<0.01, ** p<0.05, * p<0.10. This table compares the characteristics of nurses in a non-treated state (which includes both states that are pre-implementation and those that are never treated) and those in a treated state. Columns show means and standard deviations for some covariates (age, gender, race, marital status, employment status) in addition to the outcomes of total income and hourly wage. The difference columns show the difference in means along with a p-value. There are statistically significant differences between the two groups in all areas except employment status. Nurses in treated states are older, less female, less white, less black, less likely to be married, and better off in income terms. Treatment is the implementation of a law or regulation prohibiting mandatory overtime for nurses.

Table 3. Covariate Balance Table

	Non-Treated State		Pre-Treaded State		Difference	
Variable	Mean	SD	Mean	SD	Diff	p-value
Employed	0.990	0.122	0.978	0.115	-0.001	0.574
Age	42.074	11.949	43.236	11.085	0.970***	0.000
Female	0.924	0.279	0.928	0.274	-0.001	0.636
White	0.854	0.347	0.800	0.371	-0.051***	0.000
Black	0.100	0.274	0.074	0.265	0.004	0.177
Married	0.642	0.465	0.660	0.467	-0.017***	0.000

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.10$. This table compares nurse characteristics in never-treated states with nurses in not-yet-treated states, conditional on year. Columns show means and standard deviations for demographic covariates. Difference columns show the difference in means along with a p-value. There are significant differences in age, racial characteristics and marital status: nurses in not-yet-treated states are older, less white, and more likely to be married. Treatment is the implementation of a law or regulation prohibiting mandatory overtime for nurses.

Table 4. Effect of Overtime Laws on Income Measures

	Total Income	Earned Income	Wage Income	Welfare Income
	(1)	(2)	(3)	(4)
Panel A. Canonical DiD				
ATT	0.025	0.022	0.021	-0.208
	(0.010)	(0.010)	(0.010)	(0.160)
Panel B. CS DiD				
ATT	0.064	0.053	0.051	-0.488
	(0.024)	(0.017)	(0.016)	(0.529)

Notes: This table shows the average effect of mandatory overtime laws on registered nurses in states with such laws on four income measures: total income, total earned income, total wage income and total welfare income. All outcomes are in log terms. There are two sets of coefficients from two different differences-in-differences frameworks. The first is canonical differences-in-differences, and the second is the Callaway and Sant'Anna (2021) estimator. As such, panel B reports *aggregate* ATT. Data from the ACS. Clustered standard errors are in parentheses.

Table 5. Effect of Overtime Laws on Wage and Time Measures

	Hourly Wage (1)	Weekly Hours (2)	Total Hours (3)	Total Weeks (4)
Panel A. Canonical DiD				
ATT	0.015	0.229	10.356	-0.040
	(0.007)	(0.130)	(5.788)	(0.086)
Panel B. CS DiD				
ATT	0.055	0.172	2.570	0.077
	(0.026)	(0.210)	(11.971)	(0.172)

Notes: This tables shows the average effect of mandatory overtime laws on registered nurses in states with such laws on four different outcomes: hourly wage, weekly hours worked, total yearly hours worked and total yearly weeks worked. Hourly wage is in log terms. There are two sets of coefficients from two different differences-in-differences frameworks. The first is canonical differences-in-differences, and the second is the Callaway and Sant'Anna (2021) estimator. As such, panel B reports *aggregate* ATT. Data from the ACS. Clustered standard errors are in parentheses.

Table 6. Robustness Checks for Treatment Effects on Income Measures

	Total Income (1)	Earned Income (2)	Wage Income (3)	Welfare Income (4)
Panel A. Modern ACS Data				
ATT	0.025	0.029	0.024	0.025
	(0.009)	(0.007)	(0.007)	(0.406)
Panel B. State Linear Time Trends				
ATT	0.019	0.008	0.005	-0.286
	(0.011)	(0.010)	(0.011)	(0.365)
Panel C. Only Laws				
ATT	0.037	0.037	0.035	0.092
	(0.012)	(0.009)	(0.010)	(0.399)
Panel D. Never Treated				
ATT	0.060	0.049	0.046	-0.633
	(0.024)	(0.017)	(0.018)	(0.628)
Panel E. LPNs Included				
ATT	0.048	0.044	0.041	0.018
	(0.017)	(0.013)	(0.014)	(0.507)
Panel F. No Covariates				
ATT	0.067	0.059	0.057	-0.503
	(0.026)	(0.020)	(0.020)	(0.199)

Notes: This table presents robustness checks for the effect of mandatory overtime laws on income outcomes for nurses in treated states. Outcomes include total income, total earned income, total wage income and total welfare income. All outcomes are in log terms. Panel A restricts the sample to 2005-2016 when ACS methods were standardized. Panel B adds state-specific linear time trends. Panel C excludes California and Missouri (which have regulations rather than laws). Panel D uses only never-treated states as controls. Panel E includes licensed practical nurses. Panel F excludes all covariates. All estimates are computed using the Callaway and Sant'Anna (2021) estimator and show the *aggregate* average treatment effects on the treated. Data from the ACS. Clustered standard errors are in parentheses.

Table 7. Robustness Checks for Treatment Effects on Wage and Time Measures

	Hourly Wage (1)	Weekly Hours (2)	Total Hours (3)	Total Weeks (4)
Panel A. Modern ACS Data				
ATT	0.018	0.140	3.129	-0.167
	(0.009)	(0.196)	(9.293)	(0.186)
Panel B. State Linear Time Trends				
ATT	0.020	-0.042	-11.787	-0.068
	(0.010)	(0.154)	(9.440)	(0.182)
Panel C. Only Laws				
ATT	0.027	0.266	10.352	-0.036
	(0.009)	(0.182)	(9.352)	(0.181)
Panel D. Never Treated				
ATT	0.048	0.242	8.387	0.100
	(0.023)	(0.198)	(11.142)	(0.193)
Panel E. LPNs Included				
ATT	0.046	0.215	2.070	-0.123
	(0.026)	(0.215)	(14.236)	(0.213)
Panel F. No Covariates				
ATT	0.058	0.160	2.801	0.068
	(0.028)	(0.204)	(9.105)	(0.125)

Notes: This table presents robustness checks for the effect of mandatory overtime laws on wage and labor supply outcomes for nurses in treated states. Outcomes include hourly wage, weekly hours worked, total yearly hours worked and total yearly weeks worked. Panel A restricts the sample to 2005-2016 when ACS methods were standardized. Panel B adds state-specific linear time trends. Panel C excludes California and Missouri (which have regulations rather than laws). Panel D uses only never-treated states as controls. Panel E includes licensed practical nurses. Panel F excludes all covariates. All estimates are computed using the Callaway and Sant'Anna (2021) estimator and show the *aggregate* average treatment effects on the treated. Data from the ACS. Clustered standard errors are in parentheses.