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Paid fast, paid few: Payment timeliness, denials and recipiency in unemployment insurance, 2016 to 2024

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ABSTRACT

US unemployment insurance is now an online-first, rule-driven public system, and the federal government judges each state by how fast it makes first payments. That timeliness measure counts only claimants who have already passed the system's eligibility rules. This article sets it beside recipiency, the share of unemployed people who are actually paid. I combine Department of Labor administrative reports for 50 states and the District of Columbia with federal unemployment counts and survey data on household broadband for 2016–2024. Before the pandemic, 44 states met the 87% timeliness standard on a 21-day reading, yet 18 of them paid fewer than one in five unemployed people. Across states, timeliness and recipiency were unrelated in every year (Pearson r between −0.07 and 0.16). After 2021, timeliness had not recovered (81.3% within 21 days nationally in 2024, against 91.1% in 2019), while nonseparation denials rose from 236 to 316 per 1,000 initial claims, driven by denials for reporting requirements and for availability or work search. Broadband share was associated with recipiency between states, but not once state income was considered, and not within states over time. The decisive filtering in this system happens in rule-based continuing eligibility checks that the timeliness measure does not see. Evaluation of algorithmic public administration should count who a system turns away as well as how fast it pays those it accepts.

Keywords: unemployment insurance, administrative burden, digital discretion, performance measurement, system-level bureaucracy, broadband

1. Introduction

Unemployment insurance (UI) in the United States is a joint federal and state programme run by 53 state agencies under federal rules. Most state systems are now built around online self-service: a person who loses a job files an initial claim, certifies eligibility every week or two through a form, and receives decisions that are generated or triggered by the agency's claims system. The caseworker who once took the claim at a local office has largely been replaced by forms, eligibility rules coded into software, and automated notices. Bovens and Zouridis (2002) called this kind of agency a system-level bureaucracy: discretion moves from front-line officials into the design of the information system itself.

The federal government evaluates state UI systems with a short list of core measures. The most visible one is first payment promptness: the share of first payments made within 14 or 21 days of the end of the first compensable week, with an acceptable level of performance of 87% (Electronic Code of Federal Regulations, 2026; U.S. Department of Labor, 2026a). The measure is precise, comparable and easy to report. It also has a structural blind spot. Its denominator is first payments, so it counts only people who have already been found eligible and paid. A person who never files, who is denied, or who drops out after missing a weekly certification never enters it.

The experienced side of the system is better captured by recipiency, the share of unemployed people who receive UI. The Department of Labor uses this concept in its own reporting (U.S. Department of Labor, 2021), and economists have long used it to study who is reached by the programme (Anderson & Meyer, 1997; Blank & Card, 1991). Recipiency is not a performance measure in the federal scheme. A state can therefore be judged a good performer on payment speed while paying a small fraction of its unemployed residents. This is the kind of gap the present article examines: a system evaluated on one set of measures and experienced on another.

The gap matters for the study of discretion in algorithmic public administration. If discretion now sits in system rules, then its effects show up less in the speed of a single payment and more in how many people the rules filter out. The federal UI data allow these filters to be observed at the state level. States report every nonmonetary determination and denial, the reason for each denial, every appeal decision and every first payment by time lapse. Together with labour-force data, these reports show how fast the system pays, how often its rules say no, and how many unemployed people end up paid.

Online-first claiming adds a further condition. A system that expects claimants to file and certify on the web assumes that they can get online. Household broadband is therefore a plausible precondition for access, and its absence a plausible source of what Moynihan et al. (2015) call compliance costs. Earlier work found that the move to telephone and internet claiming did not raise UI take-up (Ebenstein & Stange, 2010), but that study covered a period before online systems became the default. Whether digital access now shapes who is paid is an open question.

This article asks three research questions. RQ1: How did first payment timeliness, nonmonetary denials, appeal outcomes and recipiency change across US states between 2016 and 2024, including the pandemic break of 2020–2021? RQ2: Does meeting the federal timeliness measure tell us how many unemployed people a state's system reaches? RQ3: Is household broadband access associated with recipiency and with nonseparation

denials, net of labour-market conditions, state income and year effects?

The answers are direct. Timeliness and recipiency are unrelated across states in every year from 2016 to 2024. Many states that met the timeliness standard paid fewer than one in five unemployed people. After the pandemic, timeliness stayed well below its 2019 level, while denials based on continuing eligibility rules rose sharply. Broadband share does not explain differences in recipiency once state income is taken into account. The contribution is to show, with complete administrative data for all states, that the measure used to evaluate an online-first public system is blind to where that system now does most of its filtering.

2. Background and theoretical framing

2.1 From street-level to system-level discretion

Street-level bureaucracy research begins from the observation that front-line workers make policy through the many small judgements they apply to individual cases. Information and communication technology changes where those judgements are made. Bovens and Zouridis (2002) argued that in agencies such as tax and student-finance offices, decisions on individual cases are produced by systems, and that discretion passes to system designers, legal policy staff and IT managers. Buffat (2015) set out two positions on this shift: a curtailment thesis, in which technology reduces front-line discretion, and an enablement thesis, in which it gives workers new tools and new room for judgement.

Busch and Henriksen (2018) reviewed this literature under the label digital discretion. They found that technology most often reduces discretion when tasks are routine and rules are clear. UI eligibility has both features in large part. Weekly certification asks fixed questions about work, earnings, availability and job search. An answer outside the accepted range, or a missed certification, can trigger an issue that leads to a determination without any conversation with a person. Wenger and Wilkins (2008) studied this transition in UI directly. Using state panel data for 1992–2005, they found that telephone claims filing increased the number of women receiving benefits and had no effect for men, which they read as evidence that removing in-office discretion removed some biased judgement.

Discretion does not disappear when caseworkers leave. Bullock (2019) and Young et al. (2019) describe artificial discretion: decisions that once required human judgement are delegated to rules and models, with effects that depend on the task and on how the system is designed. Peeters and Widlak (2018) showed how a Dutch registry's information architecture could lock citizens into a digital cage, where an error in the data cannot be fixed by any single official. The common point is that in a system-level bureaucracy, the rules encoded in forms and software decide who gets through. Their aggregate effect should be visible in denial counts and in the share of eligible people who are paid.

2.2 Administrative burden in digital self-service

Administrative burden is the experience of policy implementation as onerous. Moynihan et al. (2015) divide it into learning costs, psychological costs and compliance costs, and argue that burdens are often policy choices made by other means (Herd & Moynihan, 2019). Herd et al. (2013) showed that when Wisconsin moved burdens from claimants to the state in Medicaid, take-up rose. A systematic review by Halling and Baekgaard (2024) found consistent evidence that burdens reduce take-up and that they fall hardest on people with fewer resources. Christensen et al. (2020) linked this unequal impact to differences in cognitive resources such as executive function.

Digital self-service changes who bears the burden. Madsen et al. (2022) describe the citizen as an accidental caseworker who must now do the work that officials once did: finding the right form, entering data correctly and following up on errors. Lindgren et al. (2019) set out an agenda for studying such digital encounters between citizens and the state. Schou and Pors (2019) showed how digital-by-default welfare in Denmark created new forms of exclusion for people who could not use online services. These accounts predict that online-first UI should lower burden for some claimants and raise it for others, and that rule-based checks inside the online process are a likely source of exclusion.

2.3 Digital access as a precondition

The digital divide literature distinguishes between access to the internet, the skills to use it and the outcomes people get from using it (van Dijk, 2006). Helsper and Reisdorf (2017) found that as internet access spread in Britain and Sweden, the people who remained offline were increasingly concentrated among the disadvantaged. Bélanger and Carter (2009) found that differences in internet access and use, along with income, education and age, were linked to use of e-government services in the United States. For UI, these findings suggest that household broadband should matter for whether people can file and keep certifying, and that the effect could appear in both recipiency and denials for reporting failures.

The evidence for UI is mixed. Ebenstein and Stange (2010) used state variation in the timing of remote claiming and found no effect on take-up, which they read as evidence against simple time costs as the main barrier. Anderson and Meyer (1997) and Auray et al. (2019) show that take-up depends strongly on the expected value of benefits and on eligibility rules. Forsythe (2023) estimated that about 88% of eligible unemployed people received benefits in the first year of the pandemic, which shows how far recipiency among the eligible can differ from recipiency among all unemployed people. None of these studies examines broadband directly in the period when online claiming became the default.

2.4 What performance measures see

Performance measures direct attention. Bevan and Hood (2006) showed that targets in English health care improved reported performance while leaving unmeasured aspects of care exposed, and that organisations sometimes met targets by gaming them. Courty and Marschke (2004) found that US job training agencies timed the reporting of outcomes to improve measured performance. Heinrich and Marschke (2010) review how such measures evolve and how agencies learn to meet them. The UI timeliness measure invites a similar concern. It rewards speed for those who reach a first payment. It is silent on how many people reach one.

Put together, these strands give the expectations for this study. If discretion has moved into system rules, then denials for rule-based reasons should be a main channel through which the system filters claimants, and recipiency should vary with those denials. If the timeliness measure is blind to filtering, it should be unrelated to recipiency. If digital access is a binding precondition, broadband share should be associated with recipiency and with reporting denials, both between states and within states over time.

3. Data and methods

3.1 Data sources

The unit of analysis is the state-year, for 50 states and the District of Columbia from 2016 to 2024 (459 state-years). Puerto Rico and the Virgin Islands were excluded because broadband and labour-force data are not comparable. UI data come from five federally required reports published by the Department of Labor's Office of Unemployment Insurance (U.S. Department of Labor, 2026b). Column meanings were taken from the office's published data maps. Each file had exactly one record per state and reporting period, so revised figures had already been merged by the agency and no deduplication was needed.

ETA 539 (weekly claims): continued weeks claimed under state UI, averaged over the weeks ending in each calendar year.

ETA 5159 (monthly claims and payments): initial claims and weeks compensated for total unemployment under state UI.

ETA 9050 (monthly first payment time lapse): intrastate state UI first payments by time band (7 days or less, 8–14, 15–21 days and so on). The bands summed exactly to the total in every state-month used.

ETA 207 (quarterly nonmonetary determinations): determinations and denials for single claimants, split into separation issues and nonseparation issues, with nonseparation denials by reason (able and available or actively seeking work, disqualifying income, refusal of suitable work, reporting requirements).

ETA 5130 (monthly appeals): lower-authority appeal decisions and decisions in favour of the appellant, in total and for claimant appellants.

Unemployment levels and rates come from the Bureau of Labor Statistics Local Area Unemployment Statistics programme, retrieved through its public API (U.S. Bureau of Labor Statistics, 2026). The API version used does not return annual averages, so each state-year value is the mean of the 12 monthly estimates. Household broadband and median household income come from the American Community Survey 1-year estimates, tables B28002 and B19013 (U.S. Census Bureau, 2026). The Census data API now requires a registered key, so the same estimates were taken from the Census Bureau's public summary files. Broadband share is households with a broadband subscription of any type divided by all households. The 2020 1-year release does not exist in standard form, so broadband and income are missing for 2020.

3.2 Measures

Timeliness is the share of intrastate state UI first payments made within 21 days, summed over the calendar year. The federal standard counts 14 days in states with a waiting week and 21 days in states without one, and evaluates performance over the 12 months ending 31 March (Electronic Code of Federal Regulations, 2026). Waiting-week status changes over time and could not be verified for every state and year, so I use the 21-day share as the main measure and the 14-day share as a stricter alternative. The 21-day reading is generous to waiting-week states. Counts of states meeting 87% at 21 days are therefore an upper bound on compliance.

Recipiency is the average number of weeks compensated per week (annual weeks compensated divided by the number of weeks in the year) divided by the average number of unemployed people, times 100. This follows the Department of Labor's concept of recipiency as people receiving UI divided by unemployed people (U.S. Department of Labor, 2021). It differs in two ways: the numerator covers state UI only, and the denominator is all unemployed people rather than those in covered employment. A second version uses continued weeks claimed in place of weeks paid. The two versions correlate at 0.97 across state-years.

Nonseparation denials are denials on issues that arise while a person is claiming, such as failing to report, not being able and available for work, or having disqualifying income. These are the issues most often raised by online weekly certifications. I express them per 1,000 state UI initial claims, which shows how often the system says no relative to the flow of new claims, and per 100 nonseparation determinations, which shows how often a determination ends in denial. Separation denials (about why the job ended) are reported for comparison. The appeal outcome is the share of lower-authority decisions in favour of the appellant.

The two denial measures answer different questions, and both are needed. Denials per 100 determinations describe the decision once an issue has been raised. Denials per 1,000 initial claims also capture how often the system raises issues in the first place, which depends on how strictly weekly certifications are checked and how readily an unusual answer is turned into an issue. A system can deny the same share of the issues it examines and still deny far more people if it examines more cases. For an online-first system, the second measure is the closer indicator of how much filtering the coded rules do.

3.3 Analysis

The analysis has three parts that match the research questions. For RQ1, I compute national series by summing numerators and denominators across the 51 jurisdictions, and state means for 2016–2019, 2020–2021 and 2022–2024. For RQ2, I correlate timeliness and recipiency across states in each year, count states that meet the 87% level while paying fewer than 20% of unemployed people, and estimate a within-state model of recipiency on timeliness with state and year fixed effects.

For RQ3, I estimate linear panel models of the form outcome = β·broadband + γ·unemployment rate + state effect + year effect, using the eight years with broadband data (408 state-years). Standard errors are clustered by state. Variants drop 2021, add log median household income, and use claimed rather than paid weeks. Because state fixed effects remove all stable differences between states, β is identified only from changes in broadband within states. I therefore also report pooled models with year effects only and a between-state cross-section of state means,

with and without income and Census region. The cross-section uses heteroskedasticity-consistent (HC3) standard errors. All analysis was done in R with the fixest and sandwich packages, and all code runs from the raw downloads.

4. Findings

4.1 Trends from 2016 to 2024

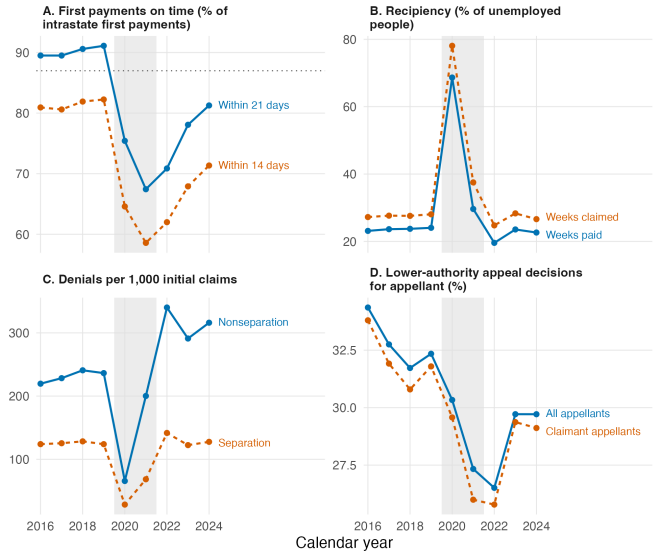


Figure 1 shows the national series. Before the pandemic, the system was stable. Nationally, 89.5% to 91.1% of intrastate first payments were made within 21 days in 2016–2019, and 80.6% to 82.3% within 14 days. Between 42 and 46 states met 87% on the 21-day reading in each of these years. Recipiency was also stable and low: weeks paid equalled 23.1% to 24.0% of unemployed people, and weeks claimed 27.2% to 28.1%.

The pandemic broke every series. Initial claims rose from 11.2 million in 2019 to 71.1 million in 2020. Timeliness fell to 75.4% within 21 days in 2020 and to 67.4% in 2021. Only 9 states met 87% in 2020 and 4 in 2021. Recipiency rose to 68.6% in 2020 and fell back to 29.6% in 2021. Denials per 1,000 initial claims fell sharply in 2020, because initial claims rose far faster than the number of denials issued.

The years after the pandemic did not return to the earlier pattern. Timeliness recovered slowly, to 70.9% within 21 days in 2022, 78.1% in 2023 and 81.3% in 2024, still ten points below 2019. Only 15 to 19 states met 87% at 21 days in 2022–2024, and only 8 at 14 days. Fifty of the 51 jurisdictions had lower mean timeliness in 2022–2024 than in 2016–2019; the median state lost 10.6 percentage points. The largest falls were in Alabama (from 94.9% to 41.6%) and Colorado (from 95.3% to 47.0%). In 2024, state timeliness ranged from 40.1% to 99.3%, and 7 states paid fewer than 70% of first payments within 21 days.

Recipiency returned to its earlier level or below it. Weeks paid equalled 19.6% of unemployed people in 2022, 23.6% in 2023 and 22.6% in 2024. Thirty-seven states had lower mean recipiency in 2022–2024 than in 2016–2019, with a median change of −3.8 percentage points. The number of states paying fewer than 20% of their unemployed residents rose from 19 in 2016 to 33 in 2022 and stood at 26 in 2024. Table 1 summarises the state-level distributions by period.

Table 1. State-level indicators by period: mean (standard deviation) across state-years.

Indicator	2016–2019	2020–2021	2022–2024
First payments within 21 days (%)	92.0 (5.4)	74.3 (12.4)	78.1 (14.6)
First payments within 14 days (%)	84.8 (9.1)	65.7 (13.9)	69.6 (16.1)
Recipiency, weeks paid (% of unemployed)	23.6 (10.5)	48.2 (25.7)	20.1 (9.9)
Recipiency, weeks claimed (% of unemployed)	27.6 (10.6)	57.5 (26.5)	24.4 (10.7)
Initial claims per unemployed person	1.8 (0.7)	4.6 (2.1)	1.9 (0.6)
Separation denials per 1,000 initial claims	156.5 (89.8)	70.5 (55.8)	148.6 (65.7)
Nonseparation denials per 1,000 initial claims	300.9 (178.4)	183.9 (179.0)	422.4 (277.7)
Nonseparation denials per 100 determinations	86.9 (10.1)	88.2 (12.1)	90.4 (8.5)
Appeal decisions for appellant (%)	30.7 (6.9)	28.3 (9.4)	27.2 (9.9)
Households with broadband (%)	83.5 (4.0)	89.6 (2.5)	91.6 (2.0)
Unemployment rate (%)	4.0 (1.0)	6.1 (2.0)	3.5 (0.8)

Denials moved in the opposite direction to recipiency. Nationally, nonseparation denials rose from 236.3 per 1,000 initial claims in 2019 to 340.1 in 2022, and stood at 316.0 in 2024 (Figure 1, panel C). Separation denials returned to almost exactly their earlier level, about 122 to 128 per 1,000 claims. The share of nonseparation determinations that ended in denial also rose, from 85.8% in 2019 to 87.8% in 2022–2024. The rise in denials is therefore concentrated in the rules applied while people are claiming, not in the rules about why they lost their job.

Figure 3 breaks the nonseparation denials down by reason. Two reasons account for the rise. Denials for reporting requirements, such as missing or late certifications and failure to respond to requests for information, rose from 83.7 per 1,000 initial claims in 2019 to 139.5 in 2022 and 132.5 in 2024. Denials for not being able and available or not actively seeking work rose from 79.1 to 113.6 and then 107.5. Denials for disqualifying income stayed near 25 per 1,000 claims and denials for refusing suitable work stayed near 1. Together, reporting and availability denials made up 68% to 70% of nonseparation denials before the pandemic and 74% to 76% in 2022–2024.

Claimants also became less likely to win when they appealed. In

2016, 34.4% of lower-authority appeal decisions favoured the appellant. This share fell to 26.5% in 2022 and recovered only to 29.7% in 2023–2024. The pattern for claimant appellants was similar, falling from 33.8% to 25.8% before rising to 29.1% in 2024. More denials, a lower chance of reversal on appeal and fewer people paid point in the same direction.

4.2 The gap between timeliness and recipiency

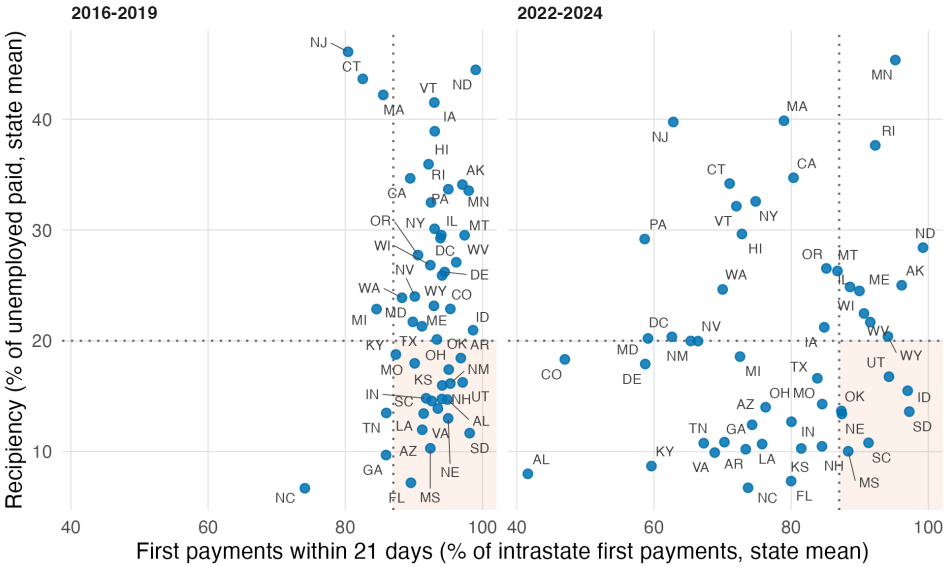


Figure 2 plots each state's mean timeliness against its mean recipiency in 2016–2019 and in 2022–2024. In neither period is there a visible relationship. The Pearson correlation of state means is 0.02 (95% confidence interval −0.26 to 0.29) for 2016–2019 and 0.13 (−0.15 to 0.40) for 2022–2024. Year by year, the correlation ranged from −0.07 to 0.16, and the 95% interval included zero in all nine years. Spearman rank correlations gave the same result.

The scatter shows how wide the gap is. In 2016–2019, 44 states met 87% on the 21-day reading. Their recipiency ranged from 7.2% in Florida to 44.5% in North Dakota, and 18 of the 44 paid fewer than 20% of their unemployed residents. Mississippi, South Dakota, Arizona, Nebraska and Louisiana all paid more than 91% of first payments within 21 days and fewer than 14% of their unemployed. On the stricter 14-day reading, 25 states met 87%, and 11 of these had recipiency below 20%.

The pattern held after the pandemic, with fewer states meeting the standard. In 2022–2024, 16 states met 87% at 21 days. Their recipiency ranged from 10.0% in Mississippi to 45.3% in Minnesota, and 7 of the 16 were below 20%. South Carolina paid 91.3% of first payments within 21 days and reached 10.8% of its unemployed. On the 14-day reading, 7 states met 87%, and 3 of these were below 20% recipiency. Some states with poor timeliness, such as New Jersey and Massachusetts, had among the highest recipiency rates in the country.

Changes within states tell the same story. The correlation between a state's change in timeliness and its change in recipiency from 2016–2019 to 2022–2024 was 0.15 (−0.13 to 0.41). In the panel model with state and year fixed effects and the unemployment rate as a control, one more percentage point of first payments within 21 days was associated with 0.02 percentage points higher recipiency (clustered standard error 0.04). Excluding 2020 and 2021 gave 0.05 (standard error 0.04). Faster payment and wider reach are separate properties of these systems.

Denials, by contrast, are related to recipiency. Across states, mean nonseparation denials per 1,000 initial claims in 2022–2024 correlated with mean recipiency at −0.38 (−0.60 to −0.12). The same correlation for 2016–2019 was −0.20 (−0.45 to 0.08). States whose systems denied more claimants on continuing eligibility grounds paid a smaller share of their unemployed, and this association became stronger after the pandemic. This is a cross-sectional association and may reflect state policy choices that affect both denials and recipiency, but it places the filtering where the timeliness measure does not look.

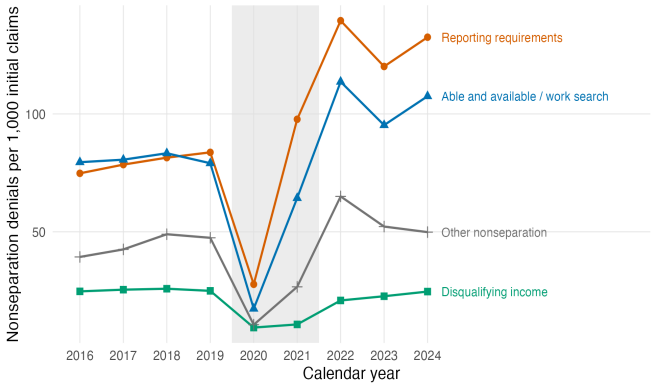


Figure 3. Nonseparation denials by reason per 1,000 state UI initial claims, national, 2016–2024. Reporting requirements cover failures to certify or respond as required; able and available covers availability and work search. Refusal of suitable work (about 1 per 1,000 claims) is included in other nonseparation. The shaded band marks 2020–2021. Source: ETA 207 and 5159 reports.

4.3 Broadband, recipiency and denials

Household broadband spread quickly over the period. The median state's broadband share rose from 80.9% in 2016 to 92.8% in 2024. In 2016, it ranged from 70.7% in Mississippi to 87.4%; by 2024, from 88.3% to 95.3%. Most of the variation is between states and across years shared by all states. After removing state and year effects, the remaining within-state standard deviation of broadband is only 1.07 percentage points, against a total standard deviation of 5.03. This limits what fixed-effects models can detect.

Figure 4 and Table 2 show the results. Between states, the association looks clear: states with higher mean broadband had higher mean recipiency (1.31 percentage points per point of broadband, 95% CI 0.51 to 2.11, $R^2 = 0.14$). But broadband share is tightly linked to state income; the correlation between state mean broadband and state mean log household income is 0.87. When log income and Census region are added, the broadband coefficient turns negative (−2.01, 95% CI −3.89 to −0.12), while income carries the association. With so much collinearity, neither sign can be read as an effect of broadband. The safe reading is that the raw between-state association reflects richer states with more generous systems, not digital access as such.

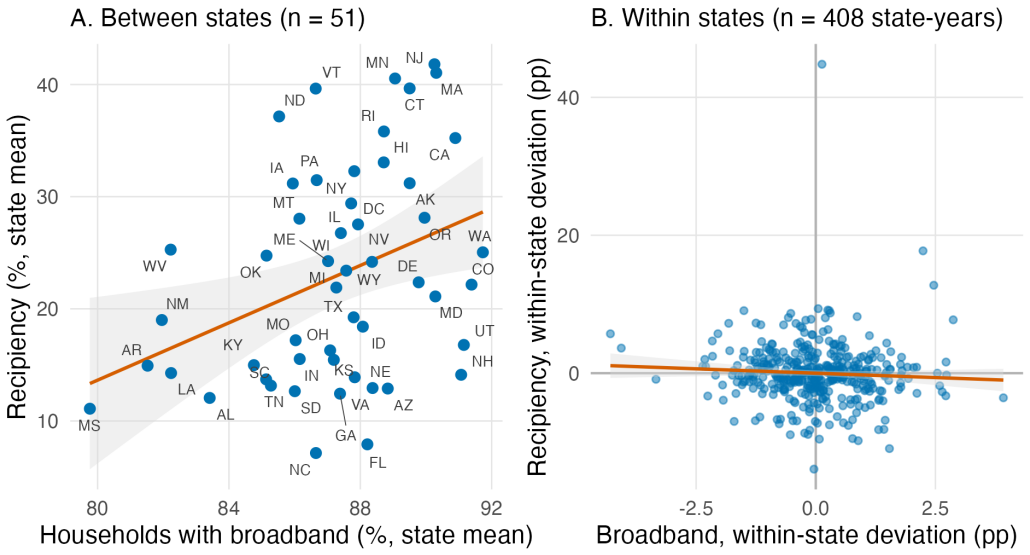


Figure 4. Household broadband share and recipiency. Panel A: state means over the eight years with broadband data (2016–2019, 2021–2024), with a least-squares line and 95% confidence band. Panel B: within-state deviations after removing state effects, year effects and the unemployment rate from both variables, for 408 state-years. The outlier in panel B is Oklahoma in 2021. Sources: ACS 1-year table B28002; ETA 5159; BLS LAUS.

Within states, broadband is not associated with recipiency. With state and year fixed effects and the unemployment rate as a control, one more percentage point of broadband was associated with 0.25 percentage points lower recipiency (95% CI −0.84 to 0.33). The estimate stayed small and its interval included zero when 2021 was dropped, when log income was added and when claimed weeks were used instead of paid weeks. Broadband was also unrelated to nonseparation denials per 1,000 initial claims (10.7, 95% CI −21.6 to 43.1), to the share of determinations ending in denial, and to timeliness. The unemployment rate, by contrast, had a clear within-state association: one point higher unemployment went with 2.5 points lower recipiency (95% CI −4.1 to −0.9), consistent with more people exhausting benefits or entering unemployment without eligibility.

Table 2. Household broadband share and unemployment insurance outcomes: panel and cross-sectional estimates.

Model and outcome	Fixed effects / controls	Broadband, pp (95% CI)	Unemployment rate (95% CI)	N	R^2
1. Recipiency (paid)	State, year	−0.25 (−0.84, 0.33)	−2.50 (−4.14, −0.87)	408	0.06

2. Recipiency (paid), excl. 2021	State, year	−0.35 (−0.98, 0.28)	−2.89 (−4.69, −1.08)	357	0.10
3. Recipiency (paid)	State, year, log income	−0.42 (−0.89, 0.06)	−2.55 (−4.05, −1.04)	408	0.09
4. Recipiency (claimed)	State, year	−0.29 (−0.95, 0.37)	−3.19 (−5.14, −1.24)	408	0.09
5. Nonseparation denials per 1,000 claims	State, year	10.7 (−21.6, 43.1)	−17.0 (−60.1, 26.1)	408	0.01
6. Nonseparation denials per 100 determinations	State, year	−0.02 (−0.67, 0.64)	−0.05 (−2.34, 2.24)	408	0.00
7. First payments within 21 days	State, year	−0.45 (−1.88, 0.98)	−0.40 (−3.45, 2.66)	408	0.00
8. Recipiency (paid), pooled	Year	1.11 (0.44, 1.78)	0.67 (−1.99, 3.33)	408	0.16
9. Recipiency (paid), pooled	Year, region, log income	−1.27 (−2.13, −0.41)	0.68 (−1.43, 2.79)	408	0.48
10. Recipiency (paid), state means	None	1.31 (0.51, 2.11)	1.44 (−2.15, 5.02)	51	0.14
11. Recipiency (paid), state means	Region, log income	−2.01 (−3.89, −0.12)	1.73 (−1.37, 4.82)	51	0.57

The broadband results therefore answer RQ3 in the negative at the level of aggregation available here. Neither the level of household broadband in a state nor its growth over time explains how many unemployed people the state pays or how often its system denies continuing claims. This does not show that digital access is irrelevant for individual claimants. It shows that state-level broadband differences, which narrowed to a few points by 2024, are not where the large differences in recipiency come from.

4.4 Summary of answers to the research questions

Taken together, the findings give clear answers to the three research questions. Timeliness fell sharply in 2020–2021 and had not recovered by 2024, while recipiency returned to its low pre-pandemic level and procedural denials rose (RQ1). Meeting the timeliness standard told almost nothing about how many unemployed people a state paid, whereas denial rates did carry information about recipiency (RQ2). Household broadband, once state income and stable state differences were taken into account, was not associated with recipiency, denials or timeliness (RQ3).

5. Discussion

The study set out to compare the measure by which US UI systems are evaluated with the measure that describes how they are experienced. The two diverge sharply. Timeliness and recipiency are unrelated across states in every year from 2016 to 2024, and unrelated within states over time. In the years before the pandemic, when nearly all states met the timeliness standard, 18 of them paid fewer than one in five unemployed people. A state's record on payment speed says almost nothing about whether its system reaches the people it was built for.

This is not a fault in the arithmetic of the timeliness measure. It is a consequence of what the measure counts. The denominator is first payments, so the measure begins after the system has applied its eligibility rules. Any filtering that happens earlier, in the initial claim, the identity check or the first weekly certification, never reaches it. Bevan and Hood (2006) warned that targets can improve what is measured while leaving the rest of performance unexamined. The UI case adds a structural version of that warning. A measure can be accurate and still look only at the people the system has already let in.

The findings on denials locate where much of the filtering happens. The post-pandemic rise in nonseparation denials came almost entirely from reporting requirements and from availability and work search. These are the rules applied through the weekly online certification: did the claimant certify on time, answer the questions in the accepted way, report the required job contacts and respond to requests for information. In the language of Bovens and Zouridis (2002), this is where the system-level bureaucracy exercises its discretion. The decision that a claimant failed to report is not made by a caseworker weighing a story. It is made when a form is not submitted, a field is answered in a way that triggers an issue, or a deadline passes.

The appeal data add a second piece of evidence. If the extra denials after 2021 had mostly been correct applications of the rules, the share of appeals won by claimants might have stayed stable. Instead it fell from about one in three before the pandemic to about one in four in 2022, and stood near 29% in 2024. The appeal process is the main place where a person can still put a case to a human decision maker. A lower reversal share can mean that first decisions became more accurate, that appeals became harder to win, or that a different mix of cases reached appeal. The reports cannot separate these, but none of them suggests that the rise in procedural denials was absorbed by later human review.

This pattern fits the account of the citizen as an accidental caseworker (Madsen et al., 2022). An online-first UI system asks claimants to do their own case management every week. Missing a step is a compliance cost in the sense of Moynihan et al. (2015), and the system converts it into a denial. One possible reading of the rise in reporting denials after 2021 is that states restored or tightened these checks after the pandemic, for example by reinstating work search rules or adding identity and fraud controls. The data cannot test this reading, but they show the result: more denials on procedural grounds and a lower share of appeals decided for the appellant.

The digital discretion literature has often framed automation as a way to reduce arbitrary human judgement. Wenger and Wilkins (2008) found exactly that in an earlier phase of UI automation. The present results do not contradict them. They suggest a different concern. When decisions move into rules, the rules can be applied consistently and still exclude many people, and the exclusion is harder to see because no one decided any

single case. Young et al. (2019) note that artificial discretion changes who is accountable for outcomes. In UI, the federal accountability scheme looks at payment speed. It does not look at denial rates for procedural failures, and it treats recipiency as context rather than performance.

The broadband results are a useful negative finding. The raw between-state correlation would have supported a simple digital divide story: states with more broadband pay more of their unemployed. That story does not survive scrutiny. Broadband share tracks state income closely, and once income is considered, the association reverses sign and becomes uninterpretable. Within states, growth in broadband did not raise recipiency or reduce denials. This is consistent with Ebenstein and Stange (2010), who found that making UI claims more convenient did not raise take-up. It points away from household connectivity and towards the rules inside the online process as the main barrier.

The within-state association between unemployment and recipiency deserves a brief comment. In years when a state's unemployment rate was one point higher than its usual level, its recipiency was about 2.5 points lower. A rising unemployment rate brings in people with weaker work histories and, in longer downturns, more people who have used up their benefit weeks. Both groups count as unemployed but cannot be paid under state rules. This is a reminder that recipiency reflects eligibility rules as well as access, and that it should be read alongside denial rates rather than as a stand-alone verdict on a system.

That conclusion should be read at its proper scale. Household broadband is a crude proxy for the ability to use an online claims system. A household can have a subscription and still lack a computer, digital skills or a stable address for identity checks (van Dijk, 2006; Helsper & Reisdorf, 2017). The people most affected by digital exclusion are a small share of the population, and their exclusion would barely move a state average. The finding is that state-level broadband does not explain state-level recipiency, not that digital access is irrelevant for the individual claimant.

For the special issue's theme, the case suggests a practical change in how algorithmic public administration is evaluated. If discretion has moved into system rules, then the outputs of those rules need to be measured as carefully as the speed of the process. For UI, that would mean reporting recipiency, denial rates by reason and appeal reversal rates alongside timeliness, and treating sharp rises in procedural denials as a signal that a system may be filtering out eligible people. All of these figures are already collected through the federal reports used here. What is missing is their place in the evaluation.

6. Limitations

The study uses state-level aggregates. It cannot observe individual claimants, so it cannot say which people were denied, whether they were eligible, or whether they reapplied. The reports do not record whether a claim was filed online, by telephone or in person, nor which identity verification system a state used or when it changed. The link between online-first design and procedural denials is therefore an interpretation of the denial categories, not a direct measurement of the online channel.

Recipiency is an imperfect measure of access. Its denominator includes unemployed people who are not eligible, such as new entrants to the labour force and people who quit without good cause, and the eligible share differs across states. Differences in state benefit rules, benefit levels and duration explain much of the variation in recipiency (Anderson & Meyer, 1997; Auray et al., 2019), and the models here do not include them. The timeliness measure is computed on calendar years and on a 21-day window for all states, which differs from the federal evaluation period and overstates compliance in waiting-week states. Broadband varies little within states, so the fixed-effects estimates are imprecise, and the between-state estimates are confounded by income. All results are associations; none should be read as causal effects.

The analysis also leaves out parts of the pandemic response. Federal programmes such as Pandemic Unemployment Assistance paid many people who were not eligible for state UI, and their claims are not in the state UI series used here, so recipiency in 2020–2021 is not comparable with other years. Interstate claims and claims by federal employees and former service members are excluded from the timeliness measure. Finally, the denial categories are those that states report to the Department of Labor; states may classify similar failures in different ways, which adds noise to comparisons between states but matters less for changes within a state over time.

7. Conclusion

US unemployment insurance is evaluated on how fast it pays and experienced through whether it pays at all. Using complete federal administrative data for 2016–2024, this article shows that the two are unrelated across states and within states over time. Many states met the federal timeliness standard while paying fewer than one in five unemployed people. After the pandemic, timeliness stayed below its earlier level and procedural denials for reporting and availability rose by more than a third, while recipiency fell back to the low level of the 2010s. Household broadband share did not explain these differences once state income was considered.

The broader point concerns discretion in online-first public systems. When eligibility is decided through forms and coded rules, the decisive filtering happens in places a speed measure does not reach. Evaluating such systems requires counting who they turn away, and why, as carefully as how fast they pay those they accept. The federal UI reports already contain the numbers needed to do this.

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Ethics statement

This study is a secondary analysis of publicly available, aggregated and de-identified administrative and survey data. No human participants were contacted, and no ethics approval was required.

Data availability statement

All data are public. Unemployment insurance reports ETA 539, ETA 5159, ETA 9050, ETA 207 and ETA 5130 and their data maps: U.S. Department of Labor, Office of Unemployment Insurance, https://oui.doleta.gov/unemploy/DataDownloads.asp (accessed October 2026). State unemployment levels and rates: U.S. Bureau of Labor Statistics, Local Area Unemployment Statistics, public API v1, https://api.bls.gov/publicAPI/v1/timeseries/data/ (series LASST..03 and LASST..04; accessed October 2026). Household broadband (table B28002) and median household income (table B19013): U.S. Census Bureau, American Community Survey 1-year summary files, https://www2.census.gov/programs-surveys/acs/summary_file/ (accessed October 2026). The analysis code will be deposited in a public repository on acceptance; it is withheld during review only to keep the review anonymous.

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