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Aversion to Student Debt? Evidence from Low-Wage Workers

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ABSTRACT

We combine state minimum wage changes with individual-level income and credit data to estimate the effect of wage gains on the debt of low-wage workers. In the three years following a $0.88 minimum wage increase, low-wage workers experience a $2,712 income increase and a $856 decrease in debt. The entire decline in debt comes from less student loan borrowing among enrolled college students. Credit constraints, buffer-stock behavior, and other rational channels cannot explain the reduction in student debt. Our results are consistent with students perceiving a utility cost of borrowing student debt arising from mental accounting.

HOW DO WAGE GAINS AFFECT the borrowing and debt decisions of low-wage workers? In particular, do wage gains influence how low-wage workers finance human capital investments, such as enrollment in higher education? The answers to these questions are of first-order economic importance for at least two reasons. First, a large number of college students in the United States work low-wage jobs while attending school (Dinkes et al. (2008)). Given that these individuals also finance a significant portion of their education-related expenses with student loans (Darolia (2017)), a better understanding of how low-wage individuals respond to wage gains could have important implications for the aggregate growth rate of student loan debt. Second, outside of student loan debt, estimates of the borrowing and debt responses of low-wage workers to wage gains could have far-reaching economic applications. For instance,

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knowing such responses is useful for developing and testing consumption-savings models (Japelli and Pistaferri (2010)) and for understanding low-wage workers' preferences, borrowing motives, and financial constraints (Zinman (2015)).

In this paper, we combine state-level minimum wage changes with individual-level income and credit data to estimate the effect of wage gains on the debt of low-wage workers. In the three years following a $0.88 increase in the minimum wage, the average low-wage worker experiences a $2,712 increase in income and—in sharp contrast to prior work such as Aaronson, Agarwal, and French (2012)—a $856 decrease in debt. We find that the entire decline in debt comes from a reduction in student loan borrowing among enrolled college students. Nonstudents and former students, in contrast, use their wage gains to finance automobile purchases. Future credit constraints, default costs, buffer-stock behavior, and several other rational channels cannot explain the reduction in student loan debt. Even more puzzling, enrolled students with large amounts of credit card debt choose to borrow less student debt after they experience a wage increase. With the aid of a consumption-savings model, we find that the behavior of enrolled students is consistent with them attributing a utility cost, which we call student debt aversion, to borrowing student loan debt. As we discuss later, such debt aversion can arise from underlying mental accounting behavior.

Our empirical analysis uses payroll and consumer credit data from Equifax Inc. The payroll data contain anonymized information on the wages, hours, and job tenures of employees from over 2,000 firms in the United States between 2010 and 2020, and the consumer credit data contain anonymized information on the credit histories of all individuals in the United States. In contrast to most studies in the minimum wage literature, the presence of exact wage rates in our data allows us to precisely identify low-wage workers affected by the minimum wage. Furthermore, our credit data allow us to examine multiple dimensions of individual liabilities. We are unaware of any other research that uses data of this quality and breadth to study the effect of wage gains on the debt of low-wage workers.

To identify the effect of wage gains on the debt of low-wage workers, we use a difference-in-differences framework that exploits state-level variation in the minimum wage over time. We focus on 13 states and one district (hereafter, treated states) that each implemented at least one large, isolated minimum wage change between 2014 and 2017. For each treated state, we select a set of geographically adjacent control states that did not increase their minimum wage. We then restrict our final sample to border counties in the treated and control states (Dube, Lester, and Reich (2010)). Our identification assumption is that, in the absence of a minimum wage change, economic conditions in adjacent cross-border counties would have evolved similarly. In support of this assumption, we show that treated and control counties are observably similar and trend in tandem prior to treatment.

We estimate our difference-in-differences models at the individual level. Our sample consists of incumbent hourly wage workers in treated and control counties whose pretreatment wages are below their state's new minimum wage. We refer to these individuals as *bound employees*. For each treated state, our sample period is the 48 months surrounding the date of its minimum wage change (12 months before to 36 months after).[1] Thus, our estimates capture the long-run effects of the minimum wage on directly affected low-wage workers.

We begin our analysis by estimating the effect of the minimum wage on employment and wages. We confirm our prior findings from Gopalan et al. (2021) that an increase in the minimum wage raises the wages of incumbent low-wage workers without affecting their employment or average hours worked per week. Our estimates imply that, on average, bound employees experience a \$904 increase in annual earnings following a \$0.88 minimum wage change (\$2,712 over the 36-month posttreatment period). Our estimates also provide support for our empirical setting. For instance, we find no evidence of differential wage or employment pre-trends across bound employees in treated and control counties, and we find no evidence of wage or employment responses in the upper tail of the wage distribution.

Next, we estimate the borrowing and debt responses of low-wage workers to wage gains. The standard consumption-savings model predicts that bound employees should borrow in anticipation of a wage increase. Furthermore, in the presence of durables or credit constraints, the size of the borrowing response could be much larger than the size of the wage change. Inconsistent with these predictions, however, we find that total debt declines by \$856, on average, in the 36 months after a minimum wage increase. The \$856 decline in debt implies that the average marginal propensity to borrow (MPB) out of a minimum wage change in our sample is -0.32 ($= -\$856/\$2{,}712$). This estimate of the MPB is much lower than that documented in Aaronson, Agarwal, and French (2012), and it is also much lower than (and opposite in sign to) what is predicted by standard consumption-savings models with permanent wage increases and credit constraints (Japelli and Pistaferri (2010)).

Although bound employees reduce their debt on average, we find that the average debt response masks considerable heterogeneity. In particular, we find that the entire decline in debt comes from a reduction in student loan borrowing among enrolled college students. Enrolled students use their wage gains to reduce their borrowing within one quarter of the treatment date. In contrast, former students and nonstudents behave in a more conventional manner and use their wage gains to finance automobile purchases à la Aaronson, Agarwal, and French (2012).

From a consumption-savings standpoint, the reduction in student loan borrowing is surprising. Enrolled students have high expected future incomes but limited avenues for smoothing their consumption because of credit constraints

[1] Our difference-in-differences models include separate event-time fixed effects for each pair of treated and control counties. The coefficient estimates can therefore be interpreted as the average treatment effect across multiple event studies run in event time. We do not estimate a staggered two-way fixed effects model with variation in treatment timing, and hence the Goodman-Bacon (2021) and Callaway and SantAnna (2021) criticisms do not apply to our setting.

(Cadena and Keys (2013)). Thus, enrolled students should have strong incentives to spend in response to an increase in their wages. For enrolled students to prefer to save their wage gains, the benefits of borrowing less student debt must exceed the benefits of increasing and smoothing their consumption. Moreover, the benefits of borrowing less student debt must also exceed both the risk-free interest rate earned from saving and the shadow interest rate earned from reducing their use of nonstudent debt (Becker and Shabani (2010)). We note that at 4% (6%), the average interest rate on undergraduate (graduate) federal student loans is lower than the interest rate on most other forms of consumer credit during our sample period.

We investigate several rational explanations for the decline in student loan debt. One explanation could be that enrolled students borrow less student debt to improve their credit scores and hedge against future financial constraints (Rothstein and Rouse (2011), Dettling and Hsu (2018)). We find no evidence to support this explanation—enrolled students with both high and low credit scores reduce their borrowing in response to an increase in their wages, and enrolled students do not use their wage gains to improve their credit scores. Another explanation could be that enrolled students borrow less student debt to hedge against the high cost of going bankrupt while owing on student loans. Yet, inconsistent with this explanation, we find no differential reduction in student loan borrowing across the ex ante likelihood of going bankrupt. (This result should be interpreted with caution because of potential attenuation bias.) A third explanation could be that enrolled students choose to spend more time at work and less time at school after the opportunity cost of attending college increases (Brown, Fang, and Gomes (2012)). However, we find no differential change in employment or hours worked for enrolled students relative to former students and nonstudents, and we also find no decline in college enrollment in treated counties.

We explore and reject numerous other rational explanations for the decline in student loan debt. These explanations include buffer-stock behavior, student age, reductions in college tuition, income-driven repayments, student loan borrowing caps, proposed student loan borrowing amounts, and cash-based borrowing motives. Even more puzzling, we find that enrolled students with large amounts of credit card debt (i.e., $>25\%$ of annual income) choose to accumulate less student debt instead of paying down their credit card balances. This final finding helps rule out out a variety of preference-based explanations such as habit-based consumption and precautionary savings motives.

Instead of the rational explanations above, we find that our results are most consistent with two behavioral explanations. Of these, our favored explanation is student debt aversion. Student debt aversion captures the notion that student debt is unpleasant and entails additional costs besides just the actual interest rate (Field (2009)). For instance, student debt can undermine the pleasure received from current and future consumption (e.g., a "pain of paying" as in Prelec and Loewenstein (1998)), which in turn can generate a preference for using wage gains to finance education expenses and avoid additional indebtedness (Wong (2020)). To shed light on this explanation, we build a consumption-savings model that incorporates both student debt aversion and a one-time minimum wage hike. We model student debt aversion as a utility cost that is an increasing function of the amount of student debt. This function can be formulated in several ways, including through a high perceived student loan interest rate, a direct utility penalty per dollar of student debt, or a combination of both. At the extremes, we find that a perceived student loan interest rate of 33.1% or a direct utility penalty of 0.16 per dollar of student debt can generate a reduction in student loan borrowing that matches our empirical estimates.[2] As a form of model validation, we also show that our model predicts several cross-sectional patterns found in our data, including how the response of enrolled students varies with age and initial indebtedness.

The second behavioral explanation for our results is mental accounting (Thaler (1985)). While prior research shows that mental accounting can manifest as a form of debt aversion (Prelec and Loewenstein (1998)), a broader form of mental accounting could also operate in our setting. Specifically, to organize their financial activities, enrolled students might group their funds into separate, nonfungible mental accounts (Shefrin and Thaler (1988)). If enrolled students consider a certain fraction of their income as meant for their education, then an increase in their income could result in a reduction in student loan borrowing. Moreover, this reduction in student loan borrowing would be confined to enrolled (and not former) students, and the magnitude of the reduction would be independent of other factors such as credit constraints, default costs, and credit card debt. Another relevant departure of mental accounting from the standard consumption-savings framework is the idea that individuals adopt rules of thumb to constrain their own behavior (Thaler (1990)). For instance, a mental accounting rule of "take on as little student debt as possible" could lead to a reduction in student loan borrowing similar to what we observe in the data.

Our paper contributes to three distinct strands of literature. First, a handful of studies examine how household debt responds to income changes (e.g., Aaronson, Agarwal, and French (2012)). While most of these studies focus on the effects of one-time cash windfalls (Agarwal, Liu, and Souleles (2007), Agarwal and Qian (2014), Coibion, Gorodnichenko, and Weber (2020), Olafsson and Pagel (2021)) and repeated transfers (Cookson, Gilje, and Heimer (2022)), our paper focuses on how household debt responds to wage gains. Examining the independent effect of wage gains is important because theories of intertemporal choice predict that the debt response to an income change should depend on the nature and persistence of the change (Japelli and Pistaferri (2010)). Within the broader consumption-savings literature, our results are also related to the well-known puzzle that college students with high expected future incomes do not seem to borrow enough against their future earnings (Carroll

[2] The large and positive wedge between the perceived and actual interest rate on student debt helps highlight the importance of student debt aversion. In particular, for other forms of consumer credit, borrowers tend to perceive the price as too cheap relative to its actual cost (Stango and Zinman (2009)).

and Summers (1991)). While prior explanations for this finding focus on the role of credit constraints (Thaler (1990)), our results suggest that behavioral factors such as mental accounting and debt aversion could be important factors.

Second, our paper contributes to the literature on student loan debt (Black et al. (2020), Di Maggio, Kalda, and Yao (2020), Yannelis (2020), Perry, Karamcheva, and Yannelis (2021), Looney and Yannelis (2022), Mueller and Yannelis (2022)). Recent studies show that student loan debt can impose large costs on borrowers. For example, student debt can impede home ownership (Mezza et al. (2020)), reduce human capital accumulation (Chakrabarti et al. (2023)), delay marriage and child bearing (Goodman, Isen, and Yannelis (2018)), and constrain career choices (Rothstein and Rouse (2011)). Consistent with a high perceived cost of student debt, we find that enrolled students take out fewer student loans following an increase in the minimum wage. However, we are unable to explain the reduction in student loan debt using standard rational economic channels. Instead, we argue that the reduction in student loan debt is behavioral, and that it reflects mental accounting behavior (Prelec and Loewenstein (1998)) or student debt aversion (Oosterbeek and van den Broek (2009), Goldrick-Rab and Kelchen (2015), Meissner (2016), Caetano, Palacios, and Patrinos (2019)). Our results thus complement those in Marx and Turner (2019), who highlight behavioral barriers in the take-up of student loans.

Third, our paper contributes to the literature on the overall impact of the minimum wage. In particular, we provide the first large-scale estimates of the borrowing and debt responses to the minimum wage based on payroll and consumer credit data. Consistent with Aaronson, Agarwal, and French (2012), we find that nonstudents and former students use their wage gains to finance automobile purchases. However, we find that the average debt response is negative due to the large reduction in student loan debt among enrolled students. Our heterogeneous responses help reconcile the negative aggregate (i.e., metropolitan statistical area level) debt response in Cooper, Luengo-Prado, and Parker (2020) with the positive individual-level automobile debt response in Aaronson, Agarwal, and French (2012). Moreover, our results highlight several potential but often overlooked benefits of increasing the minimum wage. For example, our results suggest that an increase in the minimum wage could help relax credit constraints for incumbent low-wage workers (Dettling and Hsu (2021)) in addition to reducing the aggregate growth rate of student loan balances. Indeed, our paper is among the first to document a negative relation between minimum wage policies and student loan borrowing among enrolled college students.

The remainder of the paper is organized as follows. Section I describes our setting, Section II discusses our data, Section III estimates the wage and employment responses to the minimum wage, Section IV estimates the borrowing and debt responses, Section V examines rational explanations for the decline in student debt, Section VI examines behavioral explanations, and Section VII concludes and discusses caveats.

I. Institutional Background

A. State Minimum Wage Changes

We begin by providing background on state-level minimum wage changes between January 2010 and December 2017, that is, the period over which we have at least three years of posttreatment data. Following the increase in the federal minimum wage to $7.25 per hour in July 2009, few states enacted new one-time or multiphase minimum wage changes. Of the 30 state minimum wage changes that occurred between 2010 and 2013, the vast majority were from previously enacted policies that had indexed the minimum wage to inflation. However, beginning in 2014, several states enacted new one-time or multiphase minimum wage changes. Many of these changes were for large amounts. In particular, there were 45 state minimum wage changes of at least $0.25 per hour between 2014 and 2017. There were no changes to the federal minimum wage during this same period.

B. Selection of Treated and Control Geographies

We focus on large and isolated state minimum wage changes. Specifically, we restrict our sample to state minimum wage changes of at least $0.25 per hour that occurred between 2014 and 2017 and were not preceded by any other minimum wage change between 2010 and 2013. Thirteen states and one district in the continental United States (the treated states) have minimum wage changes that satisfy these conditions: Arkansas, California, Delaware, Massachusetts, Maryland, Maine, Michigan, Minnesota, Nebraska, New Jersey, New York, South Dakota, Washington D.C, and West Virginia. For treated states with more than one minimum wage change during the sample period, we focus on the chronologically first change. We refer to the date of each minimum wage change as the treatment date.

Table I describes our sample of state minimum wage changes. The sample consists of one increase of $0.25, one increase of $0.50, six increases of $0.75, three increases of $1.00, two increases of $1.25, and one increase of $1.50. The employment-weighted-average increase in the minimum wage is $0.88 (11.6%). Most of the increases occurred in 2014 and 2015.

We match each treated state to a set of adjacent control states that did not increase their minimum wage. We then follow Gopalan et al. (2021) and limit our final sample to individuals who reside in the border counties of treated and control states. Table I lists the 162 treated counties and 162 control counties in our final sample, and Figure IA.1, which is located in the Internet Appendix, displays their geographic locations.[3] We exclude two border counties in Maryland with their own local minimum wage ordinances.

[3] The Internet Appendix is available in the online version of this article on *The Journal of Finance* website.

Table I
State Minimum Wage Changes

This table describes the sample of state minimum wage changes. The columns are defined as follows: *Treated state* is one of the 14 treated states, *MW Δ date* is the year-month in which a treated state changes its minimum wage, *BOP MW* is the state's minimum wage at the beginning of the sample period, *MW Δ amount* is the size of the minimum wage change on *MW Δ date*, *Control states* is the set of control states for each treated state, *Treated counties* is the number of counties in the treated state that border a county in a control state, and *Control counties* is the number of counties in the control states that border at least one county in a treated state.

Treated state (1)	MW Δ date (2)	BOP MW (3)	MW Δ amount (4)	Control states (5)	Treated counties (6)	Control counties (7)
AR	201501	7.25	0.25	(OK, TX, LA, MS, TN)	19	25
CA	201407	8.00	1.00	(NV)	10	8
DC	201407	8.25	1.25	(VA)	1	1
DE	201406	7.25	0.50	(PA)	1	1
MA	201501	8.00	1.00	(NH)	4	3
MD	201501	7.25	0.75	(PA, VA)	12	15
ME	201702	7.50	1.50	(NH)	2	3
MI	201409	7.40	0.75	(WI, IN)	9	10
MN	201408	7.25	0.75	(ND, IA, WI)	25	25
NE	201501	7.25	0.75	(WY, KS, IA)	25	21
NJ	201401	7.25	1.00	(PA)	7	5
NY	201401	7.25	0.75	(PA)	10	9
SD	201501	7.25	1.25	(WY, ND, IA)	16	14
WV	201501	7.25	0.75	(PA, KY, VA)	21	22

We assign each border county to a *cross-border county pair* that consists of two or more adjacent treated and control counties.[4] Cross-border county pairs attempt to proxy for areas over which economic conditions evolve smoothly but where the level of the minimum wage varies discontinuously. For each cross-border county pair, we restrict the sample period to the 48 months surrounding a minimum wage change (12 months before to 36 months after). We set the pretreatment period of control counties to be the same as their paired cross-border treated counties.[5]

[4] Some control counties border several treated counties. In these cases, we pair control counties to adjacent treated counties at random, subject to the condition that each one of our 324 border counties belongs to a pair. Because of this last constraint, a few cross-border pairs will have more than two counties (e.g., one treated county and two control counties within a pair). Later, we show that our results are robust to alternative methods of pairing adjacent treated and control counties, such as including duplicate observations for counties that have multiple cross-border matches (Dube, Lester, and Reich (2010)).

[5] For example, consider the case of West Virginia, which increased its minimum wage by $0.75 in January 2015, and its neighboring control state of Kentucky. In our setting, the pretreatment period for Kentucky counties that are paired with West Virginia counties along the Kentucky-West Virginia border is January 2014 to December 2014. The posttreatment period for these counties is January 2015 to December 2017.

While comparing individuals in adjacent counties has intuitive appeal, Neumark, Salas, and Wascher (2014) question whether cross-border counties serve as valid counterfactuals. To alleviate this concern, Table IA.I and Figure IA.2 compare the economic conditions of treated and control counties prior to treatment. Consistent with control counties being valid counterfactuals, we find that treated and control counties are similar along most observable dimensions and trend in tandem prior to treatment. (We also find similar results when zooming out to the state level. See Table IA.II and Figure IA.3.) Another potential concern with our setting is that there could be spillovers from treated counties to adjacent control counties (Neumark (2018)). For example, individuals in control counties might relocate to treated counties in pursuit of higher wages, or companies in control counties might raise their wages to avoid losing more experienced workers. We note that such spillovers should bias against finding an effect of the minimum wage on the debt of low-wage workers. Moreover, the fact that the wages of minimum wage workers increase just up to the new minimum wage (see Section III) suggests that spillovers are not operating in a meaningful manner.

II. Data and Sample Selection

A. Data

Our analysis uses payroll and consumer credit data from Equifax Inc. The payroll data contain anonymized information on the monthly earnings, hours, and job tenures of employees from over 2,000 firms in the United States between 2010 and 2020 (roughly 20 million active employee records per month). The data distinguish between hourly and salary employees and between voluntary and involuntary turnover, and they specify exact hourly wage rates. The presence of exact hourly wage rates in the payroll data allows us to precisely identify minimum wage workers. This stands in contrast to most studies in the minimum wage literature, which usually rely on proxies such as age (Dube, Lester, and Reich (2016)), industry (Dube, Lester, and Reich (2010)), or location (Dettling and Hsu (2021)) to identify minimum wage workers.

Section II of the Internet Appendix describes the payroll data in greater detail and compares it to population data sources. We find that the data are representative of the U.S. labor force along several dimensions, including median personal incomes. However, while most industries are represented in the correct proportions in the payroll data, the share of employment in the retail trade industry is much higher than in the population.[6] The average firm in the payroll data also tends to be larger than the average firm in the United States.

[6] The payroll data contain 48% of all jobs in the retail trade industries (NAICS 44 and 45) and 14% of all jobs in the leisure and hospitality industries (NAICS 71 and 72). Given that these industries are the main providers of minimum wage jobs in the United States (Dube, Lester, and Reich (2010)), our data should be well suited for examining the impact of the minimum wage on low-wage workers.

We combine the payroll data with anonymized consumer credit data. The consumer credit data contain the credit histories of the entire U.S. population, conditional on having a credit history. At the individual level, the data contain information on credit scores, credit inquiries, and derogatory public records such as foreclosures or bankruptcies. At the credit account level, the data contain information on account types, balances, credit limits, and any missed or late payments. The data begin in 2005 and continue until 2020. For a more detailed description of the consumer credit data, see Avery et al. (2003).

B. Sample

We conduct our empirical analysis at the individual employee level. Our sample consists of incumbent hourly wage employees between the ages of 18 and 64 whose pretreatment hourly wages are below their state's new minimum wage, that is, the state's minimum wage after it enacts its scheduled increase.[7] We refer to these individuals as *bound employees*. Bound employees are directly affected by the minimum wage and hence are of both economic interest and policy interest (Neumark (2018)). Table IA.III records our definition of bound employees.

We restrict our sample to bound employees in the intersection of the payroll and the consumer credit data, and we limit sample entry to the pretreatment period. Our sample therefore comprises incumbent low-wage workers with valid credit histories that are employed prior to a minimum wage change. For our analysis of labor market outcomes, we drop employees from the sample after they separate from their employer. For our analysis of credit market outcomes, we continue to follow employees regardless of their labor market status. This is because, in contrast to our payroll data, our credit data are not subject to sample attrition.

Table II describes our sample of 76,982 bound employees. Prior to treatment, the median bound employee is 25 years old, works 30 hours per week, and earns $7.75 per hour. Forty percent of bound employees earn exactly the minimum wage, and 39% have student debt (average student debt balance = $7,269). The median bound employee is credit constrained with a credit score of 583, no open credit cards, and no open auto loans. However, on average, bound employees have $1,325 in credit card debt, $2,641 in auto loan debt, and $4,070 in open credit limits. Along most observable dimensions, bound employees in treated counties are statistically similar to bound employees in control counties. Furthermore, in support of the external validity of our setting, bound employees in border counties are observably similar to bound employees in interior counties (Table IA.IV).

[7] We define an employee's pretreatment hourly wage as their wage in the month closest to three months prior to treatment. For control counties, the new minimum wage is defined as the minimum wage it would have had if it had implemented the same minimum wage change as its cross-border treated county.

Table II

Bound Employees

This table contains descriptive statistics for the 76,982 bound employees. The descriptive statistics are as of the month closest to three months prior to treatment. The right-most columns are defined as follows: *Treated* is the mean value in treated counties, *Control* is the mean value in control counties, *Diff* is the difference in means between treated and control counties, and *t(Diff)* is the t-statistic for the difference in means across treated and control counties.

	Mean (1)	*SD* (2)	P25 (3)	P50 (4)	P75 (5)	Treated (6)	Control (7)	Diff (8)	t(Diff) (9)
Hourly wage	7.82	0.53	7.30	7.75	8.25	7.88	7.78	0.10	0.34
Hours per week	29.18	10.46	20.00	30.00	40.00	28.60	29.74	−1.14	−0.58
Tenure (months)	16.87	31.71	1.00	6.00	18.00	15.72	17.82	−2.10	−0.45
Age (years)	30.70	12.56	21.00	25.00	39.00	30.14	31.16	−1.02	−0.70
Enrolled student? (1/0)	0.29	0.45	0.00	0.00	1.00	0.27	0.30	−0.03	−0.40
Former student? (1/0)	0.10	0.30	0.00	0.00	0.00	0.10	0.10	0.01	0.70
Credit score	582	126	516	583	671	579	585	−6	−0.78
Has active trade? (1/0)	0.75	0.43	0.00	1.00	1.00	0.74	0.76	−0.02	−1.41
Has credit card? (1/0)	0.48	0.50	0.00	0.00	1.00	0.49	0.48	0.01	0.33
Has auto loan? (1/0)	0.20	0.40	0.00	0.00	0.00	0.19	0.21	−0.01	−0.49
Has student loan? (1/0)	0.39	0.49	0.00	0.00	1.00	0.38	0.40	−0.02	−0.30
Total balances	11,499	19,750	0	3,644	15,388	10,900	11,995	−1,094	−0.87
Card balances	1,325	4,305	0	0	661	1,333	1,318	15	0.09
Auto balances	2,641	6,941	0	0	0	2,455	2,796	−341	−0.60
Student balances	7,269	16,980	0	0	7,900	6,892	7,581	−689	−0.38
Card credit limits	4,070	11,693	0	0	2,000	3,854	4,250	−396	−0.51
Card credit utilization	0.28	2.91	0.00	0.00	0.36	0.30	0.26	0.04	1.04
Is 90+ delinquent? (1/0)	0.17	0.38	0.00	0.00	0.00	0.16	0.18	−0.02	−1.58
Card 90+ delinquent? (1/0)	0.09	0.28	0.00	0.00	0.00	0.09	0.09	0.00	0.80
Auto 90+ delinquent? (1/0)	0.03	0.16	0.00	0.00	0.00	0.02	0.03	−0.01	−1.69
Student 90+ delinquent? (1/0)	0.06	0.24	0.00	0.00	0.00	0.06	0.07	−0.01	−0.79

III. Wages, Employment, and Income

In this section, we examine the wage, employment, and income effects of the minimum wage. Consistent with our prior findings in Gopalan et al. (2021), we find that an increase in the minimum wage raises the wages of incumbent bound employees without affecting their employment or hours worked per week. Hence, incomes rise.

A. Wages

We start by estimating the wage response to the minimum wage. The model is

$$\omega_{i,t} = \alpha + \Gamma \times \mathit{Treated}_s \times \mathit{Post}_{t,s} + \delta_i + \delta_{p,t} + \gamma' X_{s,t-12} + \varepsilon_{i,t}, \tag{1}$$

where $\omega_{i,t}$ is the hourly wage of bound employee i in state s and cross-border county pair p in month t. The dummy variable $\mathit{Treated}_s$ is equal to one if state s enacts a minimum wage change and zero otherwise, and $\mathit{Post}_{t,s}$ is equal to

Table III

Wages, Employment, and Incomes

This table reports coefficient estimates from equation (1). The dependent variable in columns (1) and (2) is the hourly wage. The dependent variable in columns (3) and (4) is a dummy variable for employment. The dependent variable in columns (5) and (6) is the average number of hours worked per week. The dependent variable in columns (7) and (8) is annual income. The sample is restricted to bound hourly wage employees who remain employed. Standard errors, presented below the coefficient estimates, are calculated by clustering at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

	Hourly Wage		Employment		Hours		Income	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated × Post	0.651***	0.576***	0.000	0.000	0.267	0.221	1058***	912***
	(0.083)	(0.022)	(0.001)	(0.001)	(0.209)	(0.171)	(265)	(120)
Employee FE	Y	Y	Y	Y	Y	Y	Y	Y
Border-pair month FE	Y	Y	Y	Y	Y	Y	Y	Y
Control variables		Y		Y		Y		Y
N	2,021,644	2,014,193	2,021,644	2,014,193	884,581	881,318	884,581	881,318
R^2	0.76	0.76	0.13	0.13	0.84	0.84	0.82	0.82

one in all months t after the treatment date in state s and zero otherwise. The model includes employee fixed effects (δ_i) and cross-border county pair month effects ($\delta_{p,t}$) to ensure that the treatment effect (Γ) is estimated using within-employee variation after netting out common shocks to cross-border counties. In our tightest specification, we also include lagged state-level controls for house price growth and GDP per-capita growth (Clemens and Wither (2019)). Standard errors are clustered at the state level to match the assignment of treatment.

The coefficient of interest, Γ, measures the average change in wages for bound employees in treated counties relative to adjacent control counties. Table III reports the coefficient estimates from the model. We find that hourly wages rise \$0.58, on average, in the 36 months after a sample-average \$0.88 minimum wage change.[8] Figure IA.4 plots the dynamics of the coefficient estimates. We find that wages trend in tandem during the pretreatment period and increase within one month of the treatment date, and that the increase does not dissipate. Consistent with our empirical setting capturing the causal effect of the minimum wage, we also find no significant evidence

[8] One reason the wage response is not equal to the sample-average minimum wage change is because a significant fraction of bound workers have pretreatment wages above the old minimum wage. Hence, the relevant comparison is not to the sample-average minimum wage but rather to the weighted-average gap between pretreatment wages and the new minimum wage. (Indeed, we estimate a short-run wage response of \$0.54 cents (±12 months), which is right above the weighted average pretreatment gap of \$0.47. This finding also suggests that cross-county spillovers do not have a meaningful effect on our results.) For bound workers who remain in the labor market sample after 12 months, we find that wages continue to respond to later minimum wage changes. However, their wages become less responsive to later minimum wage changes because of movements up the income distribution (e.g., because of raises). See also Dube, Giuliano, and Leonard (2019).

of wage responses in the upper tail of the wage distribution (Figure IA.5). Overall, the timing, magnitude, and location of the wage response supports the quality of our data and the validity of our experimental design.

B. Employment and Income

We next estimate the employment response to the minimum wage. Specifically, we reestimate equation (1) after replacing the outcome variable with either a dummy variable for employment or the average number of hours worked per week (variable definitions are provided in Table IA.V). Table III reports the coefficient estimates. We find no economically or statistically significant changes in employment or hours following an increase in the minimum wage. We also find no significant evidence of differential employment pretrends (Figure IA.6), and no evidence of employment responses in the upper tail of the wage distribution (Figure IA.7). For a comparison of our employment estimates to other recent estimates in the minimum wage literature, see Neumark and Shirley (2021).

Given that bound employees work an average of 30 hours per week, our results imply that the average worker experiences a \$904 (= \$0.58 × 30 × 52) increase in annual income following a minimum wage change (7.4% from a base of \$12,200 per year; \$2,712 over the 36-month postperiod). Formal estimates in Table III are consistent with this calculation.

IV. Debt and Borrowing

A. Debt

Given that the wages and incomes of low-wage workers rise in response to minimum wage changes, we next estimate the debt response to the minimum wage. The model is

$$y_{i,t} = \alpha + \Gamma \times Treated_s \times Post_{t,s} + \delta_i + \delta_{p,t} + \gamma' X_{s,t-12} + \varepsilon_{i,t}, \tag{2}$$

where the outcome variable is the total debt of bound employee i in month t. Similar to equation (1), the coefficient of interest, Γ, measures the average change in debt for bound employees in treated counties relative to adjacent control counties. Standard errors are again clustered at the state level to match the assignment of treatment.

Table IV reports the coefficient estimates from the model. If bound employees behave according to the standard consumption-savings framework, then the Γ coefficient should be greater than or equal to zero. Yet, we find the opposite in the data—total debt declines by \$856, on average, in the 36 months after a minimum wage change. Assuming that our income estimate from Section III is applicable for the entire sample, an \$856 decline in debt implies that the average MPB out of a minimum wage change is −0.32 (= −\$856/\$2,712). This estimate of the MPB is much lower than that documented in Aaronson, Agarwal, and French (2012), and it is also much lower than (and opposite in

Table IV

Debt Balances and Open Accounts

This table contains coefficient estimates from equation (2). The dependent variable in columns (1) and (2) is total debt balances and open accounts. The dependent variable in columns (3) and (4) is credit card balances and open accounts. The dependent variable in columns (5) and (6) is auto loan balances and open accounts. The dependent variable in columns (7) and (8) is student debt balances and open accounts. In the top portion of the table, the sample contains the full set of bound hourly wage employees. In the bottom portion of the table, the model is reestimated on the labor market subsample from Table III. Standard errors, presented below the coefficient estimates, are calculated by clustering at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

	Total		Card		Auto		Student	
	Balances (1)	Accounts (2)	Balances (3)	Accounts (4)	Balances (5)	Accounts (6)	Balances (7)	Accounts (8)
Treated × Post	−856***	−0.09*	18	0.05**	117*	0.01*	−944***	−0.14***
	(310)	(0.050)	(16)	(0.020)	(67)	(0.006)	(290)	(0.036)
Employee FE	Y	Y	Y	Y	Y	Y	Y	Y
Border-pair month FE	Y	Y	Y	Y	Y	Y	Y	Y
Control variables	Y	Y	Y	Y	Y	Y	Y	Y
N	3,732,264	3,732,264	3,732,264	3,732,264	3,732,264	3,732,264	3,732,264	3,732,264
R^2	0.85	0.87	0.79	0.85	0.65	0.70	0.90	0.89

	Labor market subsample: difference-in-differences coefficient estimates							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated × Post	−765***	−0.07	35**	0.06***	119*	0.01*	−880***	−0.14***
	(283)	(0.050)	(16)	(0.018)	(65)	(0.006)	(295)	(0.045)

sign to) what is predicted by standard consumption-savings models with permanent wage increases and credit constraints (Japelli and Pistaferri (2010)).

One issue with our estimate of the MPB is that the debt response and the wage response are estimated on different samples. That is, due to sample attrition, the labor market sample that is used to estimate the wage response in Table III is a subsample of the credit market sample that is used to estimate the debt response in Table IV. To examine how differences between these two samples affect our results, we reestimate the debt response on the labor market sample. The bottom panel in Table IV reports the coefficient estimates. We find that total debt declines by \$765, on average, in the labor market sample. This estimate is \$91 lower than our estimate from the credit market sample (on an absolute basis), and it implies an MPB out of a minimum wage change of −0.28 (= −\$765/\$2,712).[9]

B. What Drives the Decline in Debt?

To better understand what drives the observed reduction in debt, we reestimate equation (2) across the three major categories of consumer credit in our sample: credit cards, auto loans, and student loans.[10] Table IV reports the coefficient estimates. We find that the entire decline in debt comes from a reduction in student loan balances ($\Gamma = -\$944$; $t = -3.26$). By contrast, auto loan balances rise following an increase in the minimum wage ($\Gamma = \$117$; $t = 1.74$), and credit card balances do not change ($\Gamma = \$18$; $t = 1.11$).

To the best of our knowledge, our paper is the first to document a negative relation between student loan debt and wage gains. A handful of other papers find that low-wage individuals use wage gains and cash windfalls to finance automobile purchases (e.g., Cookson, Gilje, and Heimer (2022)).

C. Who Drives the Decline in Student Loan Debt?

Next, we examine which subset of workers drives the decline in student loan debt. To do so, we split our sample into three groups: enrolled students (29%), former students (10%), and nonstudents (61%).[11] An enrolled student

[9] Aside from the MPB documented in Aaronson, Agarwal, and French (2012), it is difficult to compare our MPB to other estimates in the literature. This is because most other papers focus on estimating the MPB out of transitory cash windfalls (Agarwal, Liu, and Souleles (2007), Olafsson and Pagel (2021)) or liquidity (Agarwal et al. (2018), Aydin (2022)) as opposed to wage gains. In terms of magnitude, the closest estimate to ours is the −0.33 MPB out of recurring cash windfalls documented in Cookson, Gilje, and Heimer (2022).

[10] As shown in Tables IA.XI and IA.XII, we find no significant changes in mortgage balances after an increase in the minimum wage. We thus ignore mortgages because mortgages are uncommon among low-wage workers.

[11] The fraction of individuals between the ages of 18 and 24 that are enrolled students in our sample is higher than estimates from the Current Population Survey (CPS). This is because we restrict our sample to individuals with valid credit histories. If we reinclude individuals without valid credit histories in our sample, then our estimates for this group (15%) are similar to the estimates from the CPS (11%).

is an individual that satisfies either of the following two conditions: (i) the individual has positive pretreatment student loan balances and is between the ages of 18 and 22 during the pretreatment period or (ii) the individual has positive, increasing, and nondelinquent pretreatment student loan balances and is above the age of 22 during the pretreatment period. Nonstudents are individuals with zero pretreatment student loan balances, and former students are the remaining individuals with positive pretreatment student loan balances. Table IA.III records our definitions of enrolled, former, and nonstudents.[12]

Conceptually, enrolled students are teenagers and adults who are working their way through college, whereas former students are college drop-outs or graduates that experienced poor labor market outcomes. Nonstudents are career low-wage workers who did not attend (or have not yet attended) college. Because nonstudents do not have student loan debt, we restrict our sample to enrolled and former students going forward. Briefly, we note that the debt response of nonstudents mirrors the response documented in Aaronson, Agarwal, and French (2012). Specifically, nonstudents (described in Table IA.VI) use their wage gains to finance automobile purchases. However, these purchases do not materialize in average auto loan balances until around 12 to 18 months after the minimum wage change (Figure 1), which is consistent with nonstudents using their wage gains to accumulate down payments and resolve credit constraints (Dettling and Hsu (2021)).

Table IA.VII describes our sample of 22,940 enrolled students and 7,760 former students. Aside from student loan balances ($18,555 average enrolled-student balance versus $19,267 average former-student balance), enrolled and former students differ along most observable dimensions. Enrolled students are younger, have lower job tenures, earn slightly less per hour, and work fewer hours per week than former students. Enrolled students also have higher credit scores, lower credit card and auto loan balances, and are much less likely to be delinquent on their debt. In addition to these observable dimensions, enrolled and former students likely also differ along unobservable dimensions. For example, enrolled students likely have higher expected future incomes and perceived default costs than former students. Enrolled students are also more likely to be in their student loan borrowing cycle, while former students are more likely to be in their repayment cycle.

[12] Our definitions are subject to two sources of classification error. First, our sample of enrolled students might contain some former and nonstudents (e.g., parents of current college students who are taking out Parent PLUS loans). Second, our sample of nonstudents might include some former students who have paid off their student debt, or some enrolled students who are not accruing student debt (e.g., due to scholarships). To address the first source of classification error, in Table IA.VIII we show that our results are robust to several different definitions of enrolled students. To address the second source of classification error, in Figure 1 we show that nonstudents do not adjust their student loan balances in response to an increase in the minimum wage. Therefore, misclassified enrolled students do not help explain our results.

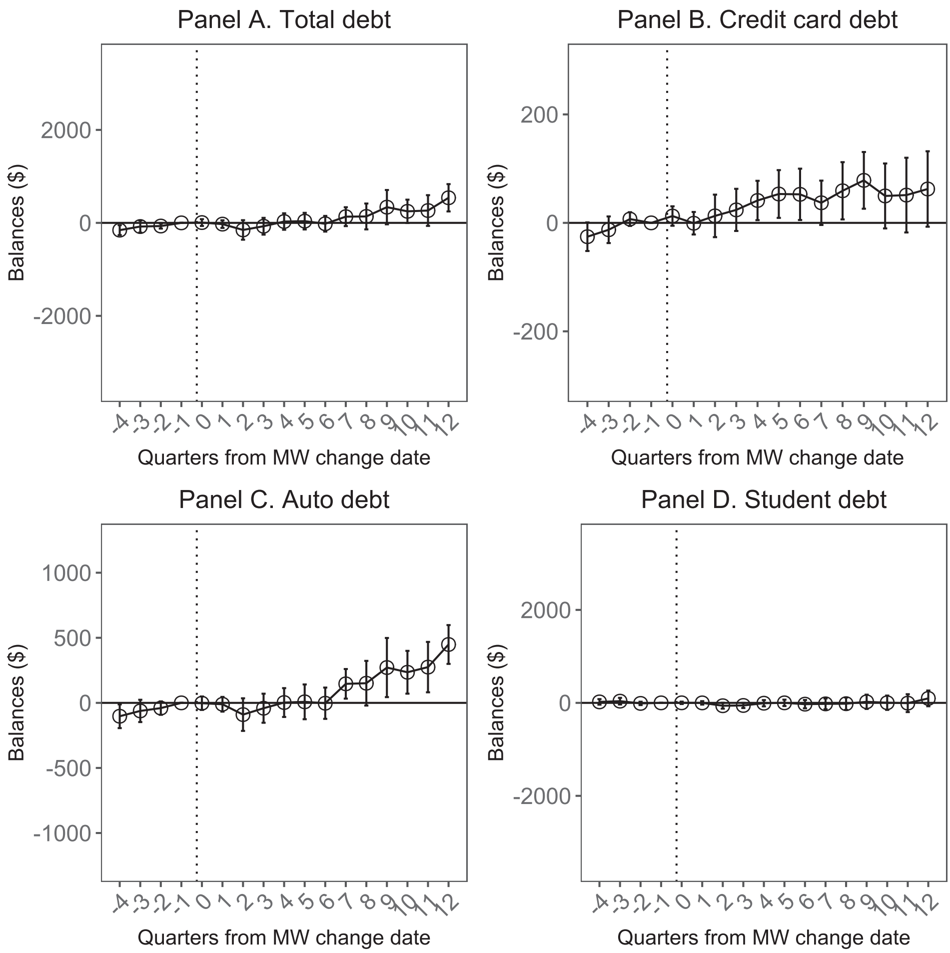

Figure 1. Debt response of nonstudents. This figure plots coefficient estimates from a dynamic version of equation (2), $y_{i,t} = \alpha + \sum_{\tau=-4}^{12} \Gamma_\tau \times \mathit{Treated}_s \times D_{s,t,\tau} + \delta_i + \delta_{p,t} + \gamma' X_{s,t-12} + \varepsilon_{i,t}$, where $D_{s,t,\tau}$ is equal to one when quarter t is τ quarters away from a minimum wage increase in state s. The sample is restricted to nonstudents. The outcome variable is either total debt, credit card debt, auto loan debt, or student loan debt. The x-axis corresponds to the number of quarters from a minimum wage increase. The circles correspond to the coefficient estimates. The vertical bars correspond to 95% confidence intervals. Standard errors are clustered at the state level. Quarter $\tau = -1$ is excluded as the reference level.

To understand whether enrolled or former students drive the reduction in student loan debt, we estimate the triple-differences model:

$$\begin{aligned} y_{i,t} = {} & \alpha + \beta \times \mathbf{E}_i \times \mathit{Treated}_s \times \mathit{Post}_{t,s} + \Gamma \times \mathit{Treated}_s \times \mathit{Post}_{t,s} \\ & + \delta_i + \delta_{p,t} + \delta_{\mathbf{E},t} + \gamma' X_{s,t-12} + \varepsilon_{i,t}, \end{aligned} \tag{3}$$

where the outcome variable is the student loan balances of bound employee i in month t. The dummy variable $\mathbf{E}_i$ is equal to one if bound employee i is an enrolled student and zero otherwise. The model includes the same individual fixed effects, cross-border county pair month effects, and control variables as in equation (2). In addition, the model includes separate month effects for enrolled and former students ($\delta_{\mathbf{E},t}$) to control for differential time trends across these two groups. Standard errors are again clustered at the state level.

Table V reports the coefficient estimates. For former students, we find no significant change in student loan balances after an increase in the minimum wage ($\Gamma = \$117$; $t = 0.92$). Instead, former students behave similar to nonstudents and use their wage gains to finance automobile purchases ($\Gamma = \$282$; $t = 2.50$). Consistent with former students facing binding credit constraints, we find that the increase in auto loan balances materializes around 12 to 18 months after a minimum wage change (Figure 2). Furthermore, consistent with the predictions of consumption-savings models that feature permanent wage increases and credit constraints, we find that former students have an implied MPB out of a minimum change of 0.11 (0.01 in the labor market subsample).

In contrast to former students, enrolled students reduce their student loan balances by \$1,025, on average, in response to an increase in their wages ($t = -2.64$). This reduction begins within one quarter of the treatment date and grows throughout the posttreatment period (Figure 3). Overall, we estimate that enrolled students have an implied MPB out of a minimum wage change of -0.32 (-0.33 in the labor market subsample).[13]

[13] Note that the weighted average of the coefficient estimates for the subsamples of enrolled, former, and nonstudents does not equal the baseline coefficient estimate in Table IV. For the most part, this difference stems from the combination of two factors. The first is that there is a small, but important, difference in the proportion of individuals who are enrolled students across treated states (27%) and controls states (30%). The second is that enrolled students have different student loan time trends than former students and nonstudents (i.e., enrolled students are in school and growing their student loan balances over the sample period, whereas former students and nonstudents are not). Given that there is a difference in the proportion of enrolled students across treated and control states, a portion of the differential enrolled-student time trend will appear in the baseline coefficient estimates. However, the subsample estimates purge these differential time trends through the inclusion of subsample-specific time fixed effects, which causes their weighted average to differ from our baseline difference-in-differences estimates. In particular, our baseline difference-in-differences estimates for student loan debt will be lower (i.e., more negative) than the weighted average of our subsample estimates because (i) there is a lower proportion of enrolled students in treated states than control states, and (ii) the enrolled student-specific time trend for student loan debt is both large and positive. Note, however, that this difference does not necessarily mean that our baseline estimates are biased or inconsistent (as opposed to due to sampling error). Indeed, the three percentage point difference in the proportion of enrolled students across treated and control states is insignificant ($t = -0.40$). Note also that if we were to calculate the overall MPB as the weighted average of the subsample MPBs, then our estimate of -0.05 ($= 0.05 \cdot 0.61 + 0.11 \cdot 0.10 - 0.32 \cdot 0.29$) would still be lower than predicted by standard consumption-savings models. Please see Section IV of the Internet Appendix for a full decomposition of our baseline estimate.

Table V

Enrolled Students Versus Former Students

This table contains coefficient estimates from equation (3). The dependent variable in columns (1) and (2) is total debt balances and open accounts. The dependent variable in columns (3) and (4) is credit card balances and open accounts. The dependent variable in columns (5) and (6) is auto loan balances and open accounts. The dependent variable in columns (7) and (8) is student debt balances and open accounts. In the top portion of the table, the sample is restricted to enrolled students and former students. In the bottom portion of the table, the sample is further restricted to just enrolled students. Standard errors, presented below the coefficient estimates, are calculated by clustering at the state level. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

	Total		Card		Auto		Student	
	Balances (1)	Accounts (2)	Balances (3)	Accounts (4)	Balances (5)	Accounts (6)	Balances (7)	Accounts (8)
Treated × Post	307**	0.12***	−2	0.03	282**	0.04***	117	0.05**
	(139)	(0.027)	(47)	(0.026)	(113)	(0.005)	(127)	(0.023)
Treated × Post × Enrolled student	−1,185**	−0.28***	29	−0.00	−82	−0.03***	−1,142***	−0.25***
	(507)	(0.074)	(66)	(0.042)	(76)	(0.006)	(426)	(0.053)
Employee FE	Y	Y	Y	Y	Y	Y	Y	Y
Border-pair month FE	Y	Y	Y	Y	Y	Y	Y	Y
Enrolled month FE	Y	Y	Y	Y	Y	Y	Y	Y
Control variables	Y	Y	Y	Y	Y	Y	Y	Y
N	1,446,763	1,446,763	1,446,763	1,446,763	1,446,763	1,446,763	1,446,763	1,446,763
R^2	0.87	0.85	0.79	0.83	0.64	0.69	0.88	0.85

	Enrolled student subsample: difference-in-differences coefficient estimates							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated × Post	−879*	−0.17**	27	0.03	200**	0.01	−1,025***	−0.20***
	(477)	(0.063)	(25)	(0.028)	(83)	(0.007)	(388)	(0.039)

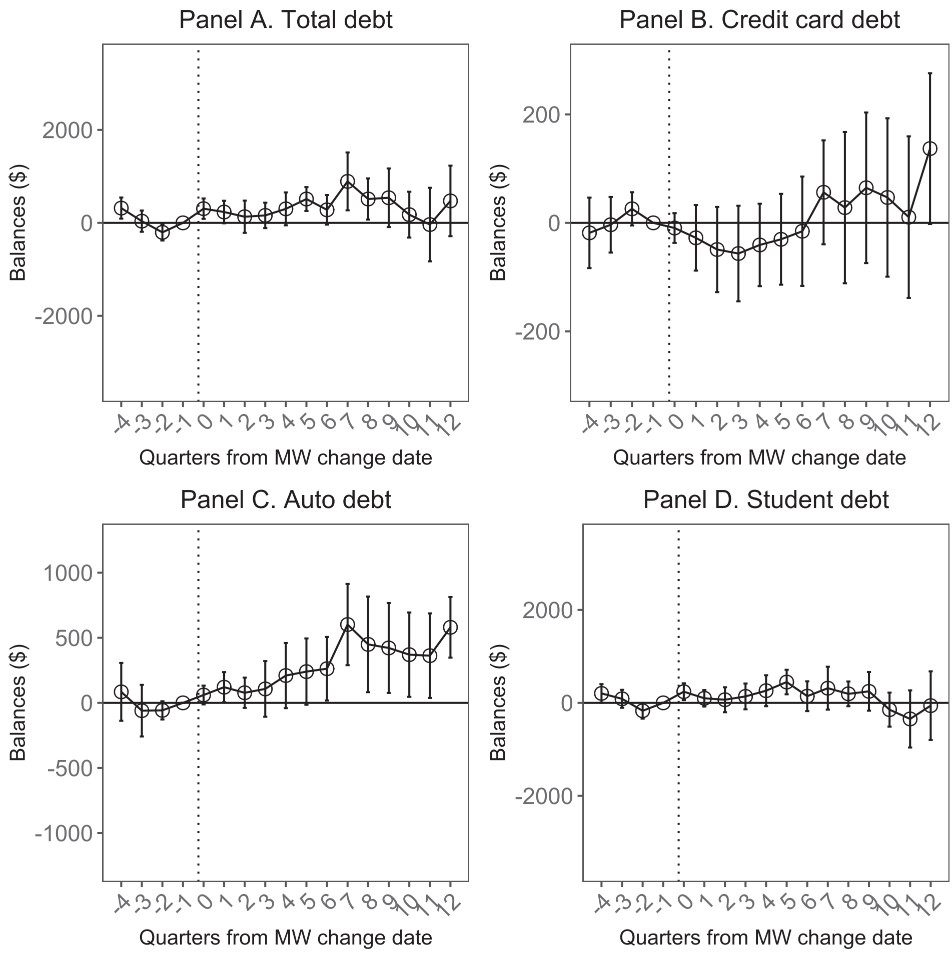

Figure 2. Debt response of former students. This figure plots coefficient estimates from a dynamic version of equation (2), $y_{i,t} = \alpha + \sum_{\tau=-4}^{12} \Gamma_\tau \times Treated_s \times D_{s,t,\tau} + \delta_i + \delta_{p,t} + \gamma' X_{s,t-12} + \varepsilon_{i,t}$, where $D_{s,t,\tau}$ is equal to one when quarter t is τ quarters away from a minimum wage increase in state s. The sample is restricted to former students. The outcome variable is either total debt, credit card debt, auto loan debt, or student loan debt. The x-axis corresponds to the number of quarters from a minimum wage increase. The circles correspond to the coefficient estimates. The vertical bars correspond to 95% confidence intervals. Standard errors are clustered at the state level. Quarter $\tau = -1$ is excluded as the reference level.

D. How Do Enrolled Students Reduce Their Student Loan Balances?

Throughout the rest of the paper, we focus on explaining the reduction in student loan debt among enrolled students. As a first step, we restrict our sample to enrolled students and examine whether the reduction in student loan debt comes from less borrowing or greater repayment. Table VI reports the coefficient estimates from equation (2) when the outcome variable is either

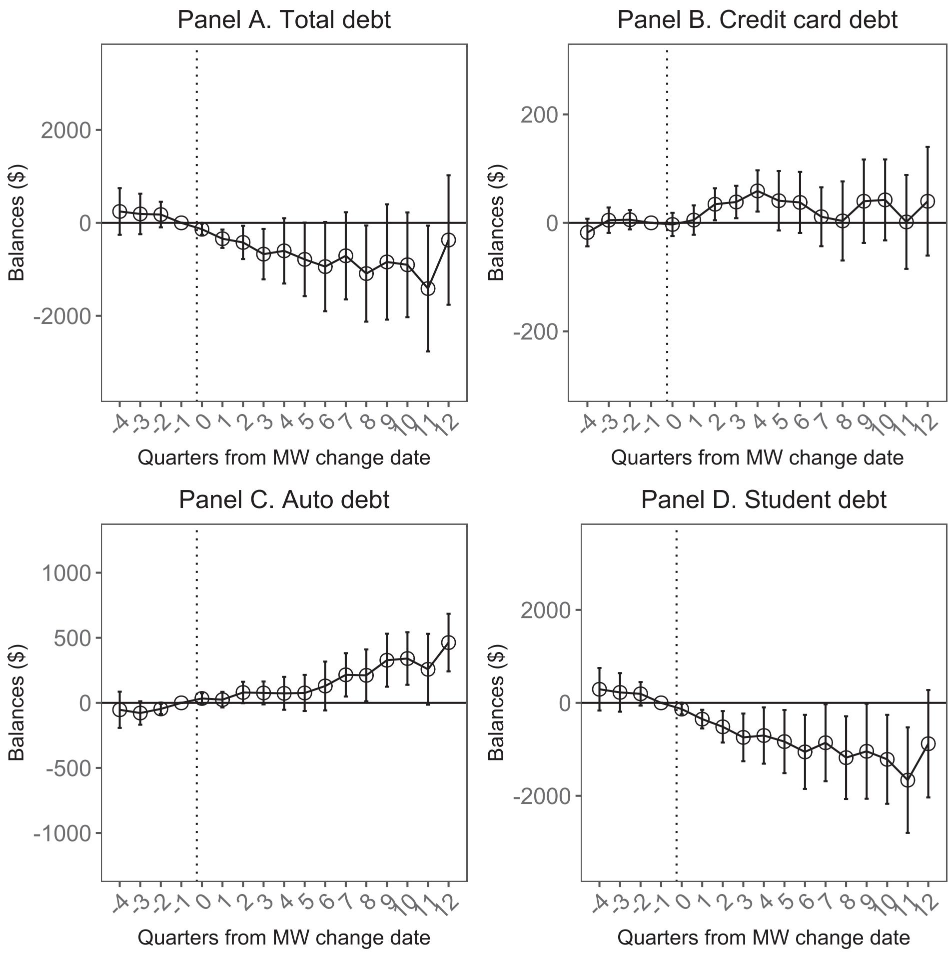

Figure 3. Debt response of enrolled students. This figure plots coefficient estimates from a dynamic version of equation (2), $y_{i,t} = \alpha + \sum_{\tau=-4}^{12} \Gamma_\tau \times Treated_s \times D_{s,t,\tau} + \delta_i + \delta_{p,t} + \gamma' X_{s,t-12} + \varepsilon_{i,t}$, where $D_{s,t,\tau}$ is equal to one when quarter t is τ quarters away from a minimum wage increase in state s. The sample is restricted to enrolled students. The outcome variable is either total debt, credit card debt, auto loan debt, or student loan debt. The x-axis corresponds to the number of quarters from a minimum wage increase. The circles correspond to the coefficient estimates. The vertical bars correspond to 95% confidence intervals. Standard errors are clustered at the state level. Quarter $\tau = -1$ is excluded as the reference level.

cumulative student loan borrowing or cumulative student loan payments (variable definitions are again provided in Table IA.V). Consistent with a reduction in borrowing driving our results, we find that enrolled students borrow $1,113 less in the 36 months after an increase in the minimum wage ($t = -3.62$) but do not increase their payments ($\Gamma = -\$32$; $t = -0.66$). Figure 4 plots the dynamics of the coefficient estimates. We find that borrowing declines within one

Table VI

How Do Enrolled Students Reduce Their Student Debt Balances?

This table contains coefficient estimates from equation (3). The dependent variable in column (1) is the cumulative amount of student debt borrowed since the beginning of the sample period. The dependent variable in column (2) is the cumulative amount of student loan payments made since the beginning of the sample period. The dependent variable in column (3) is the cumulative number of new student loans taken out since the beginning of the sample period. The dependent variable in column (4) is the amount of student debt borrowed conditional on taking out a new student loan. The dependent variable in column (5) is equal to one if an individual is still in their borrowing period and zero otherwise. The dependent variable in column (6) is equal to one if an individual takes out a new student loan in a given month and zero otherwise. In columns (1) through (4), the sample is restricted to enrolled students. In column (5), the sample is further restricted to individual-months in which an enrolled student takes out a new student loan. In column (6), the sample is restricted to enrolled students during their borrowing periods. Standard errors, presented below the coefficient estimates, are calculated by clustering at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

	Σ Borrowed (1)	Σ PMT (2)	Σ Loans (3)	Loan size (4)	Duration (5)	Borrowed? (6)
Treated × Post	−1,113***	−32	−0.35***	38	0.00	−0.003*
	(307)	(48)	(0.075)	(27)	(0.004)	(0.002)
Employee FE	Y	Y	Y	Y	Y	Y
Border-pair month FE	Y	Y	Y	Y	Y	Y
Control variables	Y	Y	Y	Y	Y	Y
N	1,070,176	1,070,176	1,070,176	82,953	1,070,176	476,517
R^2	0.76	0.68	0.78	0.57	0.72	0.17

quarter of the treatment date (but not before), and that this decline persists throughout the entire postperiod.

Next, we examine whether the reduction in student loan borrowing comes from the extensive margin (i.e., fewer new loans) or the intensive margin (i.e., smaller loan sizes). Column (3) in Table VI reports the coefficient estimates from our model when the outcome variable is the cumulative number of new student loan openings. We find that enrolled students open 0.35 fewer loans after a minimum wage change ($t = -4.70$). Column (4) repeats the estimation for the new loan amount conditional on borrowing. In contrast to loan openings, we find that enrolled students do not reduce their average loan amounts conditional on borrowing ($\Gamma = \$38$; $t = 1.44$). Therefore, enrolled students appear to be reducing their student loan balances through less borrowing along the extensive margin.[14]

[14] Are enrolled students reducing the duration of their enrollment, or are enrolled students simply able to avoid borrowing during some semesters? To answer this question, we use data on student loan origination dates up to 2020 to construct a borrowing period for each enrolled student in our sample. The borrowing period begins in the month that an enrolled student originates their first student loan and ends in the month that an enrolled student originates their last student loan. We then use the borrowing period as a measure of the duration of enrollment, and we reestimate our model with a dummy variable for whether an enrolled student is still in their borrowing period. As shown in column (5) in Table VI, we find no significant impact of the minimum wage on the

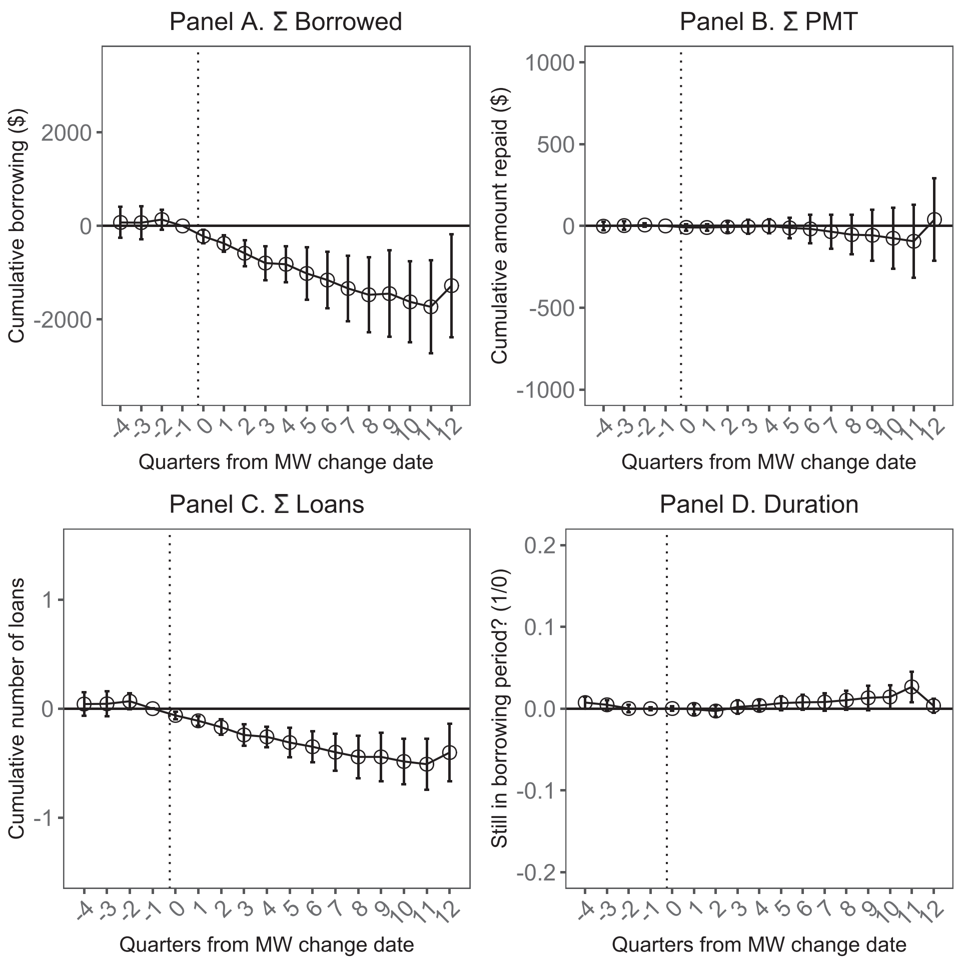

Figure 4. How do enrolled students reduce their student debt balances? This figure plots coefficient estimates from a dynamic version of equation (2), $y_{i,t} = \alpha + \sum_{\tau=-4}^{12} \Gamma_\tau \times Treated_s \times D_{s,t,\tau} + \delta_i + \delta_{p,t} + \gamma' X_{s,t-12} + \varepsilon_{i,t}$, where $D_{s,t,\tau}$ is equal to one when quarter t is τ quarters away from a minimum wage increase in state s. The sample is restricted to enrolled students. The outcome variable is either the cumulative amount of new student loan borrowing (Σ *Borrowed*), the cumulative amount of student loan payments made (Σ *PMT*), the cumulative amount of new student loans taken out (Σ *Loans*), or an indicator equal to one if an individual is still in their borrowing period (*Duration*). The x-axis corresponds to the number of quarters from a minimum wage increase. The circles correspond to the coefficient estimates. The vertical bars correspond to 95% confidence intervals. Standard errors are clustered at the state level. Quarter $\tau = -1$ is excluded as the reference level.

We can rationalize the results above with the following back-of-the-envelope calculation. The average loan size of a new student loan is \$3,208 during the pretreatment period. Therefore, if there is no change in average loan sizes during the posttreatment period, then opening 0.35 fewer loans should translate into an average \$1,123 reduction in student loan borrowing ($= -0.35 \times$ \$3,208). This back-of-the-envelope estimate is right around the \$1,113 reduction in student loan borrowing in Table VI, and it is also similar to the \$1,025 reduction in student loan balances in Table V.

E. Robustness

We perform several robustness tests. A brief description of each test is provided below.

E.1. Falsification

If our setting captures the causal effect of the minimum wage, then we should not expect to find significant debt responses in the upper parts of the wage distribution (i.e., there should be no effect of the minimum wage on high-income workers). Instead, all of our effects should be concentrated in the lower parts of the wage distribution around the new minimum wage. To ensure this is the case, we follow Cengiz et al. (2019) and reestimate equation (2) across the rest of the wage distribution. The stacked regression model is

$$y_{i,b,t} = \alpha + \sum_{b'=-1}^{b'=14} \Gamma_{b'} \times \mathit{Treated}_s \times \mathit{Post}_{t,s} \times \mathit{Bin}_{b'} + \delta_i + \delta_{p,b,t} + \gamma_b' X_{s,t-12} + \varepsilon_{i,b,t}, \tag{4}$$

where the outcome variable, $y_{i,b,t}$, is either total debt, credit card debt, auto loan debt, or student loan debt (all scaled by pretreatment income) of employee i in wage bin b in month t.[15] The dummy variable $\mathit{Bin}_{b'}$ is equal to one if employee i is in wage bin $b = b'$ and zero otherwise. For each wage bin b, the model includes a separate set of cross-border pair month effects and a different set of coefficients for the state-level control variables. The coefficients of interest are

duration of enrollment. Instead, we find that enrolled students borrow fewer times during their borrowing period (column (6)).

[15] We define the wage bins as follows. Bin $b = -1$ corresponds exactly to the “old” minimum wage. Bin $b = 0$ corresponds to the interval between the old minimum wage and the new minimum wage, that is, the interval $(\mathrm{MW}_s, \mathrm{MW}_s + \Delta_s)$, where Δ_s is the size of the minimum wage increase (or hypothetical increase for control states) in state s. Finally, bin $b \geq 1$ corresponds to the wage interval between b and $b+1$ increments of size Δ_s above the old minimum wage: $[\mathrm{MW}_s + b \cdot \Delta_s, \mathrm{MW}_s + (b+1) \cdot \Delta_s)$. Intuitively, bins $b = -1$ and $b = 0$ correspond to bound employees while bins $b \geq 1$ correspond to nonbound employees. We cap the wage bins at $b = 14$; the corresponding wage interval is $[\mathrm{MW}_s + 14 \cdot \Delta_s, \infty)$. To estimate the model, we supplement our baseline sample with 100,000 randomly selected individuals earning above the new minimum wage during the pretreatment period.

the $\Gamma_{b'}$s, which measure the average relative change in the outcome variable for employees in each wage bin.

Figure IA.8 plots the coefficient estimates. Consistent with our results capturing the causal effect of the minimum wage, we find that our debt responses are concentrated in the lower parts of the wage distribution. The fact that we find no meaningful responses in the upper parts of the wage distribution helps us rule out several alternative explanations. For instance, if concomitant state-level policies or state-level differences in the composition of lenders were driving our results, then we would expect to find an impact on workers earning above the new minimum wage (which we do not). We note that estimating the effects of the minimum wage across the entire wage distribution generalizes the within-state triple-differences approach used in Clemens and Wither (2019).

E.2. Alternative Definitions of Enrolled Students

Given that we do not observe enrollment dates in the credit data, our definition of enrolled students is subject to classification error. To examine the importance of this classification error, Table IA.VIII reports results of our model using three alternative definitions of enrolled students. Under our first alternative definition, an enrolled student is an individual whose student loans are in deferment during the pretreatment period. Under our second alternative definition, an enrolled student is an individual who took out their first student loan sometime during the 24 months prior to the treatment date. Finally, under our third alternative definition, our original sample of enrolled students is restricted to those between the ages of 18 and 22. For each alternative definition, we find similar results as in our baseline analysis.

E.3. Standard Errors and Fixed Effects

In Tables IA.IX and IA.X, we reestimate our models using different clustering schemes and fixed effects. The purpose of these tests is to examine whether our results are robust to different assumptions about the standard errors (Cameron and Miller (2015)) and whether our coefficient estimates are sensitive to the inclusion of additional controls (Oster (2019)). We find that our main results do not change in both cases.

E.4. Transformations of the Outcome Variables and Poisson Regression

In Table IA.XI, we report coefficient estimates from models that apply log and inverse hyperbolic sine transformations to the outcome variables.[16] We find that our main results do not change: Enrolled students reduce their student debt while former and nonstudents increase their auto loan balances.

[16] The log transformation is $\log(1+y)$. The inverse hyperbolic sine transformation is $\log(y+\sqrt{y^2+1})$.

In Table IA.XII, we reestimate our baseline model using a Poisson regression. We use the Poisson regression to account for the left-censoring of debt balances at zero (Cohn, Liu, and Wardlaw (2022)). We again find that our main results do not change.

In Table IA.XIII, we report estimates with changes in debt as the outcome variable (i.e., the impulse response coefficients). The estimated 36-month cumulative decline in debt is \$1,235. This estimate falls within the 95% confidence interval of our baseline estimate.

E.5. Alternative Pairings of Adjacent Treated and Control Counties

In Table IA.XIV, we follow Dube, Lester, and Reich (2010) and reestimate our difference-in-differences model with duplicate observations for counties with multiple cross-border matches. For this test, our sample includes 676 counties (inclusive of duplicates), 338 unique cross-border pairs, and 7,899,812 individual-month observations. The estimation includes a separate month fixed effect for each cross-border pair, and we double-cluster our standard errors at the state and cross-border pair levels. Overall, we find no material changes in our coefficient estimates when we incorporate duplicate observations. For example, the estimated decline in student loan debt is −\$944 ($t = -3.26$) in our baseline sample and −\$932 ($t = -3.14$) in our stacked sample with duplicates.

In Figure IA.9, we reestimate our model using 1,000 random assignments of treated counties to cross-border control counties. The purpose of this test is to demonstrate that, in situations in which treated counties border multiple control counties, our baseline choice of control counties does not have a material impact on our results. We find that our baseline estimates are not abnormal relative to estimates from other possible assignments. For example, the \$944 decline in student loan debt in our baseline sample falls within the 75$^{\text{th}}$ percentile of its empirical distribution.

V. Why Do Enrolled Students Borrow Less?

From a consumption-savings standpoint, the reduction in student loan debt is surprising. Enrolled students have high expected future earnings but have limited options for smoothing their consumption because of credit market frictions (Cadena and Keys (2013)). Hence, enrolled students should have strong incentives to spend in response to an increase in their wages. For enrolled students to prefer to save their wage gains, the benefits of borrowing less student debt must exceed the benefits of increasing and smoothing their consumption. Moreover, the benefits of borrowing less student loan debt must also exceed both the risk-free interest rate earned from saving and the shadow interest rate earned from reducing their use of nonstudent debt (Becker and Shabani (2010)). We note that Zinman (2015) finds that student loans determine the "true" risk-free rate for fewer than 5% of households.

In this section, we examine potential rational explanations for the decline in student loan debt. Throughout the section, we restrict our sample to enrolled students.

A. The Financial Constraints Channel

Enrolled students might choose to borrow less student debt to improve their credit scores and hedge against future financial constraints (Rothstein and Rouse (2011), Dettling and Hsu (2018)).[17] To test whether such motives drive our results, we split our sample into credit-constrained students (pretreatment credit score ≤ 620; 52% of the sample) and unconstrained students (pretreatment credit score > 620; 48% of the sample) and estimate the triple-differences model

$$y_{i,t} = \alpha + \beta \times \mathbf{C}_i \times \mathit{Treated}_s \times \mathit{Post}_{t,s} + \Gamma \times \mathit{Treated}_s \times \mathit{Post}_{t,s} + \delta_i + \delta_{p,t} + \delta_{\mathbf{C},t} + \gamma' X_{s,t-12} + \varepsilon_{i,t}, \tag{5}$$

where the outcome variable, $y_{i,t}$, is the student loan balances or open student loan accounts of enrolled student i in month t. The dummy variable $\mathbf{C}_i$ is equal to one if enrolled student i is credit constrained and zero otherwise. Standard errors are clustered at the state level.

Columns (1) and (5) in Table VII report the coefficient estimates. Inconsistent with financial constraints driving our results, we find that both unconstrained and constrained students reduce their borrowing in response to an increase in their wages ($\beta = -\$312$; $t = -1.00$). We also find that enrolled students do not use their wage gains to improve their credit scores (Table IA.XV). This is consistent with the finding in Dettling and Hsu (2021) that a \$1 increase in the minimum wage results in a credit score improvement of just 1.6 points.

B. The Bankruptcy Channel

One distinguishing feature of student debt is that it is much more difficult to discharge in bankruptcy than other forms of consumer credit. All else equal, this feature could increase the cost of student debt and raise its effective interest rate, which in turn could encourage enrolled students to reduce their use of it in response to an increase in their wages.

To examine whether the inability to discharge student loan debt drives our results, we conduct two tests. In our first test, we reestimate equation (5) across the ex ante likelihood that an enrolled student will go bankrupt in the

[17] Student debt imposes a significant consumption commitment after graduation (Chetty and Szeidl (2007)), and the presence of this commitment can induce drastic reductions in other forms of consumption (e.g., food) or lead to default in response to negative income shocks. Thus, enrolled students who anticipate being credit constrained might choose to avoid student debt when possible. Recent evidence also suggests that individuals view student debt as an impediment to home ownership (Mezza et al. (2020)).

Table VII

Why Do Enrolled Students Borrow Less Student Debt?

This table contains coefficient estimates from equation (5). The dependent variable in columns (1) through (4) is student loan balances. The dependent variable in columns (5) through (8) is open student loan accounts. The sample is restricted to enrolled students. Standard errors, presented below the coefficient estimates, are calculated by clustering at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

	Balances				Accounts			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated * Post	−851**	−945***	−911**	−867**	−0.19***	−0.15***	-0.16***	−0.18***
	(417)	(288)	(451)	(396)	(0.052)	(0.036)	(0.037)	(0.045)
Treated × Post × Credit constrained	−312				−0.03			
	(312)				(0.058)			
Treated × Post × High credit utilization		88				0.06		
		(260)				(0.056)		
Treated × Post × High bankruptcy risk			−166				−0.05	
			(272)				(0.036)	
Treated × Post × Low graduation rate				192				0.05
				(810)				(0.099)
Employee FE	Y	Y	Y	Y	Y	Y	Y	Y
Border-pair month FE	Y	Y	Y	Y	Y	Y	Y	Y
Cross-section month FE	Y	Y	Y	Y	Y	Y	Y	Y
Control variables	Y	Y	Y	Y	Y	Y	Y	Y
N	1,070,176	1,070,176	1,070,128	1,070,176	1,070,176	1,070,176	1,070,128	1,070,176
R^2	0.88	0.88	0.87	0.88	0.85	0.85	0.85	0.85

future. The basic idea of this test is as follows. If an enrolled student perceives themself to be at low risk of going bankrupt, then the inability to discharge student loan debt should be less of a concern, and the effective cost of student loans should not be too high (relative to the actual interest rate). As a result, the enrolled student will not be too eager to reduce their student loan borrowing in response to an increase in the minimum wage. However, if an enrolled student perceives themself to be at high risk of going bankrupt, then the inability to discharge student loan debt should become more of a concern. Under this channel, the enrolled student should then attribute a higher effective cost to student debt, and this will push the enrolled student to reduce their use of it following an increase in their wages.

To measure the likelihood of going bankrupt, we use Equifax's "bankruptcy score." The bankruptcy score differs from the standard credit score along three dimensions. First, the bankruptcy score is optimized to predict the likelihood of filing for bankruptcy as opposed to the likelihood of defaulting on a credit line. Second, the bankruptcy score uses an expanded set of predictive attributes relative to the credit score, and it also emphasizes different attributes in its predictions (e.g., credit utilization and balances receive higher weights). Third, higher values of the bankruptcy score correspond to higher likelihoods of going bankrupt, whereas lower values of the credit score correspond to higher likelihoods of defaulting.

Using pretreatment values of the bankruptcy score, we split our sample into two groups: (i) enrolled students with above-median bankruptcy scores (high bankruptcy risk) and (ii) enrolled students with below-median bankruptcy scores (low bankruptcy risk). We then reestimate equation (5) across this sample split. Columns (3) and (7) in Table VII report the coefficient estimates from the model. Inconsistent with the bankruptcy channel driving our results, we find no differential reduction in student loan balances across pretreatment values of the bankruptcy score ($\beta = -\$166$; $t = -0.61$). However, we note that the triple-differences coefficient estimate is in the direction that this channel would predict.

In our second test, we reestimate equation (2) with the bankruptcy score as the outcome variable. If bankruptcy concerns are driving our results, then we should expect enrolled students to use their wage gains to reduce their bankruptcy risks. However, as shown in Table IA.XV, we find no significant reduction in bankruptcy scores following an increase in the minimum wage ($\Gamma = 1$; $t = 1.34$). We also find no significant changes in other measures of financial distress, such as whether an enrolled student is in default on a credit product.

Before we proceed, we highlight two issues that prohibit us from drawing strong conclusions from these tests. The first issue is that the theoretical mechanism being tested revolves around the perceived risk of going bankrupt, but this perceived risk is unobservable and therefore subject to measurement error. For instance, if the bankruptcy score is a poor measure of the perceived risk of going bankrupt, then our tests could have low power, and our triple-differences coefficient estimate in Table VII could suffer from attenuation bias. The second issue is that enrolled students could also differ based on their perceived costs of not being able to discharge student debt, conditional on declaring bankruptcy. The inability to observe this perceived cost introduces additional measurement error and further reduces the power of our tests. In Section VI below, we examine a related behavioral channel when we build and calibrate our structural model of student debt.

C. The Default Costs Channel

Another distinguishing feature of student loan debt is that its default costs are, at first glance, higher than other forms of unsecured consumer credit. For instance, the wage garnishment laws applicable to student debt are determined at the federal level, and these laws supersede state and local wage garnishment laws that are often weaker. All else equal, higher default costs could encourage enrolled students to reduce their use of student debt after an increase in their wages.

To examine whether default costs drive our results, we reestimate equation (5) across state-level four-year college graduation rates (median = 58%). The basic idea behind this test is that enrolled students with lower graduation rates should have greater income risk and a higher chance of default. Therefore, if the default-costs channel drives our results, enrolled students with lower graduation rates and higher default risk should have stronger incentives to reduce their student loan borrowing (Brown, Fang, and Gomes (2012)).

Columns (4) and (8) in Table VII report the estimates. Inconsistent with the default-costs channel, we find that the reduction in student debt does not depend on graduation rates ($\beta = 192$; $t = 0.24$). We also find similar results when we reestimate our model across other state-level measures of income risk, such as the GINI coefficient of wages (Table IA.XVI).

Before we proceed, we note that federal student loan debt offers an income-based repayment (IBR) option that is particularly attractive to low-wage workers. Under the current structure of IBR plans, the average low-wage worker in our sample would owe zero dollars per month on their student loans (Mueller and Yannelis (2022)).[18] The option to enter into an IBR plan, along with forbearance and economic hardship deferments, reduces the cost of student loans and serves as further evidence against the default-costs channel.[19]

[18] Individuals are enrolled in IBR plans can have their student loans forgiven after 20 or 25 years. In general, loan amounts forgiven under IBR programs are considered taxable income. However, if an individual is considered insolvent when their loan is forgiven, then the loan amount forgiven would not be considered taxable income or would be taxed at a partial rate (see Section III of the Internet Appendix). It is reasonable to assume that most of the low-wage workers in our sample would be considered insolvent or at least have their loans taxed at a partial rate. We note that the American Rescue Plan includes an income tax exclusion for all student loan discharges through December 31, 2025. We also note that IBR plans reduce the consumption risk from income fluctuations. In essence, the lender is willing to share in the borrower's income risk and convert the debt claim to an equity claim in bad states of the world.

[19] If default costs explain our results, then we should expect former students who are in default to use an increase in their incomes to become current on their debt. Table IA.XVII, however, shows

D. The Buffer-Stock Channel

Enrolled students might use student debt to create a cash buffer against uncertain future expenses, such as hospital bills or car repairs. As the wages of enrolled students rise, the demand for such buffer-stock borrowing (and hence student loan debt) might decline. To examine whether buffer-stock borrowing drives our results, we reestimate equation (5) across the conditional median credit utilization (ex ante conditional median = 46%). The basic idea behind this test is that individuals with high credit card utilization should be more reliant on student debt to hedge against uncertain future expenses. Therefore, if buffer-stock behavior explains our results, then the reduction in student loan borrowing should concentrate among enrolled students with higher credit card utilization.

Columns (2) and (6) in Table VII report the coefficient estimates. Inconsistent with buffer-stock motives driving our results, we find no differential reduction in student loan debt across credit card utilizations ($\beta = \$88$; $t = 0.34$). Enrolled students with both high and low credit card utilization borrow less student debt after an increase in their wages.

E. Other Rational Channels

We examine several other rational channels. None can explain the reduction in student debt among enrolled students. A brief description of each channel is provided below.

E.1. Labor Supply and College Enrollment

For low-wage workers, an increase in the minimum wage increases the opportunity cost of attending college. As a result, enrolled students might choose to spend more time at work and less time in school, thereby borrowing less student debt. Inconsistent with this channel, however, we find no significant changes in employment ($\Gamma = 0.001$; $t = 1.25$) or average hours worked per week ($\Gamma = -0.01$; $t = -0.08$) for enrolled students. We also find no significant reductions in the length of time that students remain enrolled (see Table VI), and we find no significant changes in the number of students who are enrolled in colleges that are located in treated counties (Figure IA.10). Taken together, these results suggest that changes in labor supply and college enrollment do not explain the decline in student loan debt.

E.2. Tuition Costs

If increases in the minimum wage coincide with reductions in college tuition rates, then the decline in student loan debt could be due to a simple price effect. To test whether a reduction in college tuition explains our results, we

this is not the case. Former students in default do not repay student debt more aggressively or exit default more often.

reestimate equation (2) after controlling for lagged county-level tuition (inclusive of fees). We find that controlling for tuition does not affect our results ($\Gamma = -\$1{,}131$). We also find that county-level tuition rates do not change in response to an increase in the minimum wage (Figure IA.11). Taken together, these results suggest that a decline in tuition does not explain the reduction in student debt.

E.3. Savings and Credit Card Balances

Borrowing less student debt can be thought of as saving at the student loan interest rate (Becker and Shabani (2010)). If student loan interest rates are high enough, then some enrolled students might find it optimal to borrow less student debt following an increase in their wages. To test this savings-based explanation, we reestimate equation (5) across the level of pretreatment credit card balances. The basic idea is that enrolled students concerned with savings should prioritize reducing their credit card balances over borrowing less student debt because credit cards have much higher interest rates. Inconsistent with this savings-based explanation, however, in Table IA.XVIII, we find similar reductions in student loan debt for enrolled students with above-conditional-median credit card debt ($\Gamma = -\$949$; $t = -2.33$) and below-conditional-median credit card debt ($\beta = -\$244$; $t = -0.56$).[20] Moreover, we find that enrolled students with above-median credit card debt do not reduce their credit card balances more than enrolled students with below-median credit card debt ($\Gamma = -\$22$; $t = 0.91$; $\beta = -\$17$; $t = -0.27$). We note that the choice of enrolled students to borrow less student debt instead of paying down credit card balances also helps rule out several explanations centered around the permanence of the wage increase.

E.4. Student Age

Student debt can impede major life events such as marriage (Gicheva (2009)), child birth (Perry, Karamcheva, and Yannelis (2021)), and enrolling in a graduate program (Chakrabarti et al. (2023)). If these major life events are still on the horizon, then an enrolled student might find it optimal to borrow less student debt following an increase in their wages. To test this explanation, we reestimate equation (5) across younger (< 22 years) and older (≥ 22 years) students (median age $= 21$ years; see Figure IA.12). Inconsistent with an age- or life-cycle–related explanation, we find that the reduction in student debt is present in both younger enrolled students ($\Gamma = -\$931$; $t = -2.14$) and older enrolled students ($\beta = \$114$; $t = 0.19$).

[20] The conditional median credit card debt is \$2,940, or 25% of the average income in our sample. Thus, these are likely interest-accruing balances and not just transactions that are paid off each month.

E.5. IBR Plans and Wage Garnishment Laws

A significant fraction of student loan borrowers are enrolled in IBR plans that tie their monthly loan payments to their earnings (Perry, Karamcheva, and Yannelis (2021)). If an increase in the minimum wage leads to an increase in earnings, then monthly student loan payments could mechanically increase, and hence student loan balances could decrease. However, this mechanical link cannot explain our results for two reasons. The first reason is that we find that the reduction in student loan balances comes from less borrowing and not greater repayment. The second reason is that enrolled students' earnings are too low to have to make payments under an IBR plan (Mueller and Yannelis (2022)). For the average low-wage worker in our sample, their monthly payment would be $0 under an IBR plan.[21] We also note that the income-contingent nature of wage garnishment laws cannot explain our results for similar reasons.

E.6. Student Loan Borrowing Limits and Need-Based Aid

Subsidized loan limits depend on student earnings. If an increase in the minimum wage leads to an increase in earnings, then some enrolled students might experience reductions in their subsidized loan limits and hence their level of subsidized borrowing. However, reductions in student loan limits cannot explain our results because overall loan limits are not need based. As earnings rise, each dollar of subsidized loans lost is replaced with an additional dollar of unsubsidized loans with the same nominal interest rate (Black et al. (2020)). For similar reasons, reductions in other forms of need-based aid cannot explain our results or would lead to the opposite predictions.

E.7. Habits

In consumption-savings models with habits, the initial marginal propensity to consume out of a minimum wage change is often less than one (Carroll (2001)). Moreover, this initial but fading savings response could materialize as a reduction in student loan debt if the interest rate on risk-free savings is low enough. However, habits alone cannot explain the differential borrowing responses across enrolled students, former students, and nonstudents (the latter two of which have an MPC above one). In addition, habits cannot explain why enrolled students choose to borrow less student loan debt instead of paying down their credit card balances.

[21] IBRs set monthly payments equal to either 10% or 15% of the difference between an individual's annual income and their discretionary income, that is, 150% of the federal poverty limit ($19,320 in 2020). This payment is capped below at zero. Because the discretionary incomes of most bound employees in our sample are equal to zero (both before and after the minimum wage change), the average bound employee would owe $0 each month under an IBR plan. We note that IBRs have a similar structure as wage garnishments. Conditional on being in default, wage garnishment laws require that borrowers allocate 15% of their disposable income (i.e., income above $217.50 per week) toward repaying their debts.

E.8. Precautionary Motives

Meer and West (2016) and Gopalan et al. (2021) find that companies hire fewer low-wage workers in response to an increase in the minimum wage. Given that a slowdown in hiring makes low-wage workers' future earnings more volatile (e.g., due to longer job search times), enrolled students might find it optimal to save a portion of their wage gains for precautionary reasons (Carroll (1997)). Similar to habits, however, precautionary motives cannot explain the borrowing responses of former students and nonstudents. If anything, precautionary motives would predict that former students and nonstudents would save even more than enrolled students following a minimum wage change (i.e., because these individuals are career low-wage workers). Precautionary motives also cannot explain why enrolled students choose to borrow less student debt instead of paying down credit card debt.

E.9. Proposed Loan Amounts

Most colleges and universities provide students with financial aid award letters that contain proposed loan amounts. In contrast to overall loan limits, these proposed loan amounts might depend on student earnings. If students are attention constrained and borrow whatever the proposed loan amount is, then an increase in the minimum wage could lead to a mechanical reduction in proposed loan amounts and in turn a reduction in borrowing. Furthermore, the reduction in student loan borrowing would be concentrated among enrolled students and not former students.

To examine whether reductions in proposed loan amounts drive our results, we reestimate equation (2) on the subsample of enrolled students with on-campus jobs. Our proxy for an on-campus job is whether an enrolled student works for a college or university. The basic idea behind this test is that the financial aid office should be more aware of how much an enrolled student earns when the student works at the college. Hence, enrolled students with on-campus jobs should be more likely to have their proposed loan amounts reduced in response to an increase in the minimum wage. However, inconsistent with reductions in proposed loan amounts driving our results, we find that enrolled students with on-campus jobs experience a smaller reduction in student loan debt ($\Gamma = -\$682$; $t = -0.83$) than enrolled students in general ($\Gamma = -\$1{,}025$; $t = -2.64$).[22]

[22] The conclusions that we can draw from this test are limited. Perhaps the main reason is that we have only 373 enrolled students with on-campus jobs in treated states, and these enrolled students cluster within only three states: NE (166), MD (99), and MA (48). Another reason is that almost 90% of the 2,391 enrolled students with on-campus jobs in control states reside in either PA (1,870) or TN (223). We note that the proposed loan amount channel is somewhat inconsistent with our null findings on average loan amounts along the intensive margin.

E.10. Cash-Based Motives

A significant portion of student loan borrowing is used to cover living expenses that must be paid in cash (e.g., rent). If enrolled students target a certain dollar amount of cash between their earnings and borrowing each semester, then an increase in the minimum wage could result in a mechanical reduction in student loan debt. To examine whether such cash-based motives explain our results, we reestimate equation (5) across three individual-level proxies of ex ante cash demand: (i) having a credit card, (ii) having an outstanding installment loan, and (iii) living at home with their parents (proxy = below the age of 22 and residing at an address where at least one individual above the age of 40 also resides). Inconsistent with cash-based motives driving our results, we find similar reductions in student loan borrowing across sample splits based on all three measures of ex ante cash demand.[23]

VI. Student Debt Aversion

Given that the rational channels above are unable to explain our results, we next consider behavioral explanations for the decline in student loan debt. To start, we ask the following question: How high does the perceived interest rate on student loan debt need to be to rationalize the borrowing response of enrolled students? Knowing the answer to this question is useful because the gap between the perceived interest rate and the actual interest rate serves as an intuitive measure of the disutility associated with student loan debt. Below, we use the term *student debt aversion* to describe general forms of such disutility that are increasing functions of the amount of student loan debt.

To answer the question, we start with a two-step, nonstructural approach. In the first step, we use the debt response of nonstudents to model the MPB as a function of marginal borrowing costs. Specifically, for each nonstudent in our sample, we use their pretreatment credit score to assign them to one of four borrowing cost groups. These groups, which are reported in Table IA.XIX, are based on the mapping between credit scores and average credit card interest rates from Agarwal et al. (2018) and the Consumer Financial Protection Bureau. Next, we reestimate our baseline difference-in-differences model within each group. The coefficient estimates from these four models then provide us with an idea of how the MPB of nonstudents behave as a function of borrowing costs. For instance, if we consider the first and second borrowing cost groups in Table IA.XIX, then a one percentage point decline in interest rate is associated with a $145 increase in the average debt response.

[23] We assume that ex ante cash demand should be higher when an individual does not have a credit card, has an outstanding installment loan, or does not live at home with their parents. The triple-difference coefficient estimates for our three proxies are as follows: (i) has a credit card ($\beta = -\$211$; $t = -0.57$), (ii) has an open installment loan ($\beta = \$377$; $t = 0.70$), and (iii) is living at home ($\beta = \$338$; $t = 1.14$).

In the second step of our approach, we use the shape of the implied MPB to recover the perceived interest rate on student loan debt. Here, the implicit assumption is that the MPB of nonstudents can be extrapolated to enrolled students. To recover the perceived interest rate, we use the \$145 slope from above to move the average debt response of \$52 in the first borrowing cost group to the average student debt response of −\$1,025 for enrolled students. We find that borrowing costs would need to rise 7.5 percentage points from their base to move the debt response from \$52 to −\$1,025. Given that the initial interest rate in the first borrowing cost group is 23.1%, our nonstructural approach implies that the perceived interest rate on student loan debt needs to be at least 30.6% (= 23.1 + 7.5) to rationalize the borrowing response of enrolled students.

The exercise above suggests that enrolled students perceive student loans to be much costlier than what is implied by the actual interest rate. This high perceived cost reflects a general disutility associated with student loan debt, a behavior that we refer to as student debt aversion (Field (2009)). To complement our nonstructural approach and to formalize the notion of student debt aversion, we build and calibrate a consumption-savings model. The model serves three purposes. First, the model allows us to formalize student debt aversion as a utility cost of debt that is an increasing function of the amount of student debt. As we discuss below, this function can be formulated in several ways, including through a high perceived student loan interest rate, a direct utility penalty per dollar of student debt, or a combination of both. Second, the model allows us to measure the level of student debt aversion under each formulation that is consistent with the behavior of enrolled students. Third, we can use the calibrated model to test whether student debt aversion is consistent with other features of the data, such as the cross-sectional patterns found in Section V.

Albeit helpful, the structural approach is not without its own flaws.[24] We therefore introduce mental accounting as an alternative behavioral explanation outside the model.

A. A Structural Model of Enrolled Students

We consider a consumption-savings model with initial indebtedness and labor income risk. Let c_t denote the consumption of an enrolled student in period t (measured in years). In each period, the enrolled student chooses current and

[24] The nonstructural approach also has its own set of technical drawbacks. The first is that we do not have a source of quasi-exogenous variation for the cost of borrowing. This generates an additional correlated omitted variables problem to contend with: Higher borrowing costs should induce lower MPBs all else equal, but higher borrowing costs could correlate with credit constraints or credit rationing, which should induce higher MPBs. The second drawback is that the nonstructural approach is subject to extrapolation bias. For instance, we must assume that the MPB at a given interest rate is the same for both nonstudents and enrolled students, despite there being several differences between nonstudents and enrolled students that might affect the MPB (e.g., differences in expected future incomes).

future consumption to maximize expected lifetime utility,

$$\mathrm{E}_t \sum_{i=0}^{T-t} \beta^i \cdot u(c_{t+i}), \tag{6}$$

where β is the subjective discount factor, T is the planning horizon, E_t is the expectation operator conditional on information at time t, and $u(\cdot)$ is the one-period utility function with coefficient of relative risk aversion γ:

$$u(c_t) = \frac{c_t^{1-\gamma} - 1}{1-\gamma}. \tag{7}$$

The enrolled student begins the model working in a minimum wage job that provides a risky income stream of y_t per period. The enrolled student also has an initial debt load of b_0, for which the per-period interest rate is r.[25] Given an amount of debt d_t borrowed or repaid in period t, the intertemporal budget constraint is

$$c_t = y_t + d_t, \tag{8}$$

and the stock of debt evolves as

$$b_t = \big(b_{t-1} + d_t\big) \cdot (1 + r), \tag{9}$$

where we assume b_t is bounded below at zero. Following Zeldes (1989), we assume that the enrolled student cannot default on their debt. Therefore, the terminal condition

$$c_T = y_T - b_{T-1} \geq 0 \tag{10}$$

must also hold with probability one.[26]

Finally, the income process is

$$y_t = y_t^* + \ddot{y}_t, \tag{11}$$

where y_t^* denotes the life-cycle component of income and $\ddot{y}_t$ denotes the graduation component. The life-cycle component of income has a deterministic component and a stochastic component:

$$y_t^* = \bar{y}_t \cdot \delta(\omega_t). \tag{12}$$

The deterministic component is

$$\bar{y}_t = \begin{cases} y_0 \cdot \prod_{i=1}^{t} (1 + \mathbf{g}_t) & \text{if } t \leq T' \\ y_0 \cdot \prod_{i=1}^{T'} (1 + \mathbf{g}_t) & \text{otherwise,} \end{cases} \tag{13}$$

[25] The debt in our model represents student debt. Introducing another form of unsecured debt and limiting student loan borrowing to an enrollment period does not affect our results.

[26] Given our baseline parameter values, the $b_t \geq 0$ constraint never binds along the equilibrium path. The terminal no-default constraint is satisfied whenever the stock of debt is bounded above at the present value of future labor income along the worst possible income path.

where $\mathbf{g}_t$ is the income growth rate in period t, and T' marks the end of the income growth period ($0 < T' < T$). The stochastic component, δ, follows a discretized Rouwenhorst AR(1) process with volatility σ and autocorrelation parameter ρ across K states of the world:

$$\delta(\omega_t) = \begin{cases} \delta_1 & \text{if } \omega_t = 1 \\ \delta_2 & \text{if } \omega_t = 2 \\ \dots & \dots \\ \delta_K & \text{if } \omega_t = K \end{cases}, \tag{14}$$

where $\delta_1 < \delta_2 < \ldots < \delta_K$ and $\omega_t = 1, \ldots, K$ denotes the realized state in period t.

The graduation component of income depends on an absorbing state variable π_t that captures graduation from college. Graduation can occur with probability q during any of the first τ periods of the model. If the enrolled student graduates ($\pi_t = 1$), then their permanent income increases by $\Upsilon > 0$. If the student does not graduate during the first τ periods ($\pi_t = 0$), then they "drop out" and do not experience an increase in income. The graduation component of income is thus

$$\ddot{y}_t = \begin{cases} 0 & \text{if } \pi_t = 0 \\ \Upsilon & \text{if } \pi_t = 1. \end{cases} \tag{15}$$

A.1. Minimum Wage Changes

To examine how wage gains affect the enrolled student's debt decisions, we simulate the model with and without a minimum wage hike. We model the minimum wage hike as an unexpected $\Delta\%$ increase in the life-cycle component of income for enrolled students who have not (yet) graduated. Formally, the income process in the presence of the minimum wage hike is

$$y_t' = y_t^* \cdot (1 + m_t) + \ddot{y}_t, \tag{16}$$

where m_t captures the minimum wage hike:

$$m_t = \begin{cases} \Delta & \text{if } \pi_t = 0 \\ 0 & \text{if } \pi_t = 1. \end{cases} \tag{17}$$

Once an enrolled student graduates, the minimum wage hike no longer affects their earnings.

A.2. Calibration

We calibrate the model to the parameter values listed in Table VIII. For each parameter value, we list its source in the right-most column.

Table VIII
Model Parameters

This table contains the parameter values for the structural model.

Parameter	Symbol	Value	Source
Coefficient of relative risk aversion	γ	2	Aaronson, Agarwal, and French (2012).
Subjective discount factor	β	0.93	Aaronson, Agarwal, and French (2012).
Number of time periods	T	40	
Autocorrelation of income	ρ	0.995	Aaronson, Agarwal, and French (2012).
Income risk	σ	23.45%	Aaronson, Agarwal, and French (2012).
Deterministic income growth	$\mathbf{g}$	1.08%	Aaronson, Agarwal, and French (2012).
States of the world	K	5	
Income growth periods	T'	20	Aaronson, Agarwal, and French (2012).
Initial debt	b_0	\$19,200	Average student loan and credit card debt of enrolled students in Table IA.VII.
Interest rate	r	4%	Average undergraduate federal student loan interest rate during sample period.
Initial income	y_0	\$10,243	Average bound employee income in Table II.
Income jump at graduation	Υ	\$10,000	Average income jump upon graduation from the National Longitudinal Survey of Youth 1997.
Per-period graduation probability	q	0.08	Average per-period attrition probability of enrolled students in sample.
Potential graduation periods	τ	6	
Minimum wage increase	Δ	7.81%	Average minimum wage hike during sample period.
Protected assets	$\bar{a}$	\$2,575	Average wildcard exemption in California, South Dakota, Massachusetts, Michigan, Nebraska, and West Virginia.
Credit constraint	χ	34%	Alonso (2018).
Number of simulations	N'	1,000	Number of simulations of the model with and without a minimum wage change to estimate the MPB.

Table IX

Model-Implied MPBs Out of a Minimum Wage Change

This table contains estimates of the three-year marginal propensity to borrow out of a minimum wage change. The columns are defined as follows: *Model* is the estimated model, *MPB* is the estimate of the marginal propensity to borrow, and *Fitted values* are other calibrated parameters from the model.

Model	MPB	Fitted Values
Baseline (no frictions)	0.37	
Credit constraints	0.00	
Tuition costs	0.64	
Defaultable debt	0.22	$r^* = 4.6\%$
Perceived interest rate	−0.38	$r^* = 33.1\%$
Direct utility penalty	−0.38	$\theta = 0.16$

A.3. Baseline Results

We first confirm that our baseline model does not generate a reduction in borrowing. Table IX reports the MPB from our baseline model, calculated from averaging the debt response across 1,000 simulations with and without a minimum wage hike. In sharp contrast to our empirical estimate of −0.38, the MPB in our baseline model is 0.37.[27] That is, while the data indicate that enrolled students borrow less in response to a minimum wage change, our baseline model predicts that enrolled students should do the exact opposite.

Consistent with our cross-sectional tests in Section V, the incorporation of common rational frictions in our model does not allow us to match the empirical MPB. Estimates from models with three such frictions—credit constraints, defaultable debt, and tuition— are shown in Table IX and discussed below.[28]

Credit Constraints: Define the natural borrowing limit $\bar{b}_t$ as the present value of future labor income along the worst possible income path. (For example, the worst path at $t = 0$ is the one with $\omega_t = 1$ and $\pi_t = 0$ for all t.) Following Alonso (2018), we model credit constraints as the minimum of the natural borrowing limit and an exogenous fraction χ of current income. Although introducing credit constraints reduces the MPB (because the high initial debt load causes the credit constraint to bind at the onset), the enrolled student still spends their entire increase in income from the minimum wage.

Defaultable Debt: Let $\bar{a}$ denote a minimum limit on consumption during the last period of the model. We model default as a terminal debt load

[27] Because the debt in our model represents student debt, we use an MPB of −0.38 (= −\$1,025 / \$2,712) as our target instead of −0.32. The horizon of our model-implied MPB is the same as our empirical MPB.

[28] As a form of model validation, we examine whether our model matches the borrowing response of non-students. The model-implied MPB for nonstudents is 0.00, which is similar to the empirical MPB of 0.05 (95% CI: [−0.02, 0.13]). To calibrate the model to nonstudents, we make three changes. First, we define debt as unsecured credit and we set the initial debt to that of nonstudents. Second, we set the interest rate to be 10%. Third, we eliminate the income boost from graduation as nonstudents cannot graduate.

$\tilde{b}_T = \min(b_T, y_T - \bar{a})$ and a risk-adjusted interest rate r^* that allows the lender to break even. Although default lowers the MPB because it raises the risk-adjusted interest rate (lenders must charge more to break even), it does not do so enough to generate a reduction in student loan borrowing.

Tuition: We model tuition as a cost κ that is incurred during each of the first N periods prior to graduation. Relative to our baseline model, introducing tuition leads to a larger (not smaller) borrowing response. This occurs because the enrolled student must borrow more to finance both their education and their optimal consumption.

A.4. Student Debt Aversion

We now introduce student debt aversion into our model (Goldrick-Rab and Kelchen (2015), Caetano, Palacios, and Patrinos (2019)). Student debt aversion captures the notion that student debt is unpleasant and entails additional costs besides just the actual interest rate (Field (2009), Wong (2020)). For instance, student debt can undermine the pleasure received from current and future consumption (e.g., a "pain of paying" as in Prelec and Loewenstein (1998)), and it can also increase the likelihood of experiencing future student debt overhang (Di Maggio, Kalda, and Yao (2020)). In the presence of such additional costs, enrolled students might prefer to use their wage gains to finance their education and avoid additional indebtedness.

We model student debt aversion as a utility cost of debt that is an increasing function of the amount of debt outstanding. This function can be formulated in several ways, including through a high perceived student loan interest rate, a direct utility penalty per dollar of student debt, or a combination of both. Although indistinguishable from one another in the data, the perceived interest rate formulation has the attractive feature of being comparable to the actual interest rate, whereas the direct utility penalty formulation can be linked to other existing studies on debt aversion (e.g., Field (2009)).

To start, we quantify the level of student debt aversion using the perceived interest rate formulation. As shown in Table IX, we find that a perceived interest rate of 33.1% allows us to match our empirical MPB of −0.38. Our structural estimate is comparable to the 30.6% we recovered using our nonstructural approach. It is also well above the actual interest rate on both federal student loans and credit card debt (Agarwal et al. (2018)). In some sense, the latter observation helps rationalize the fact that enrolled students do not reduce their credit card debt following an increase in their wages.

Next, we quantify the level of student debt aversion using the direct utility-penalty formulation. Following Field (2009), we model this penalty as a per dollar cost of holding student debt,

$$u(c_t) = \frac{c_t^{1-\gamma} - 1}{1-\gamma} - \theta \cdot b_t, \tag{18}$$

where θ is the direct utility-penalty parameter. As shown in Table IX, we find that a direct utility penalty of $\theta = 0.16$ per dollar of student debt allows us to match our empirical MPB of -0.38. To interpret the value of this parameter, we can calculate the initial amount of debt that would provide the same expected lifetime utility in the absence of this utility penalty (i.e., when $\theta = 0$). We find that our model with a direct utility penalty of $\theta = 0.16$ per dollar of student debt is equivalent to a model with an initial debt load of $69,000. This represents a 259% markup on the actual initial face value of debt of $19,200.

As stated above, student debt aversion can be formulated in several ways, including through a high perceived student loan interest rate or a direct utility penalty per dollar of student debt. Yet another formulation of student debt aversion is to consider a combination of both. Figure IA.15 plots the pairs of r and θ that generate an MPB of -0.38 in our model. We find that lower perceived interest rates require higher direct utility penalties to match the MPB, and vice versa.[29]

A.5. Model Validation

One benefit of our structural model is that we can test whether its predictions match other features of our data.[30] The first feature that we attempt to match is the flatness of the MPB across student age (see Section IV). In Figure IA.13, we plot our model-implied MPB based on simulations with planning horizons between 20 and 40 periods. (Here, the planning horizon serves as an inverse measure of age.) Consistent with our cross-sectional results, we find that the MPB does not depend much on age and varies within a tight window.

The second feature that we attempt to match is the slight negative relation between the MPB and the initial amount of student debt. In the data, the MPB for enrolled students with above-median initial debt loads is -0.38 ($\Gamma = -\$1{,}041$), whereas it is -0.29 for enrolled students with below-median initial debt loads ($\Gamma = -\$804$). In Figure IA.14, we plot our model-implied MPB based on simulations with different initial student loan balances. Consistent with our cross-sectional results, we find a slight negative relation between the MPB and initial student loan balances that is on par with our empirical estimates.

B. Mental Accounting

What is the source of student debt aversion? Our preferred explanation is mental accounting, which prior theoretical studies argue can generate debt

[29] Is the behavior of former students inconsistent with student debt aversion? We do not believe so. Given their low incomes, former students should be enrolled in IBR plans (Perry, Karamcheva, and Yannelis (2021)) and should expect their loans to be discharged in the future (Mueller and Yannelis (2022)). Hence, former students should perceive a much smaller student debt obligation than their book value of debt. Even if former students exhibit student debt aversion, their low perceived student debt obligations would encourage them not to reduce their student debt, as both r and θ would be interacted with a small perceived level of debt.

[30] For these tests, we focus on the direct utility-penalty formulation of student debt aversion.

aversion (Thaler (1990)).[31] Indeed, one key piece of evidence that suggests that mental accounting is the underlying determinant of student debt aversion is that our results are driven by enrolled students borrowing fewer loans as opposed to taking out smaller loan amounts.

In addition to student debt aversion arising from mental accounting, our results are consistent with a broader mental accounting explanation (Thaler (1985)). Specifically, to organize their financial activities, enrolled students might group their funds into separate, nonfungible mental accounts (Shefrin and Thaler (1988)). If enrolled students consider a certain fraction of their labor income as dedicated toward their education, then an increase in their income could result in a reduction in student loan borrowing. Furthermore, this reduction in student loan borrowing would be confined to enrolled (and not former) students, and the magnitude of the reduction would be independent of other factors such as credit constraints, default costs, and the amount of credit card debt.

To illustrate the above explanation, suppose an enrolled student dedicates a fraction x of their income ω toward their education. Suppose tuition and living expenses are equal to e, and that an increase in the minimum wage raises income from ω to $\omega + \Delta$. Assuming $e > x \cdot (\omega + \Delta)$, the enrolled student borrows $d = e - x \cdot \omega$ prior to the minimum wage change, and $d' = e - x \cdot (\omega + \Delta)$ after. Hence, the change in borrowing is $d' - d = -x \cdot \Delta < 0$ and the MPB is equal to $-x < 0$.

Labeling behavior, such as that described above, is one of the main departures of mental accounting from the traditional consumption-savings framework. Another relevant departure is the idea that individuals adopt rules of thumb to constrain their own behavior (Shefrin and Thaler (1988)). For instance, a mental accounting rule of "take on as little student debt as possible" could generate both a reduction in student loan borrowing and heterogeneous responses across enrolled and former students. Indeed, Thaler (1990) argues that such rules of thumb can act as self-imposed credit constraints that are akin to debt aversion.

VII. Conclusion

In this paper, we use individual-level income and credit data to estimate the effects of wage gains on the debt of low-wage workers. We find the effects are nuanced. In the three years following a \$0.88 increase in the minimum wage, the average low-wage worker experiences a \$2,712 increase in income and a \$856 decrease in debt. The entire decline in debt comes from a reduction in student loan borrowing among enrolled college students. Nonstudents and former students, in contrast, borrow more in response to an increase in their wages. Credit constraints and several other rational channels cannot explain the reduction in student loan borrowing. Even more puzzling, enrolled

[31] Prelec and Loewenstein (1998) show that debt aversion arises in a "double-entry" model of mental accounting.

students choose to accumulate less student debt instead of reducing their higher interest credit card balances. Two behavioral explanations—student debt aversion and mental accounting—best explain our findings.

Our results should be interpreted with several caveats in mind. First, we focus on the effect of the minimum wage on incumbent low-wage workers. This effect could be different than the aggregate effect if firms reduce hiring in response to higher mandated wages. Second, we focus on enrolled students inframarginal to the college attendance decision. An increase in wages could also affect the initial decision to attend college as well as the decision to finance education with debt (Neumark and Wascher (1995)). Third, our sample consists of enrolled and former students who work in low-wage jobs and hence might not be representative of the overall population of enrolled and former students. Therefore, broad generalizations about student debt aversion or other behavioral motives might not be warranted from our paper. Fourth, although our sample contains historically large minimum wage changes, these changes are still much smaller than those recently proposed, such as $15 per hour. Finally, our estimates alone cannot be used to understand the total welfare effects of the minimum wage. For a comprehensive analysis of welfare, see MaCurdy (2015).

Our paper has a number of important implications. First, our paper is one of the first to highlight a negative relation between minimum wage policies and student loan balances. Therefore, an important consequence of increasing the federal minimum wage could be a reduction in the aggregate growth rate of student loan balances.[32] Second, we are among the first to provide large-scale evidence consistent with student debt aversion. Exploring a potential rational basis for student debt aversion could be a fruitful area for future research. Third, because of the granular nature of our data, our paper is one of the first to document heterogeneous debt responses to wage gains across different groups of low-wage workers. Knowing such responses is useful for developing and testing consumption-savings models (Japelli and Pistaferri (2010)), and for understanding low-wage workers' preferences, borrowing motives, and financial constraints (Zinman (2015)).

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[32] We note that the potential effect of raising the minimum wage on the aggregate amount of student loan debt is limited during our sample period (the estimated percent of individuals with positive student loan balances that are both enrolled in college and are bound employees = 1.6%, and the estimated lower bound on the elasticity of aggregate student loan balances to the minimum wage = −0.005). However, larger potential future minimum wage changes (e.g., to $15 per hour) could have much larger impacts. This is because the change in student loan balances in response to an increase in the minimum wage depends on the number of workers who receive a wage increase, and this depends in turn on the location of the new minimum wage along the wage distribution. Larger minimum wage changes would affect a larger portion of the wage distribution, and hence could lead to larger effects on student loan balances. Furthermore, larger minimum wage changes could encourage former students to repay their loans or force those students in IBR plans to do so.

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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's website:

Appendix S1: Internet Appendix.
Replication Code.