

A Letter and Encouragement: Does Information Increase Postsecondary Enrollment of UI Recipients?[†]

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For individuals who experience job loss, enrollment in postsecondary programs may provide an opportunity to improve future employment outcomes. However, decisions to enroll may be hampered by insufficient information about the benefits and costs and the necessary steps and assistance available to facilitate such investments. Using variation in the dissemination and timing of letters sent to Unemployment Insurance (UI) recipients containing this information, we find that individuals sent the information are 40 percent more likely to enroll. These findings suggest that well coordinated information interventions delivered with institutional support may be more effective than raising the generosity of existing government programs in increasing participation. (JEL E24, E32, I23, I26, J24, J31, J65)

The sharp erosion of economic conditions associated with the Great Recession led to a dramatic spike in job loss in the United States, with the unemployment rate rising from 6.5 percent in October 2008 to 10 percent in October 2009. Of the millions of individuals who lost their jobs, those with low earnings potential and limited education were hit hardest by the recession. As a result of their job loss, these individuals will likely face large and long-lasting reductions in income as well as increases in the likelihood of sickness and disability (Jacobson, LaLonde, and Sullivan 1993; Couch and Placzek 2010). However, formal training may attenuate these effects. Indeed, Jacobson, LaLonde, and Sullivan (2005a) finds that completing only a few courses at a community college results in higher earnings for displaced workers; furthermore, the estimated returns to college remain high for young individuals.¹ Despite potential earnings gains, rates of postsecondary enrollment remain low for Unemployment Insurance (UI) recipients. This may be efficient and represent

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¹“Displaced workers” are generally distinguished from all unemployed workers in the dimensions that they have not been discharged for cause and they are permanently separated from their employer, with a long-standing attachment to the former employer or industry (Jacobson, LaLonde, and Sullivan 2005a). In contrast, our study focuses primarily on UI recipients. Jacobson, LaLonde, and Sullivan (2005a, 2005b) presents two of the only studies on the return to postsecondary enrollment among displaced workers. They find that an additional year of community

optimal individual responses if present costs outweigh future returns from enrollment for this particular population. Alternatively, constraints such as access to credit to finance enrollment or family obligations may limit postsecondary participation. Yet, the availability of substantial federal financial aid in the form of grants and loans would suggest that credit constraints do not form a complete explanation for limited enrollment among UI recipients. Evidence from a series of recent behavioral interventions suggests an alternative explanation: perhaps UI recipients are unaware of the costs and benefits of postsecondary attainment and the necessary steps and assistance available to facilitate such investments.

Workers who are unemployed, particularly those who lose their jobs, may face substantial barriers in pursuing postsecondary enrollment. One concern is that the decision to pursue college enrollment, the aid application process, and the choice of a college program well matched to prior achievement and aspirations, may represent difficult choices for imperfectly rational individuals. This is particularly true for those choosing among the wide array of postsecondary programs, which can be complicated to access and navigate, as well as the application process for federal financial aid (Dynarski and Scott-Clayton 2008). These barriers may be especially salient for the most vulnerable among the unemployed, resulting in an exacerbation of inequality in long-term outcomes (Bertrand, Mullainathan, and Shafir 2004). We address these questions using state variation in the dissemination and timing of information sent to UI recipients informing them of the expected benefits of postsecondary enrollment, potential eligibility for financial aid, and the steps necessary to navigate the application and enrollment process. As part of the informational outreach, the intervention also engaged state employment services offices and local financial aid administrators, priming them to assist UI recipients interested in pursuing enrollment.

Our primary innovation is to study the impact of an informational outreach on the postsecondary enrollment of unemployed individuals. This is important because these individuals generally start from the bottom quartiles of the income distribution and may have few prospects for upward mobility without access to postsecondary education. Moreover, workers with limited education and several years of full-time labor force participation may lack the resources, such as guidance counselors and a large group of peers making similar decisions, available to those on a more traditional college trajectory. In contrast to our study, most of the existing literature has focused on high school seniors, with mixed evidence on the effectiveness of information-based interventions.

In May 2009, at the height of the recession, the Department of Labor and the Department of Education issued guidelines intended to encourage postsecondary enrollment among UI recipients and to inform these potential students of their likely eligibility for financial aid in the form of Pell Grants. The ad hoc state decisions to send UI recipients a letter of notification about the Pell Grant and other training opportunities (“Pell Letter”) following the May 2009 guidance provides a natural

college education generates increases in long-term earnings of 7 to 10 percent among displaced workers. In contrast, the evidence on the effects of informal short-term training provided through the Workforce Investment Act is less definitive (for example, see Andersson et al. 2013 and Heinrich et al. 2009).

experiment through which we can address open questions about the effect of information interventions at scale (more than 20 million individuals were sent letters). The information sent to UI recipients facilitated engagement with a number of intermediaries, including employment services offices and local financial aid administrators, who were prepared to assist individuals responding to the letter. At question is whether this very general informational intervention affected the enrollment behavior of those experiencing unemployment.

We find that the “Pell Letter” increased the likelihood of enrollment substantially; UI recipients aged 20 to 40 who received the letter were 4 percentage points more likely to enroll during the following 6 months. These estimates are robust to multiple specifications and treatment definitions; furthermore, the results are similar across three datasets: the Survey of Income and Program Participation (SIPP), administrative data on the Free Application for Federal Student Aid (FAFSA) filings, and data from the Benefit Accuracy Measurement (BAM) survey. We address endogeneity concerns related to the sending of the letters using two placebo exercises, which reveal no effect on enrollment or FAFSA filing for two groups of potential students during the months the letter was sent. A back of the envelope calculation suggests that the intervention resulted in an increase in enrollment of at least half a million UI recipients.

The effects are somewhat larger for women than for men. The effects were most pronounced for UI recipients who were black or had lower earnings potential at baseline, suggesting that differences in the accessibility of information about program benefits and how to access them may contribute to inequality. Individuals exposed to the letter were also less likely to be employed and more likely to draw UI over the following year. We observe moderate effects on educational attainment, but the evidence on subsequent labor market outcomes is inconclusive given the limited window of observation available in the data. The broad takeaway is that low cost informational interventions may have large impacts on behavior, particularly for the most disadvantaged; in our case, the effect of informational outreach is larger than that of increasing annual aid by \$1,000 per individual (Dynarski and Scott-Clayton 2013).

Our findings are consistent with prior research that suggests information may be particularly effective when it facilitates access to assistance or simplifies the process of uptake. While some evidence suggests that information alone may be ineffective (Bergman et al. 2017, Bettinger et al. 2012), a number of studies in the higher education context find substantial effects of information provision, particularly when it is paired with access to assistance (Hoxby and Turner 2013; Castleman and Page 2015; Barr, Bird, and Castleman 2017; and Carrell and Sacerdote 2013).² Most existing evidence related to how information-based interventions impact college

²Hoxby and Turner (2013) finds that customized information about college application and net price has a substantial effect on where high-achieving, low-income students choose to apply and enroll, with effects on application to a peer institution of 12.2 percentage points (22.3 percent) and attendance at a peer institution of 5.3 percentage points (18.5 percent). Castleman and Page (2015) found that text messages and counselor-based support increased transitions to college in two of three experimental sites with effects in the range of 5–7 percentage points. Barr, Bird, and Castleman (2017) finds effects of a similar texting intervention on student loan borrowing levels of 5 percent, growing to 14 percent when focusing on the students who sent the messages during a relevant time period (before the priority aid deadline). Carrell and Sacerdote (2013) finds that information is only effective when combined with

enrollment focuses on first-time students making the transition from high school to college.³ While focused on the distinct group of individuals sent the letters, the “Pell Letter” initiative similarly conveyed information alongside connections to counselors who were primed to assist with the navigation of the enrollment process. That this “connection” occurred is reflected in the volume of calls received by employment services offices in response to the letter (National Association of State Workforce Agencies (NASWA) 2010). The formal endorsement of the letters by the White House and state and local agencies as well as the relatively low level of existing training information and encouragement provided to most UI recipients may have magnified the effect of the letters.

Our analysis begins with a detailed exposition of the “Pell Letter” policy, explaining the implementation and the potential mechanism of effect along with the observed variation across states. In Section II, we provide an overview of the data and the descriptive results in our analysis. In Section III, we cover the estimation approach, which relies on variation in “Pell Letter” implementation within states over time to identify the effects of the intervention on enrollment. Section IV concludes.

I. “Pell Letter” Guidance and Implementation

A. Enrollment of UI Recipients and the “Pell Letter” Initiative

At the height of the fiscal crisis, the Executive Branch, the Department of Labor, and the Department of Education worked together to encourage UI recipients to invest in training. With the May 2009 announcement that nearly 540,000 individuals lost jobs in the prior month, President Obama announced a proactive effort to encourage postsecondary investment and to improve the alignment of the Department of Labor and Department of Education policies affecting the unemployed. In a speech on May 8, 2009, President Obama noted:

In a 21st century economy where the most valuable skill you can sell is your knowledge, education is the single best bet we can make not just for our individual success, but for the success of the nation as a whole ... So if we want to help people not only get back on their feet today but prosper tomorrow, we need to take a rigorous new approach to higher education and technical training.

The policy initiatives were multipronged and involved coordinated action from the Department of Labor and the Department of Education. First, in a guidance letter, Secretary of Labor Hilda Solis sent a letter to all states encouraging them to inform all UI recipients of the benefits of postsecondary enrollment and opportunities for federal financial aid and related postsecondary education options. Second, Department of Education Secretary Arne Duncan sent a letter to all

assistance or mentoring, with information combined with mentoring producing an impact of about 6 percentage points (14 percent) on enrollment.

³Bettinger et al. (2012) is an exception in that they measure the treatment effect of an intervention providing financial aid application guidance to adult students and find a college enrollment effect of about 13 percent.

postsecondary financial aid officers encouraging them to facilitate the determination of aid eligibility for UI recipients. At the federal level, these efforts were promoted through the new website, Opportunity.gov. In the words of higher education policy expert Anthony Carnevale (2009), “[w]ith one quick opening step Obama crossed the Mall between the DOE and the DOL.”

The federal guidance was in no way binding and states were given latitude to decide whether to send a letter to UI recipients, when to send the letter, and to whom to send the letter. Roughly 40 states had sent or were in the process of sending these letters by the end of 2009 (NASWA 2010). The decision in each state resided with the state employment office. Broadly, the letters sent by the states included three general messages: they encouraged UI recipients to seek training; they informed UI recipients about the Pell Grant program and educational opportunities; and they suggested that UI recipients would be given special consideration for Pell Grant receipt.

Beyond information distributed by state employment agencies, the federal Department of Education also provided guidance to colleges and universities, which was intended to make it easier for institutions to “use ‘professional judgment,’ to adjust aid awards for those workers who had experienced job losses.” The guidance indicated that:

[D]uring this period of economic hardship, you may use the letter from the state unemployment agency, or other evidence that a student is receiving unemployment benefits, to document that the income earned from work of that student is zero for the purposes of adjusting data items for the student on the student’s federal financial aid application. For purposes of implementing this letter only, unemployment benefits can also be considered ... Unemployed individuals will be able to present letters for 90 days from the date of issuance of those letters to an aid administrator for consideration under this guidance. (The letter should not be accepted if you know that an applicant already has obtained other employment.) Other verification of current receipt of unemployment benefits is an acceptable substitute for the state unemployment agency letter.⁴

In effect, those able to present information on eligibility for UI were to be treated as having zero income for calculating eligibility for federal financial aid. While many aid administrators were already adjusting income for UI recipients, the letter sent by state employment offices to UI recipients served as an effective ticket for determination of financial aid eligibility. A second feature of this guidance letter to financial aid administrators was assurance that the use of professional judgment in the case of UI recipients would not affect the likelihood that the college or university was selected for program review by the Department of Education.

The Department of Labor issued an update to the original guidance on July 8, 2009, in which it recommended clarifying the correspondence with UI recipients to make clear the need for beneficiaries to check to make sure any enrollment satisfied the states “approved training” criteria.⁵ In addition, this guidance acknowledged

⁴The full guidance letter is available at <http://www2.ed.gov/policy/gen/guid/secletter/090512.html>.

⁵See http://workforcesecurity.doleta.gov/dmstree/tegl/tegl2k8/tegl_21_08ch1.pdf.

a strong initial impact among states that had sent letters within weeks of the initial mailing, noting that “[s]everal states that have already sent their initial mailings have experienced a 2 to 3 percent response rate.”

B. Information on State-Specific Initiatives

We systematically (and repeatedly) contacted each state to inquire about whether the state workforce agency sent a letter and, if so, the timing and incidence of the letter. Our initial requests were in the form of a survey sent via email (with follow-up). For nonrespondent states, we also sent a paper document by mail and followed up with several phone calls. For persistent nonresponders, we sent Freedom of Information Act or public records requests as permitted by state rules requesting a copy of the state’s letter (if one was sent) and other descriptive information on the initiative (see online Appendix A1 for additional information). We received responses from 46 states and the District of Columbia; we used a preexisting survey to determine that three states did not send letters; and we have been unable to obtain information from Tennessee.

Figure 1 provides an overview of state participation in the initiative, with the period from May 2009 to March 2010 displayed across the columns.⁶ Shaded cells in each state and month indicate that the state sent letters to UI recipients in that month. Overall, at least 40 states and the District of Columbia sent letters and, among those sending, there is wide variation in sending patterns. For example, California and Oklahoma sent the letters for only a short period, whereas others such as Missouri and Wyoming sent the letters repeatedly. There is also variation in the timing of initial letter sending; although three states managed to send letters promptly in June of 2009, the largest addition of states sending letters occurred between July and August 2009, and some states did not send the letter until 2010. Anecdotal evidence from an external survey suggests that variation in the timing of sending was largely idiosyncratic, but we explore the potential endogeneity issues further in Section IIIC. Finally, there is variation in the type of individual who was sent the letter. In some states and months (shaded black), the letter was sent to all UI recipients, while in others (with diagonal shading), the letter was sent only to new UI recipients. In two states (shaded with cross-hatch), letters were sent only to individuals who began drawing UI after the announcement of the intervention in May 2009. In these states, individuals who began receiving UI prior to May 2009 were not exposed to the letters regardless of when the state workforce agency decided to send them.

C. Theoretical Channels and Mechanism of Impact

UI recipients will enroll if the expected value of future benefits exceeds the costs. The intervention may have affected either the real or perceived value of future benefits or costs. First, the intervention may have affected the real monetary cost of pursuing enrollment by increasing the financial aid available to UI recipients.

⁶Maine stopped sending the letters in December 2010 and Wisconsin and Wyoming stopped sending in January 2011. All other states sending the letters in March 2010 continued sending letters throughout the sample period.



FIGURE 1. STATE “PELL LETTER” SENDING PATTERNS

Notes: Data are from our survey of state workforce agencies (see online Appendix A for additional information). The figure presents information from the 40 states and the District of Columbia that sent a Pell Letter and responded to our survey. Maine stopped sending the letters in December 2010 and Wisconsin and Wyoming stopped sending letters in January 2011. All other states sending the letters in March 2010 continued sending letters throughout the sample period. We confirmed that eight states (Arizona, Alabama, Kansas, Nebraska, Nevada, New Hampshire, New Mexico, and North Dakota) did not send a letter. Indiana’s survey response was inconclusive and Tennessee failed to respond to our requests. In some states and months (shaded black), the letter was sent to all UI recipients, while in others (with diagonal shading), the letter was sent only to new UI recipients. In two states (shaded with cross-hatch), letters were sent only to individuals who began drawing UI after the announcement of the intervention in May 2009. In these states, individuals who began receiving UI prior to May 2009 were not exposed to the letters regardless of when the state workforce agency decided to send them.

Second, the intervention may have affected the perceived value of future benefits or costs. Recent evidence from behavioral economics suggests that the provision of simplified information may change perceived benefits or costs when information frictions or deficits are present. Changes to perceived benefits or costs may have resulted if the letters provided new information to UI recipients regarding the availability of financial aid, the expected return to training, or the rules governing

receipt of UI benefits while enrolled. Finally, a more subtle reduction in real or perceived costs may have been brought about if the intervention reduced the complexity or hassle of taking proactive steps toward enrollment.

Following the above framework, there are several potential mechanisms through which the intervention might affect behavior. UI recipients may be more likely to enroll due to real increases in the financial aid available to them. While evidence on the effect of Pell eligibility on enrollment is relatively weak, substantial evidence indicates that increases in financial aid boost enrollment (Dynarski and Scott-Clayton 2013). Alternatively, the intervention may address information deficits related to the costs and benefits of enrolling if it changed individuals' perceptions about the aid available, the expected return to training, or the possibility of continuing to receive UI benefits while enrolled. Finally, the intervention may reduce the complexity or hassle costs associated with navigating the enrollment application and financial aid process by providing UI recipients with access to resources to help recipients enroll and apply for financial aid. Whereas Carrell and Sacerdote (2013) and Bettinger et al. (2012) find no effect of information alone, both studies find that providing access to assistance can increase enrollment substantially. We use surveys of workforce agencies and financial aid administrators, combined with empirical evidence, to inform our understanding of how the intervention likely affected behavior.

Increases in Financial Aid.—As a component of the intervention, the Department of Education informed financial aid administrators of the distribution of letters and encouraged them to make adjustments to financial aid eligibility for UI recipients. If this guidance resulted in meaningful changes in aid eligibility, UI recipients may have been induced to enroll by increases in the financial aid available to them. While evidence on the effect of Pell eligibility on enrollment is relatively weak, substantial evidence indicates that increases in financial aid boost enrollment (Dynarski and Scott-Clayton 2013).

In practice, the anecdotal evidence suggests that there was little change in the financial aid treatment of UI recipients as a result of the intervention, with a federal report about the intervention indicating that “[o]verall, during our site visits, states reported little change in policies or use of Pell grants” *among those who participated in training* (Barnow et al. 2012).⁷ Discussions with federal aid administrators also suggest that they were already adjusting UI recipient income on the FAFSA prior to the intervention. This is consistent with the high level of Pell eligibility (roughly 75 percent) for FAFSAs filed by dislocated workers prior to the intervention. As we show more formally below, there is also no evidence in the data that FAFSAs filed by UI recipients were more likely to be deemed Pell eligible after the intervention.

⁷ See https://wdr.doleta.gov/research/FullText_Documents/ETAOP_2011-06.pdf. As we note below, while the use of Pell Grants conditional on enrollment does not appear to have changed, the number of individuals receiving Pell Grants did increase as more UI recipients enrolled.

Information.—Mounting evidence suggests that low-cost information interventions can have large effects on behavior. Yet, there is still much uncertainty as to whether, how, and for whom information is effective in affecting college enrollment. In our setting, we focus on three specific information deficits that the intervention may have addressed: perceptions about the availability of financial aid, expectations about the return to training, and awareness of the possibility of continuing to receive UI benefits while enrolled.

While the intervention does not appear to have changed actual financial aid eligibility, it does appear to have increased awareness of the Pell Grant program. One state employment commission noted that “[t]he initiative was successful in raising the profile of the Pell Grant program and encouraging UI recipients to consider this option” (NASWA 2010). Furthermore, the direct statement of the amount of available aid—“Federal Pell Grant scholarships cover up to \$5,350 in education and training expenses”—may have made the information about likely eligibility particularly salient to UI recipients considering training. This is consistent with evidence from Bhargava and Manoli (2015) in which explicit statements of expected EITC benefit amounts increased program take-up.

On the benefit side, the intervention heightened the salience of potential returns to college. Many of the states’ letters included language from the federal government’s suggested letter about potential returns such as “[s]tudies have shown that workers with more education and training have more secure jobs and higher wages.”

Finally, the letter made clear that postsecondary education programs were, indeed, allowable options with UI receipt. Because UI receipt is accompanied by the distribution of rules about work activities prohibited with benefit receipt, and job search activities required for benefit receipt, it is plausible that many UI recipients would have assumed (incorrectly) that education and training were prohibited activities without the statements in the letter. By clarifying that they would not lose their UI benefits, the letter may have influenced the perceived costs of enrollment.

Access to Informed Assistance (Mediating Factors).—Beyond the transmittal of information, the letters may have reduced the complexity and “hassle” of taking proactive steps toward enrollment as the letters provided contact information for resources to assist in identifying a program and applying for aid. These resources included both phone numbers and websites such as www.opportunity.gov and www.fafsa.gov, as well as state-specific resources.

A distinction in the implementation of the mailing of the letters to UI recipients from many other informational interventions is that the initiative engaged a number of other intermediaries including counselors at the employment services “One-Stop” offices and financial aid administrators. In this sense, the mechanism of effect went beyond the “consumer side” of affecting the behavior of UI recipients by also activating a number of mediators who were positioned to magnify any potential impact of the basic letter. At the One-Stop employment services centers, counselors were “primed” to provide information in response to the letters. In addition, many of these centers introduced supplemental sessions such as seminars on how to complete the FAFSA.

A survey of states conducted by NASWA (2010) demonstrates that increased information and access to informed assistance were both at play:

- *Florida*—“Many were calling with the following questions: What training programs are approved and available? How do I apply for training? How much money will we pay for school?”
- *District of Columbia*—“Staff received training on the Pell Grant application process, and a Pell Grant Call Center was established. The Career Centers arranged additional workshops to meet the demand.”
- *NASWA Summary of State Actions*—“The types of actions states took to implement the initiative included: partnering with higher education to provide workshops; bringing in community college personnel to give staff and customers a better understanding of the Pell Grant process; hosting a special phone line to answer general questions regarding school attendance and UI; hosting a designated training session for local UI staff; contracting with a nonprofit to provide workshops and Pell Grants and financial aid through the Career One-Stops; and phasing the mailing of letters.”

While there is little evidence that financial aid eligibility changed for UI recipients, it is clear that the intervention altered the information provided. Despite the absence of a formal experimental design, the “Pell Letter” was designed and distributed in a way that addressed many of the information deficits and behavioral tendencies that have been the focus of other successful information interventions. While our research design does not allow for formal tests of the relative importance of the informational channels of impact identified above, we hypothesize that the transmission of information about training eligibility and the availability of financial aid in a form that was salient and authoritative, combined with the affirmative mediating role of state employment services offices (including the “One Stop” centers) and local financial aid administrators, increased the propensity of UI recipients receiving the letters to enroll in postsecondary programs.

II. Data and Descriptive Results

Our primary source of data on individual behavior is the 2008 Survey of Income and Program Participation (SIPP). The SIPP is a longitudinal survey conducted at four-month intervals, which contains month-by-month histories of school enrollment, employment, and earnings. Unlike many other data sources, the SIPP has two features that make it particularly well suited for our study: detailed information on the timing of UI receipt, and information on school enrollment.

We restrict the 2008 SIPP panel to individuals aged 20 to 40 who are initially observed receiving UI between October 2008 and November 2010. Beginning in October 2008 allows us to have equivalent information on all individuals prior to initial UI receipt.⁸ We further eliminate any individuals enrolled in the month prior

⁸We chose November 2010 as the end date because it provides a balance between sample size and our ability to observe individuals many months after initial UI receipt. Results are robust to shifting the end date in

TABLE 1—DESCRIPTIVE STATISTICS FOR INITIAL UI RECIPIENTS

| Variables | SIPP | BAM |
|--|--------|-------|
| <i>Panel A. Characteristics at initial UI receipt (baseline)</i> | | |
| Age | 31.00 | 30.24 |
| White | 0.785 | 0.693 |
| Black | 0.135 | 0.189 |
| Male | 0.584 | 0.608 |
| HS degree + | 0.900 | 0.865 |
| Some college + | 0.588 | 0.446 |
| BA + | 0.192 | 0.135 |
| Baseline earnings (annualized) | 24,250 | NA |
| Hourly wage | NA | 16.22 |
| <i>Panel B. Education outcomes</i> | | |
| Enrolled next 6 months | 0.097 | NA |
| Part-time next 6 months | 0.044 | NA |
| Full-time next 6 months | 0.065 | NA |
| Months enrolled 24 months out | 1.39 | NA |
| Months enrolled 36 months out | 1.91 | NA |
| In training (snapshot) | NA | 0.075 |
| Observations | 1,472 | 5,154 |

Notes: Samples include individuals aged 20–40. SIPP sample restricted to individuals who first received unemployment compensation between November 2008 and November 2010 and were not enrolled in the month prior to initial UI receipt. Variables of type “__Next X Months” (e.g., Pell Next 6 Months) are binary variables indicating whether the condition was true during any of the six months following first UI receipt. Baseline earnings is an annual measure generated using the three months prior to initial UI receipt. BAM sample restricted to individuals who first received unemployment compensation between November 2008 and November 2010 and were observed within 75 days of initial UI receipt. The BAM training measure is the fraction of UI recipients in training at the time of their participation in the Benefit Accuracy Measurement (BAM) survey. See notes to Table 2 for additional sample restrictions.

to initial UI receipt. We collapse the data to the individual level so that it is a repeated cross section; thus, for an individual in the SIPP there is *at most one observation in our sample corresponding to the year and month of initial UI receipt*.

Table 1 presents descriptive characteristics for our primary sample. As with the population of UI recipients more generally, individuals in the sample are more likely to be male and less educated than the general population; whereas more than a quarter of individuals in this age group have a bachelor’s degree, only 19 percent of UI recipients do. In contrast, approximately 90 percent of UI recipients have a high school degree.⁹ Looking at individuals in our sample, about 10 percent of UI recipients enroll in school within 6 months of initial UI receipt, while 13.5 percent enroll within 12 months.

In our sample, about 40 percent of individuals were treated (the “Pell Letter”), within 6 months of initial UI receipt. Figure 2 illustrates the essence of the intervention:

either direction (Table A1).

⁹With some exceptions for “ability to benefit,” Pell eligibility is limited to those with at least a high school degree who have not received a bachelor’s degree.

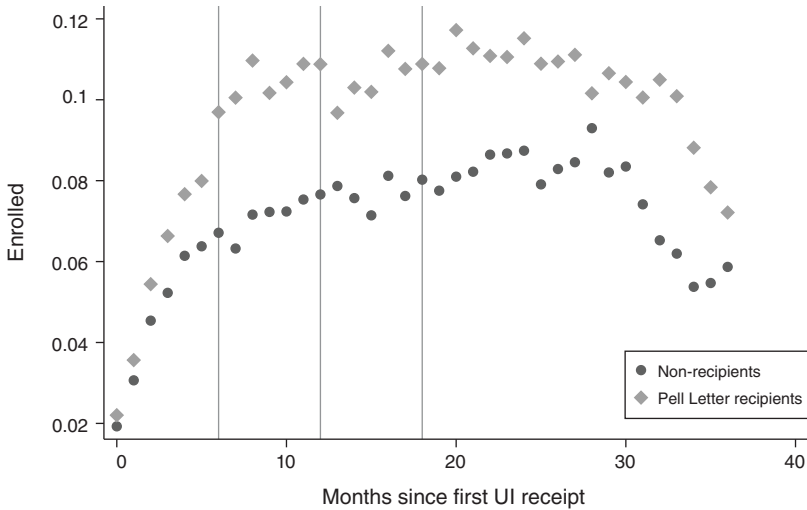


FIGURE 2. ENROLLMENT BY MONTHS SINCE INITIAL UI RECEIPT

Notes: The figure plots the unweighted average enrollment rates of individuals against the number of months since first UI receipt using the 2008 SIPP. Sample restrictions are detailed in the text. The vertical lines indicate 6, 12, and 18 months after initial UI receipt.

the enrollment rates of UI recipients are arrayed on the horizontal axis in terms of months since initial UI receipt, and the enrollment rates are shown on the vertical axis, with the enrollment rates of those who received the letter (light symbols) distinguished from those who did not receive the letter (dark symbols). For both the population that received the letter and the population that did not, enrollment rises steadily over the first six months of a spell before leveling off. The plot makes clear the sharp divergence between the two groups with the letter recipients enrolling at a rate about 4–5 percentage points greater than those who did not receive the letter.

We corroborate our findings with data from the Benefit Accuracy Measurement (BAM) program data. The BAM is a random audit of UI claimants and is designed to determine the accuracy of claims. BAM data are randomly selected from the 50 states, the District of Columbia, and Puerto Rico. Since 1997, allocated sample sizes range from 360 cases per year in the 10 states with the smallest UI workloads to a minimum of 480 cases in the remainder of the states. Each record is a snapshot of an individual during the current week of UI receipt as well as information on the date of their initial UI benefit claim and payment. Importantly, the BAM survey asks participants whether they are currently in academic, vocational, or Workforce Investment Act (WIA) training. Roughly 7.5 percent of individuals are in training during the week they are surveyed. While broadly comparable with the SIPP, UI recipients in the BAM data have lower levels of education and are more likely to be black. The larger sample sizes and greater detail of the BAM data allow us to answer additional questions about program type and mechanism as well as explore whether the information is more or less effective for different types of individuals.

Finally, we use administrative data on FAFSA filings obtained from the Department of Education for the 2009 filing cycle. The data provide counts of

FAFSA filers by state, year, month, and dislocated worker status. They are broken out separately by Pell eligibility, program type (bachelor's and academic associate's or technical associate's and certificates), and age (whether or not the individual was over 30). We use these aggregate FAFSA data to conduct complementary analyses that substantiate our main findings as well as allow us to answer related questions about the types of programs marginal enrollees likely chose. These data also allow for the examination of whether Pell eligibility for dislocated workers actually appeared to change following the May 2009 announcement.

Even as none of the datasets available provides the "ideal" measurement opportunity, a strength of our analysis is that we are able to bring three independent resources to the question. The combination of the survey data from the SIPP, administrative data in the form of FAFSA filings from the Department of Education, and combined survey and administrative data in the BAM from the Department of Labor lends credibility to our results.

III. Estimation and Empirical Results

To estimate the effect of the "Pell Letter" on postsecondary outcomes, we use variation between states over time, variation within states in the timing of the contact with UI recipients, and variation in states' decisions to contact all UI recipients or only new recipients. Using the SIPP data, our primary outcome of interest is enrollment. We also explore longer term effects of the intervention on collegiate attainment, employment, and earnings. Each individual in the SIPP contributes at most one observation to the sample and is distinguished by the state and timing of initial UI receipt. Our basic specification is

$$(1) \quad E_{ist} = X_i\beta_1 + \alpha_s + \lambda_t + \gamma Letter_{ist} + \epsilon_{ist},$$

where E_{ist} is a binary variable indicating whether an individual is enrolled at any point during the six months following initial UI receipt; X_i are indicator variables for age, race, and gender characteristics; and α_s and λ_t are state and year-month fixed effects for the state and month of initial UI receipt. Our primary measure of whether an individual was sent a "Pell Letter," $Letter_{ist}$, is whether a letter was sent to UI recipients with the same timing and state of initial UI receipt in the six months subsequent to initial UI receipt.¹⁰ When this variable is equal to one, we refer to individuals as being "treated," with the associated coefficient gamma representing the treatment effect.¹¹ The variation in letter exposure is driven by differences in

¹⁰We chose six months as it corresponds to the standard duration of UI benefits; however, the results are robust to using other intervals (Table A3).

¹¹This specification assumes that there is no interaction effect between treatment and the calendar month of initial UI receipt, implicitly assuming that academic schedules have little effect on the enrollment decisions of UI recipients. We believe this is reasonable because most UI recipients enroll in community colleges (or for-profits), which provide much greater flexibility than traditional colleges (i.e., shorter terms and more start dates). Furthermore, and perhaps as a result of this flexibility, the differences in the likelihood of enrollment across calendar months of initial UI receipt are generally small and statistically insignificant, suggesting that the calendar timing of initial UI receipt is unlikely to substantially magnify or attenuate the treatment effect.

whether a state decided to send a letter, in which months a state decided to send a letter, and whether or not a state decided to place restrictions on sending based on the dates of initial UI receipt. Recall that Figure 1 provides an illustration of this variation.

A. Baseline Enrollment Estimates

Our baseline results in Table 2 indicate that treatment increased enrollment by 4 to 5 percentage points, a large increase off a base of just less than 10 percent. We note, however, that the 95 percent confidence intervals are large and include effects as small as 2 percentage points. The estimates are robust to the inclusion of local labor market controls (state unemployment rate¹²) in column 2, as well as controls for baseline education levels in column 3; these controls include indicator variables for the attainment values contained in the SIPP. In column 4, we restrict the sample to individuals with at least a high school degree but less than a bachelor's degree—education levels eligible to receive a Pell Grant. The point estimates are similar throughout and are also robust to different measures of treatment, sample restrictions, and specification choices.¹³ The statistical significance levels of the estimates are also robust to a procedure (randomization inference) in which we randomly reassigned the timing of letter sending to states and reestimated the specification 1,000 times. This process produced a similar, but slightly larger, p -value (0.053).¹⁴

The second and third row of estimates in Table 2 indicate that the enrollment increases generated by the intervention appear to be largely full-time enrollments. Whereas the point estimates are negative or zero for part-time enrollment, treatment increases the likelihood of full-time enrollment by 5 percentage points. This suggests that, in addition to shifting individuals into college, the nudge may have led some UI recipients to dedicate more time to training.

In panel B of Table 2, we present analyses using data from Benefit Accuracy Measurement (BAM) program data, which indicate whether a UI recipient is enrolled in training at the point of observation. The major limitation of the BAM is that the BAM paid claims sampling frames *consist of payments* and therefore *claimants have an increased chance of being in the BAM samples the longer they are paid benefits*. This is an issue for our purposes if letter receipt has an effect on whether an individual continues UI receipt (and thus is more likely to be in the sample).

¹²Although the specifications in the paper contain only the state unemployment rate, the estimates are similarly robust to the inclusion of other measures of labor market conditions including the insured unemployment rate or new jobless claims (Table A2).

¹³We have explored the robustness of the results to different windows of “Pell Letter” receipt, examining the effect of having a letter in the first four or five months from first UI receipt (Table A3). Estimates are similar across specifications and are similarly robust to the inclusion of state by year trends (Table A4), the restriction of the sample to individuals in states that ever sent a “Pell Letter” (Table A5), and the inclusion of baseline earnings controls (Table A6). Placebo tests that assign the letters based on the counterfactual of moving an individual's month of UI receipt by 12 months in either direction (which affects only the assignment of treatment, not the underlying data used in estimation) show no effect (Table A8).

¹⁴To be explicit, we took the actual patterns of letter sending observed in Figure 1 and randomly reassigned them to states. So, in one iteration, Alaska's pattern of sending might be assigned to Michigan, Arkansas' to Ohio, and so on. Alternative strategies (for example, randomly choosing start and stop months for letter sending within each state) produce similar p -values.

TABLE 2—EFFECT OF PELL LETTER ON ENROLLMENT

| Variables | (1) | (2) | (3) | (4) |
|--------------------------|-------------------|-------------------|-------------------|-------------------|
| <i>Panel A. SIPP</i> | | | | |
| Enrolled (next 6 months) | 0.041 (0.018) | 0.039 (0.018) | 0.039 (0.019) | 0.051 (0.019) |
| R^2 | 0.082 | 0.083 | 0.099 | 0.132 |
| Mean | 0.097 | 0.097 | 0.097 | 0.110 |
| Full-time | 0.047 (0.015) | 0.046 (0.016) | 0.047 (0.016) | 0.054 (0.020) |
| R^2 | 0.084 | 0.084 | 0.101 | 0.134 |
| Mean | 0.065 | 0.065 | 0.065 | 0.082 |
| Part-time | -0.003 (0.012) | -0.005 (0.012) | -0.005 (0.012) | -0.001 (0.013) |
| R^2 | 0.071 | 0.072 | 0.084 | 0.122 |
| Mean | 0.044 | 0.044 | 0.044 | 0.044 |
| Observations | 1,472 | 1,472 | 1,472 | 1,042 |
| <i>Panel B. BAM</i> | | | | |
| In training | 0.025 (0.008) | 0.025 (0.008) | 0.027 (0.008) | 0.025 (0.011) |
| R^2 | 0.042 | 0.042 | 0.069 | 0.078 |
| Mean | 0.075 | 0.075 | 0.075 | 0.085 |
| Observations | 5,154 | 5,154 | 5,154 | 3,764 |
| Year-month fixed effects | Yes | Yes | Yes | Yes |
| Unemployment rate | | Yes | Yes | Yes |
| Education controls | | | Yes | Yes |
| Education restrictions | | | | Yes |

Notes: Panel A presents estimates from the SIPP and panel B presents estimates from the BAM. Each cell represents a separate OLS regression. For panel A, each observation corresponds to an individual's first spell of unemployment insurance receipt and the dependent variable is whether an individual is enrolled within six months of the first month of UI receipt. For panel B, the dependent variable is whether an individual is enrolled in any type of training program in the observed week. The explanatory variable of interest indicates whether a Pell Letter was sent in an individual's state during any of the first six months since the first month of UI receipt. All specifications include age, race, and gender indicator variables as well as state fixed effects. "Education controls" are indicator variables for all attainment levels. "Education restrictions" limits the sample to individuals with at least a high school degree, but less than a bachelor's degree at initial UI receipt. All regressions restricted to individuals aged 20–40 who first received UI between November 2008 and November 2010. In panel A, the sample is also restricted to individuals who were not enrolled during the month prior to first UI receipt. In panel B, the sample is also restricted to individuals who were observed within 75 days of initial UI receipt. Robust standard errors clustered at the state level are in parentheses.

To address this concern, we restrict the window of observation to individuals observed fewer than 75 days after initial receipt of UI benefits. We find that under this upper bound, exposure to the letter has on no effect on UI receipt duration.¹⁵

¹⁵We examine the effect of letter exposure on UI benefit duration using the following specification: $Duration_{ist} = X_i\beta_1 + \alpha_s + \lambda_t + \gamma Letter_{ist} + \epsilon_{ist}$, where $Duration_{ist}$ is a continuous variable indicating how many days an individual has been receiving UI benefits; X_i are indicator variables for age, race, and gender characteristics; and α_s and λ_t are state and year-month fixed effects for the state and month of initial UI receipt. $Letter_{ist}$ is whether a letter was sent to UI recipients of the individual's type within the six months subsequent to initial UI receipt. The estimate of γ is 0.599 (standard error 0.486).

We next turn to the effect of letter exposure on the training outcomes of interest. We use similar specifications to those used with the SIPP. We find that exposure to the letter increases the likelihood of enrollment in training by 2.5 percentage points, roughly 33 percent (Table 2). As with the earlier results, we explored the robustness of the standard error estimates using randomization inference (we randomly reasigned the timing of letter sending to states and reestimated the main specification 1,000 times). This process produced a similar, but slightly larger, p -value (0.015).

Mean enrollment levels (7–8 percent) are somewhat lower than in the SIPP; this is because we are only able to see a snapshot of enrollment behavior relatively soon after initial UI receipt and not whether an individual enrolls at all during the six month period after initial UI receipt. Indeed, when we construct a similar statistic in the SIPP, the mean enrollment levels are much closer (5–6 percent). Despite these mean differences, the percentage increase in enrollment in the BAM (33 percent) is quite similar to what we find in the SIPP (40 percent), suggesting that the difference between the level effects is mainly a result of differences in measurement.¹⁶

One of the advantages of the BAM data is that they differentiate between types of training, which is defined broadly to include enrollment in postsecondary programs. In Table A12, we find that the effect on training is coming about both through an increase in academic training and through an increase in vocational training (both increase by over 30 percent). In contrast, we find no effect on the share of individuals enrolled in WIA training. We are hesitant to draw strong conclusions, but the lack of a WIA effect suggests that our estimates are not a product of changes in WIA funding.

These estimates may appear quite large for such a light touch intervention; recall that the letter merely encouraged training, provided information about the Pell Grant program and educational opportunities, and noted likely Pell Grant eligibility for UI recipients. However, comments from state workforce agencies at the time suggest the response may have been quite large. For example, Washington noted “approximately a 20 percent increase in traffic” at its colleges; Utah indicated that the number of UI approved training participants “increase[d] 169 percent for the four months since the state wide notification letters were sent”; Rhode Island noted “a substantial increase in the number of claimants requesting approval to attend training”; Missouri “had a tremendous increase in the number of claimants ... that are receiving a Pell Grant”; and Florida noted “a dramatic increase in the number of inquiries to its call center” (NASWA 2010). The letters were generally very simple, contained the authority implicit in correspondence from state government, and contained easy and direct instructions on how to proceed to pursue training and obtain a Pell Grant. Given the existing training information and encouragement provided to most UI recipients, this nudge might have been quite important. Furthermore, the intervention not only provided information to potential students, but also primed and informed advisers who were in place to help UI recipients with this process. This last component of the intervention may have contributed greatly to the observed shifts in behavior. Finally,

¹⁶Thinking about this a different way, we can scale the SIPP estimates by multiplying them by the fraction observed enrolled two to three months after initial UI receipt (0.05 or 0.06) versus during the first six months after initial UI receipt. This procedure implies estimates of around 0.02, very close to the BAM estimates.

the weak labor market during the period the intervention was introduced may have produced a low opportunity cost of enrollment for UI recipients, magnifying the effect of the intervention.

B. *Effects on Financial Aid and FAFSA Filing*

One salient feature of the intervention was the direct reference to special consideration for financial aid; this was presented alongside resources available to help individuals with the application process. For example, the model letter presented by the Department of Labor notes “Colleges or One-Stop Career Centers can also help you apply for Pell Grant scholarships and other federal financial aid. An application is also available at www.fafsa.gov or by calling 1-800-4FEDAID.” This emphasis on the availability of federal financial aid would be expected to impact aid applications (filing the FAFSA) and Pell eligibility. The structure of the SIPP data likely introduces considerable measurement error in the recording of federal financial aid, including the Pell Grant, so we turn to administrative data from the FAFSA to assess changes in aid receipt.¹⁷

Administrative data on the counts of FAFSAs filed by dislocated worker status, state, year, month, and Pell eligibility provide important information that corroborates the evidence from the micro-level survey data available in the SIPP and the BAM administrative data. Using a similar strategy as with the individual-level data, we regress the number of FAFSA applications (a prerequisite to Pell receipt), specified in logs, on the “Pell Letter” indicator with state and year-month fixed effects. In columns 1–5 of Table 3, we provide supporting evidence for our earlier results. If the “Pell Letters” are driving the observed increases in enrollment, we should see corresponding increases in FAFSA filing for dislocated workers. The estimate in column 1 indicates that the sending of a “Pell Letter” in a particular state and month increased the number of FAFSAs filed by dislocated workers by roughly 10 percent.¹⁸

It is difficult to compare this result with the SIPP results for at least two reasons: it is not possible to determine the date of dislocation for individuals who are filing FAFSAs; and the population of dislocated workers (which includes the previously self-employed, displaced homemakers, and spouses forced to quit due to relocation of an active-duty spouse) is not equivalent to the population of UI recipients. However, under the reasonable assumption that dislocated workers are equally likely to file their FAFSA during each of the four months following dislocation, a back of the envelope calculation suggests that the FAFSA “Pell Letter” estimate (9.9 percent) is

¹⁷ The Pell Grant receipt question in the SIPP is only asked one time per wave, limiting our confidence in drawing strong conclusions about how the intervention affected Pell receipt or financial aid administrators’ treatment of dislocated workers’ FAFSAs. What is more, given that these questions may be answered by a spouse, we expect that responses about enrollment are well recorded while recall on the names of specific aid programs, including Pell, may be more limited. Consistent with this hypothesis, mean levels of Pell receipt are low (about 1/3) given the prevalence of Pell receipt among independent undergraduates. Table A9 presents the baseline regression for Pell receipt in the SIPP, and the results show an increase of about 2 percentage points, substantially smaller than the enrollment estimates in Table 2.

¹⁸ As with the prior estimates, we explored the robustness of the standard error estimates using randomization inference. This process produced a similar, but slightly larger, p -value (0.020).

TABLE 3—EFFECT OF PELL LETTER ON 2009 FAFSA SUBMISSIONS (*in logs*)

| Variables | Dislocated individuals | | | | | | Not dislocated (7) |
|------------------|------------------------|------------------------------|-------------------------------|------------------|------------------|-------------------|--------------------|
| | All (1) | Bachelor's/ acad. assoc. (2) | Certificate/ tech. assoc. (3) | < 30 (4) | ≥ 30 (5) | Pell eligible (6) | |
| Pell Letter | 0.099 (0.032) | 0.079 (0.027) | 0.109 (0.045) | 0.063 (0.024) | 0.117 (0.039) | 0.083 (0.030) | 0.009 (0.018) |
| Mean (in levels) | 2,864 | 1,326 | 1,495 | 1,043 | 1,808 | 2,079 | 14,472 |
| Observations | 588 | 588 | 588 | 588 | 588 | 588 | 588 |

Notes: Administrative FAFSA submission data provided by the Department of Education. Sample restricted to 2009 FAFSA cycle and months 1–12 (January through December). Each cell presents a separate regression. Each observation corresponds to a state and year-month. The dependent variable is indicated by the column header. Dependent variables are in logs. For example, column 1 has the log of the number of FAFSAs filed by all dislocated workers and column 2 has the log of the number of FAFSAs filed by dislocated workers for bachelor's or academic associate's degree programs. The explanatory variable of interest indicates whether a Pell Letter was sent in that state in that year-month. All specifications include state fixed effects and year-month fixed effects. Robust standard errors clustered at the state level are in parentheses.

in line with what we would expect (10 percent) based on our enrollment results from the SIPP.¹⁹ More importantly, observing a contemporaneous increase in FAFSA filings among dislocated workers lends credence to our main estimates from the SIPP data.

The intervention appears to have increased the total number of Pell eligible FAFSAs filed by dislocated workers by about 8.3 percent (column 6 of Table 3). That the relative increase in Pell eligible cases is no larger than the increase in FAFSA filings suggests that financial aid administrators may not have adjusted their treatment of dislocated workers. Estimated directly, the timing of letter sending does not correspond with an increase in the *share* of dislocated worker FAFSAs that are determined eligible. If the timing of the letter campaign was coordinated with changes in the treatment of dislocated workers aid eligibility, we would expect the share of dislocated worker FAFSAs that are Pell eligible to increase.²⁰ Furthermore, there is no discernible positive trend in the share of dislocated worker FAFSAs determined eligible during 2009. To summarize, consistent with the anecdotal evidence presented above, the empirical evidence suggests that there were limited changes in the financial aid treatment of dislocated workers.

The aggregate FAFSA data also allow us to learn more about the programs the marginal enrollees likely pursued. In columns 2 and 3, we see suggestive evidence that more of the marginal enrollment accrued at shorter term certificate or technical associate's programs. Columns 4 and 5 suggest the intervention had a stronger *percentage* effect on older individuals; this is consistent with estimates from the SIPP and the BAM, where similar level effects translate into larger percentage effects for older UI recipients (Table A11).

¹⁹ Online Appendix B provides the details of this simple example that reconciles the disparity in magnitudes. We emphasize that this is just one of many reasonable sets of assumptions that reconcile the disparity in magnitudes.

²⁰ The lack of an increase may be a result of an offsetting change in the composition of applicants (i.e., the letter may have induced individuals who were *ex ante* less likely to be Pell eligible to apply), but it is not clear why this would be the case.

C. Threats to Validity

Policy Endogeneity of “Pell Letter” Distribution.—Whereas the initial policy innovation was a direct federal response to rising unemployment, variation in the rollout of the policy occurred at the state level. Because we are controlling for over time variation with year-month fixed effects, the concern is whether states happened to send “Pell Letters” in months in which new UI recipients were more likely to enroll for some other reason. For example, states may have decided to send the letters when labor market conditions were at their worst. If this were the case, we might observe greater enrollment of UI recipients who received “Pell Letters” merely due to the lower opportunity cost of enrollment. Other state policy changes such as adjustments in appropriations or grant aid may occur annually within states; there is no evidence to suggest that such changes are related to the intervention.

The states’ own responses suggest that the timing and duration of letter sending was largely idiosyncratic (NASWA 2010), and empirical evidence is consistent with this claim. Were the intervention aligned with state-level changes that promote enrollment of UI recipients, we would expect to see a trend in enrollment leading up to the “Pell Letter” dissemination within states. The larger sample sizes and additional pre-intervention data in the BAM provide us with an opportunity to explore this visually. Figure 3 plots coefficients for indicator variables for each two-month window of initial UI receipt relative to the first period in each state during which treatment was equal to one. The event study figure illustrates the lack of trends in enrollment prior to the initial sending of a letter in a state.

We further address endogeneity concerns related to the sending of the letters using two placebo exercises which reveal no effect for two groups of potential students during the months the letter was sent. First, there is no significant increase in the total number of FAFSAs filed when those filed by dislocated workers are excluded (column 7 of Table 3). Second, we explore the existence of effects for unemployed individuals who do not report receipt of unemployment benefits in the SIPP. While, a priori, we might expect some small effect on these individuals as a result of measurement error in UI receipt, empirically we find no effect (coefficient 0.007, standard error 0.020).

Another potential concern is that states may have changed their approved training rules at the same time that they began sending letters.²¹ To explore this, we collected information from the US Department of Labor, Employment, and Training Administration’s 2008 through 2011 comparison of state UI laws. Because the information is only available at a coarse (annual) level, we are unable to conduct regression analyses to determine whether these changes are correlated with treatment. However, there were very few changes in approved training rules during this time period. In contrast, a survey of state workforce agencies in 2009 suggests some changes in the interpretation of approved training rules may have taken place around the time of the American Recovery and Reinvestment Act of 2009. In particular, four states began allowing enrollment in some four-year postsecondary

²¹ As shown in Barr and Turner (2015), these training rules may influence whether unemployed individuals enroll in college.

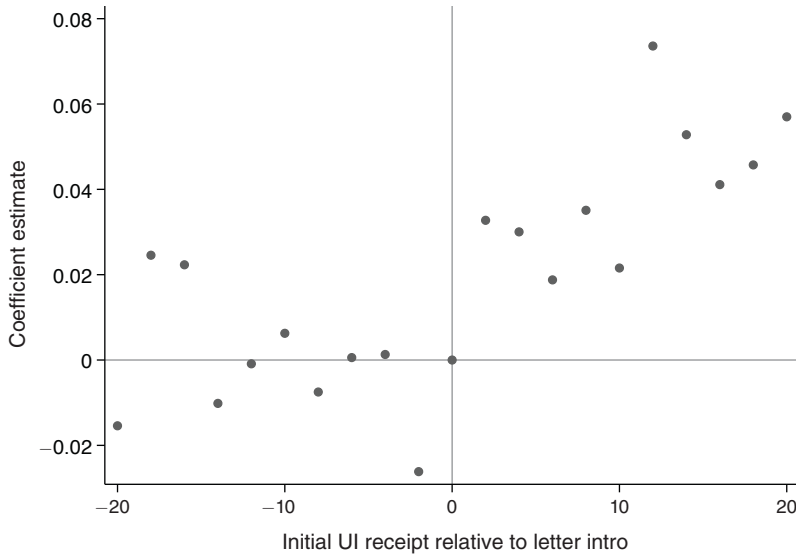


FIGURE 3. BAM TRAINING EVENT STUDY

Notes: The figure plots coefficients on indicator variables for each two-month window of initial UI receipt relative to the first two windows in each state during which treatment was equal to one (for example, in states that sent letters only to new UI recipients, the first month of letter sending). The dependent variable is whether an individual is enrolled in any type of training program in the observed week. All specifications include age, race, and gender indicator variables as well as state and year-month fixed effects. All regressions are restricted to individuals aged 20–40 who first received UI between January 2008 and November 2010 and were observed within 75 days of initial UI receipt. Data are from the BAM. Robust standard errors are clustered at the state level.

programs, and two states began allowing academic courses “not leading to a specific occupation” as approved training. This is potentially a concern if the timing of these changes was similar to the timing of “Pell Letter” sending. We address this concern by simply excluding these states from the analysis; this leaves the estimates largely unchanged (Table A14).

UI Receipt Endogeneity.—A final potential concern is the endogeneity of UI receipt. As UI receipt is a necessary condition for receiving a “Pell Letter,” restricting the sample to UI recipients is a natural way to focus the estimation on those individuals eligible for the treatment. However, UI receipt is not entirely exogenous. If individuals with greater propensities to enroll increase UI uptake, the results may be driven by selection into UI receipt and not by receipt of a “Pell Letter.” Such effects might occur if there are informational spillovers about the letter from friends or family which induce UI receipt with the intent of enrollment.

One way to address this concern is to assign treatment based on the month of initial unemployment spell for individuals experiencing layoffs instead of the month of initial UI receipt. Because an individual does not choose to experience a layoff, assigning treatment this way circumvents any potential selection concerns associated with using initial UI receipt. We derive an individual’s first month on

TABLE 4—CHARACTERISTICS OF THOSE SHIFTED BY LETTER (*BAM data*)

| Group | Observations (1) | P(E = 1) (2) | Fraction compliers (marginal effect) (3) | P(Letter = 1) (4) | P(Complier E = 1) (5) | Fraction of all compliers (6) |
|--------------------|---------------------|-----------------|--|----------------------|--------------------------|---|
| All | 5,154 | 0.075 | 0.025 (0.008) | 0.435 | 0.145 | 1 |
| Black | 976 | 0.084 | 0.062 (0.017) | 0.442 | 0.326 | 0.477 |
| Female | 2,019 | 0.098 | 0.044 (0.015) | 0.435 | 0.195 | 0.689 |
| Age > 30 | 2,708 | 0.058 | 0.029 (0.011) | 0.452 | 0.218 | 0.611 |
| Hourly wage ≤ \$10 | 1,138 | 0.081 | 0.057 (0.017) | 0.457 | 0.335 | 0.530 |

Notes: Column 2 provides the proportion of each subgroup in training. Column 3 contains the marginal effect of treatment (i.e., letter exposure) for each subgroup. See online Appendix Table A10 for estimates underlying column 3. Column 4 indicates the proportion of each subgroup that was treated (i.e., exposed to the letter). Column 5 indicates the fraction of individuals in training who were there as a result of treatment, by subgroup. Column 6 indicates the fraction of all compliers (i.e., those shifted into training by the letter) with various characteristics. Data are from the BAM.

layoff as the first month experiencing any layoff following at least two months with full month employment. We then assign treatment, expected “Pell Letter” receipt, analogously to how we used the first month of UI receipt previously. As many of these individuals are not UI eligible and/or will not choose to receive UI, we expect smaller treatment effects. In Table A17, we demonstrate that the effects using this strategy are similar, but 20 to 50 percent smaller than those in Table 2.

D. Examining Heterogeneity Using BAM Data

Of interest is whether some subgroups were more responsive than others to the treatment and, in turn, which groups accounted for the bulk of compliers. The BAM’s greater sample size allows us to explore heterogeneity in effects. Table 4 summarizes these differences across groups. Column 2 of Table 4 indicates the proportion of each subgroup enrolled, and column 3 shows the group-specific marginal effect obtained by interacting indicators for group membership with the expected “Pell Letter” receipt variable (see Table A10 for full results).²² We find larger level effects for blacks and women. While the larger effect for women is consistent with some recent research on college interventions (Carrell and Sacerdote 2013; Barr, Bird, and Castleman 2017), the larger effect for blacks has not been

²²The statistical power provided by the SIPP sample does not allow us to provide meaningful estimates of most types of heterogeneity. Where we are able to produce comparable estimates, the SIPP and BAM estimates are not statistically distinguishable at the 90 percent confidence level. However, in the case of the black interaction estimate, the confidence interval from the SIPP analysis is extremely large. The female effect is quite similar with a main effect of 0.018 (standard error 0.020) and an interaction effect of 0.032 (standard error 0.033).

identified previously.²³ As these individuals tend to have lower earnings potential and experience longer unemployment spells, it may be the case that the barriers addressed by the information and assistance are particularly salient for those who are the most vulnerable. This is consistent with the pattern of results observed when we interact our treatment indicator with the baseline hourly wage of UI recipients; individuals with lower baseline hourly wages are more likely to be affected by the intervention. Table A10 provides similar estimates using a continuous measure of wages. For minimum wage earners, the treatment effect is estimated to be about 3.5 percentage points, but a \$5 increase in baseline hourly wages reduces the treatment effect by an entire percentage point.

To understand the characteristics of the marginal individual affected by the treatment, we consider the likelihood of treatment (column 4 of Table 4), the fraction of enrollees who were shifted into enrollment by the treatment (column 5), and the distribution of compliers (column 6), by demographic group. There is little difference in the likelihood of treatment across groups (column 4), so the distribution of complier characteristics is largely driven by the heterogeneity in treatment effects (described above) combined with the demographics of UI recipients. Despite accounting for 19 percent (black) and 25 percent (*HourlyWage* ≤ \$10) of UI recipients, our estimates suggest that half of compliers are black and earned low wages at baseline. While these estimates suggest an outsized influence on these populations, our confidence intervals are too large to draw strong conclusions.

E. Educational Attainment and Labor Market Outcomes

One indicator of the return to the information policy is the persistence of enrollment, as enrollees who do not even complete a term might be considered “mismatched” with college. Although there are some issues with sample attrition, we investigate treatment effects on the total number of months enrolled at several points after initial UI receipt.²⁴ In Table 5, we see that treatment results in a significant increase in the number of months enrolled. At 12 months out from initial UI receipt, treated individuals have accrued an additional 0.34 months on average. If we assume that the point estimate on full-time enrollment (0.05) captures the shift into additional enrollment, the 0.34 estimate implies an additional (0.34/0.05) 7 months of enrollment per affected individual over the 12 months since initial UI receipt. The effect is even larger at 24 months out. At this point, treated individuals have accrued about 0.6 additional months on average, implying an additional 12 months of enrollment per individual affected. We also explore effects on the number of years completed, illustrating similar effects in online Appendix Table A13.

²³ The larger point estimate for blacks is robust to specifications that interact black with state and year-month fixed effects as well as the unemployment rate. We have also estimated our main specification using only black UI recipients in the BAM. This results in a somewhat smaller point estimate (0.038, standard error 0.025) that is statistically indistinguishable from the marginal effect for blacks derived from the estimates underlying Table A10.

²⁴ As we look at outcomes further from initial UI receipt, sample attrition worsens. We interpret these results with caution, but also note that the basic 6-month enrollment results persist when we restrict the sample to individuals who remain in the sample 12, 18, 24, 30, or 36 months. Furthermore, treatment has no effect on attrition using the basic regression framework.

TABLE 5—LONGER RUN EFFECT OF PELL LETTER ON EDUCATION AND LABOR MARKET OUTCOMES

| | Observations | Enrolled (1) | Months enrolled (2) | UI receipt (3) | Employment (4) | Earnings (5) |
|----------------------|--------------|-------------------|---------------------------|----------------------|-------------------|-------------------|
| 12 months out | 1,151 | 0.065 (0.019) | 0.339 (0.144) | 0.068 (0.048) | -0.039 (0.044) | -1,966 (2,369) |
| 18 months out | 1,057 | 0.039 (0.025) | 0.502 (0.198) | 0.061 (0.035) | -0.059 (0.034) | -1,789 (2,010) |
| 24 months out | 957 | 0.025 (0.027) | 0.594 (0.248) | 0.026 (0.022) | -0.007 (0.041) | -8.62 (2,489) |
| 30 months out | 839 | 0.034 (0.028) | 0.703 (0.312) | -0.007 (0.024) | -0.014 (0.037) | -79.1 (2,379) |
| 36 months out | 698 | -0.002 (0.024) | 0.821 (0.359) | -0.024 (0.035) | -0.031 (0.049) | 1,112 (2,300) |
| Mean (12 months out) | | 0.098 | 0.688 | 0.363 | 0.513 | 17,720 |
| Mean (18 months out) | | 0.110 | 1.045 | 0.217 | 0.605 | 21,531 |
| Mean (24 months out) | | 0.100 | 1.387 | 0.151 | 0.645 | 22,328 |
| Mean (30 months out) | | 0.098 | 1.695 | 0.082 | 0.714 | 25,238 |
| Mean (36 months out) | | 0.065 | 1.905 | 0.108 | 0.710 | 23,964 |

Notes: Each cell presents a separate regression. Each observation corresponds to an individual's first spell of UI receipt. The dependent variable in columns 1, 3, 4, and 5 is a measure corresponding to enrollment, UI receipt, employment, or earnings in a three-month window beginning 12, 18, ..., 36 months following initial UI receipt. For example, enrollment at 12 months is whether an individual is observed enrolled in month 12, 13, or 14. UI receipt and employment are constructed analogously. Earnings is constructed in annual terms using any earnings observations available during the three months following the time period in question. More specifically, we multiply the average reported earnings in the following three months (e.g., months 12, 13, 14) by 12. The dependent variable in column 2 is the total months an individual is observed enrolled over the relevant period. Estimates presented are equivalent to those from main specification underlying column 2 in Table 2. The explanatory variable of interest indicates whether a Pell Letter was sent in an individual's state during any of the first six months since the first month of UI receipt. All regressions are restricted to individuals aged 20–40 who were not enrolled during the month prior to first UI receipt and first received UI between November 2008 and November 2010. Data are from the 2008 SIPP. See Table 1 and online Appendix Table A18 for means. Robust standard errors clustered at the state level are in parentheses.

Given the shift to more and more intense training, we would expect that treated individuals might remain unemployed and receiving UI longer. The estimates in Table 5 show that this was the case; treated individuals are several percentage points more likely to receive UI at 12 and 18 months after initial UI receipt. The effects on employment and earnings are a mirror image with negative point estimates over the same period (Table 5).²⁵ The confidence intervals frequently include zero, but the point estimates suggest large reductions in earnings for those affected by the intervention.

Analogous point estimates for employment and earnings two to three years after initial UI receipt indicate no significant difference for treated individuals (Table 5). Although point estimates suggest that these individuals continue to be less likely to be employed and earn less, we cannot rule out earnings increases of as much as 20 percent. Furthermore, we interpret these results with caution due to the relatively

²⁵Earnings are constructed in annual terms using any earnings observations available during the three months following the period in question. More specifically, we multiply the average reported earnings in the following three months (e.g., months 6, 7, and 8) by 12.

large level of attrition observed towards the end of the SIPP panel. Online Appendix Figure A1 shows the trajectory of residualized earnings for treated and untreated individuals. Consistent with the regression results, treated individuals have lower earnings in the 18 to 24 months after initial UI receipt at the same time that they are more likely enrolled. By 36 months, their earnings appear to track closely with those of untreated individuals.

The earnings outcomes are consistent with the finding that the intervention caused UI recipients to pursue additional postsecondary training. This training came at the expense of lost earnings during at least the first 18 months following initial UI receipt. Furthermore, these individuals were more likely to take advantage of Pell Grants and remained on UI many months following initial receipt, which represents an additional cost of the intervention. A lingering question is whether this enrollment resulted in attainment gains that translated into better labor market outcomes, and whether the improved outcomes are worth the associated costs. Two to three years out, the evidence suggests moderate increases in college attainment and no significant gains in earnings. The point estimates suggest that the intervention increased government expenditures without resulting in improved outcomes for individuals, but the confidence intervals are too wide and the window of observation too short to rule out meaningful positive effects on earnings.

IV. Discussion and Conclusion

Despite eligibility for considerable federal financial aid and low opportunity costs to return to school for the unemployed, relatively few do. We ask whether the low postsecondary participation rates reflect limited information and hassle costs or, alternatively, expected returns that are low. One concern is that the services available to the unemployed, specifically those related to the enrollment and financial aid application process, are particularly difficult to navigate (Babcock et al. 2012). These types of issues may be especially salient for the unemployed because they lack the assistance and support services provided to those on a more traditional college trajectory. We examine the extent to which informational outreach may increase enrollment among UI recipients by providing cues and salient information consistent with the lessons of behavioral economics.

The “Pell Letter” initiative introduced by the federal government in May of 2009 presents a source of variation in dissemination similar to a randomized control trial, but implemented at scale (more than 20 million individuals were sent the letters). The idiosyncratic nature of state action in response to this federal policy guidance creates an extraordinary experiment and allows us to examine how an “informational nudge” affects decisions to pursue postsecondary attainment among the unemployed. In addition to providing information about the availability of financial aid and training options, the letters also facilitated engagement with a number of intermediaries, including employment services offices and local financial aid administrators, who were prepared to assist individuals responding to the letter.

The overall enrollment effects are striking: we observe enrollment increases of 4 to 5 percentage points, which are about 40 percent above the baseline. These results are all the more impressive because estimates from most prior research would suggest

that among recent high school graduates, it would take an increase in financial aid of between \$1,000 and \$1,500 to produce this magnitude of enrollment response (Dynarski and Scott-Clayton 2013).²⁶ Given the scale of the letter intervention, which we estimate was sent to over 20 million UI claimants, we estimate it resulted in at least half a million additional individuals enrolling in college in the year following initial UI receipt.²⁷

Evidence presented in this paper, as well as surveys of workforce officials and aid administrators, suggests that the coordinated provision of information to both UI recipients and those tasked to assist them contributed to substantial increases in post-secondary enrollment. These findings are consistent with prior research that suggests information may be particularly effective when it facilitates access to assistance or simplifies the process of uptake (e.g., Hoxby and Turner 2013; Castleman and Page 2015; and Barr, Bird, and Castleman 2017). The effects are larger for women than men. We also find particularly large effects for black UI recipients and UI recipients with relatively low earnings potential at baseline, suggesting that differences in the accessibility of information about program benefits and the steps necessary to access them may contribute to persistent inequality. The effects are also larger in states and months in which labor market conditions are worse, suggesting that this type of intervention is likely to be particularly effective during times when the opportunity cost of pursuing training is relatively low. The broad takeaway in terms of policy is that it makes more sense to implement this type of intervention when the opportunity cost of pursuing training is relatively low.

Of course, open questions remain about whether, even during a period of weak labor market conditions, the intervention was likely to have generated substantial benefits for individuals or society. At the individual level, one must weigh the additional months of UI benefits and discounted potential wage benefits from additional credit accumulation (and potential degree receipt) against the net cost of schooling and the opportunity cost (foregone wages) of enrolling. Two to three years out, the evidence suggests modest increases in college attainment and no significant gains in earnings. However, the confidence intervals are too wide to rule out meaningful positive effects on earnings; furthermore, two to three years is likely too short a period for the average enrollee to realize higher earnings. If we instead assume the estimated earnings effects in Jacobson, LaLonde, and Sullivan (2005a,b), the gains to displaced workers could be more clearly positive. Multiplying the additional 0.1 years of college education by the assumed gain in earnings for a year of community college (9 percent), the baseline annual earnings level (\$25,000), and the number of additional working years (32), and the average treated individual in our sample

²⁶ More recent work estimating the effects of financial aid on older students (Barr 2015, forthcoming) suggests that a much larger amount would be necessary.

²⁷ Focusing on just our period of analysis, there were nearly 34 million initial claims for UI between June 2009 and November 2010. Using our survey information, we estimate that states sent letters to over 13 million of these claimants. Statistics from the BLS indicate that roughly half of UI recipients are between 20 and 40. If we assume that the letters had no effect on individuals outside of this age range and use a 5 percentage point effect of the letters on enrollment, the back of the envelope estimate of the increase in enrollment is $13,000,000 \times 0.5 \times 0.05 = 325,000$. If we extend the period to include 2011 and continue with these conservative assumptions, we estimate an increase in enrollment of 570,000. Under either calculation, this corresponds to 2 percent of total enrollment in the affected states.

would stand to earn an additional \$7,200 over their lifetime, about \$4,400 in present discounted value. This estimate is conservative because it assumes a low estimate of earnings and a fixed earnings level. Even under these conservative assumptions, the gain in earnings is roughly equivalent to the average earnings lost over the two to three years since initial UI receipt. Thus, individuals are likely better off if Jacobson, LaLonde, and Sullivan's estimates are valid in this population. However, the gain to society is less clear. Although the cost of the letter sending itself was quite low, the additional UI and financial aid costs imply that the intervention may have been a net cost to society.

Despite uncertain welfare implications, our findings suggest that well coordinated information interventions may be substantially more cost-effective than raising the generosity of existing government programs in increasing postsecondary participation. This analysis, along with the growing and important body of research on the effects of financial aid on college enrollment and attainment, leaves open the important social welfare question of whether interventions that induce college enrollment for nontraditional student populations (like UI recipients) provide benefits in earnings and future employment in excess of costs.

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