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A Mechanical Accounting of the Family Incidence and Poverty Leverage of U.S. State Minimum-Wage Increases

Qianwen Zhou*

Abstract

How much poverty leverage is embedded in U.S. state minimum-wage increases? I combine a month-specific schedule of regular state and federal minimum-wage floors for 2006–2019 with Current Population Survey Annual Social and Economic Supplement records to trace mechanically predicted wage resources to workers’ official families. The accounting separates population reach, poverty targeting, and gap-closing conversion, and compares the resulting allocation with objective-specific benchmarks that reallocate the same predicted resources within each state–reform-year. In pooled reform-exposure records, 14.1 percent of workers in the primary regular-statewide-floor wage interval are linked to poor families, while 11.6 percent of predicted resources accrue to those families. The predicted allocation attains 1.0 percent of the same-resource benchmark for reducing the official poverty headcount and 6.8 percent of the corresponding benchmark for reducing poverty depth. An inclusive incremental-gap diagnostic yields larger placement ratios, but 88.9 percent of its predicted resource total originates below the Consumer Price Index-aligned no-state-reform floor. The analysis is noncausal: annual work inputs, nonwage resources, and the interview-year official-family roster are held fixed throughout.

Keywords: minimum wages; poverty; family income; policy incidence; mechanical accounting

JEL Classification: J38, I32, J31

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Minimum-wage law regulates pay at the worker–job level. Official poverty, by contrast, pools annual cash resources within an official family and compares that total with a family-specific threshold. The two rules need not select the same people. A near-minimum-wage worker may belong to a family well above poverty, while a poor family may contain no covered wage earner. Even when the two populations overlap, a predicted wage increment may be too small to close the family’s remaining resource gap. This paper measures those links directly for historical U.S. state reforms.

The mismatch is familiar, but its reform-specific components are usually examined separately. A large literature estimates minimum-wage effects on employment, earnings, and the wage distribution (Brown et al. 1982; Card and Krueger 1994; Neumark and Wascher 2008; Dube et al. 2010; Meer and West 2016; Cengiz et al. 2019). Other work studies family income and poverty (Addison and Blackburn 1999; Neumark et al. 2005; Sabia 2008; Dube 2019). Incidence studies show that low-wage workers are spread widely across the family-income distribution (MaCurdy 2015; Congressional Budget Office 2014; Congressional Budget Office 2021). Most closely, Burkhauser et al. (2025) document limited overlap between potentially affected workers and poor families and revisit causal poverty estimates with modern difference-in-differences methods.

This paper asks a different question. Conditional on observed workers, annual work inputs, nonwage resources, and the official-family roster recorded in the linked Current Population Survey Annual Social and Economic Supplement (CPS ASEC), how much direct poverty leverage is embedded in the wage resources predicted by historical statutory paths? I build a mechanical accounting that follows three distinct objects: workers assigned a positive benchmark gain, all people in the official families reached by that gain, and the resource dollars allocated to those families. The poverty-headcount change then factors exactly into population reach, poverty targeting, and gap-closing conversion. Objective-specific same-resource benchmarks show the strongest headcount or poverty-depth change obtainable by repositioning the same predicted budget within each state–reform–year.

The analysis combines a hand-coded monthly schedule of regular state and federal minimum-wage floors for 2006–2019 with CPS ASEC survey years 2007–2019. Each reform–

year path is linked to resources, wages, weeks, and usual hours from the prior income year and to the official-family roster observed at the ASEC interview. The monthly schedule preserves the month-specific path of the regular statewide floor, including multiple steps within a year. It does not encode local ordinances or separate tipped, youth, training, small-employer, occupation-specific, and enterprise-coverage rules. The primary wage interval is therefore deliberately named the *regular-statewide-floor interval*; it is not a claim about the legal floor applicable to every worker–job.

The first result concerns incidence. In pooled reform-exposure records, 14.1 percent of workers in the primary interval are linked to official families classified as poor. Only 11.6 percent of the predicted resources assigned within that interval accrue to poor families. These are different denominators: the first statistic weights workers, the second weights predicted resources, and the targeting component of the accounting weights all people in reached families. Keeping the three objects separate prevents a worker-incidence statistic from being read as family targeting.

The second result concerns poverty leverage. Under the primary interval, the predicted allocation attains roughly 1.0 percent of the same-resource headcount benchmark and 6.8 percent of the censored Foster–Greer–Thorbecke (FGT1) poverty-depth benchmark. The larger poverty-depth placement ratio means that the predicted resources align more closely with narrowing normalized shortfalls among poor families than with moving families across the official line. It does not imply a realized treatment effect, a welfare ranking, or inefficiency relative to a feasible transfer program.

An inclusive incremental-gap diagnostic assigns positive resources wherever the state-policy path raises the benchmark wage gap, including annual implied wages below the regular statewide floor. It produces larger headcount and FGT1 placement ratios, but 88.9 percent of its predicted resource total originates below the CPI-aligned no-state-reform floor. Such observations can reflect legal exclusions, tipped work, multiple jobs, incomplete compliance, within-year wage changes, or measurement in annual implied hourly wages. The inclusive measure is therefore reported as a diagnostic rather than as a second headline estimate of newly mandated resources. The data extract does not include allocation flags for annual wage

income, weeks, and usual hours, which further supports this ordering of the two definitions.

The accounting also separates resource scale from placement. It reports the pooled predicted budget in 2019 dollars, mean predicted resources per positive-gain worker and per reached official family, the budget relative to the aggregate official poverty gap, and the share of baseline-poor person mass that the same resource total could move across the line under perfect within-cell placement. These quantities clarify whether limited mechanical leverage reflects a small budget, its location in the family-income distribution, or both.

The calculations are noncausal. The observed reform-year effective path is compared with a no-state-reform path that freezes the prior-December state floor while retaining the observed federal schedule. Both paths are converted to prior-income-year dollars with the same Consumer Price Index for All Urban Consumers (CPI-U) ratio. Employment, weeks, hours, nonwage resources, and the observed interview-year official-family roster remain fixed. Because ASEC does not report work months, the benchmark spreads annual work inputs uniformly across the reform year; a sensitivity carries forward baseline annual weeks and assigns them to the lowest and highest monthly policy gaps subject to the annual work total. Additional checks exclude potentially tipped occupations and workers ages 16–19 and restrict attention to reforms implemented after the ASEC interview.

The same-resource benchmarks are intentionally demanding accounting comparisons. They use perfect family information, continuously divisible survey-weighted family copies, and no financing or administrative costs; they do not move resources across states or reform years. I use them to measure mechanical alignment with two explicit poverty objectives, not to propose an alternative policy or to measure total welfare. CPS ASEC replicate calculations are likewise presented as constructed cross-sectional replicate-sensitivity intervals conditional on the pooled exposure construction. For nonlinear placement benchmarks, leave-one-state-out ranges summarize dependence on reform-state composition without being labeled confidence intervals.

The contribution is thus not another causal estimate of the poverty effect of minimum wages. It is an auditable map from historical statutory paths to worker incidence, family reach, predicted resource placement, and two poverty objectives. The framework complements

causal evidence by identifying where direct pass-through can be mechanically limited before behavioral, price, tax, transfer, and family-formation responses are introduced. The Appendix explains why the available annual reform design is not used for causal inference and reports measurement, normalization, respondent, and policy-source diagnostics.

The remainder of the paper proceeds as follows. Section 1 relates the accounting to the policy and empirical literature. Section 2 describes the monthly schedule, ASEC linkage, and sample. Section 3 defines predicted resources, incidence, identities, and same-resource benchmarks. Section 4 reports the primary accounting. Section 5 presents scope, coverage, timing, and measurement checks. Section 6 concludes.

1 Policy Context and Relation to Existing Evidence

1.1 The statutory instrument and the family objective

U.S. minimum-wage law sets an hourly floor at the worker–job level. The binding rate in state s and month m is the larger of the state and federal rates. Official poverty instead compares an official family’s annual cash resources with a threshold that varies by family size and composition (U.S. Census Bureau 2025). A benchmark wage increment reaches a poverty objective only through a sequence: a reform must reach a family, reached persons must be classified as poor in the linked ASEC record, and the family’s pooled benchmark gain must be sufficient to close the remaining poverty gap.

This mismatch has long shaped policy analysis. The Congressional Budget Office separates workers whose pay may rise from the family-income distribution of those gains and from behavioral and price responses (Congressional Budget Office 2014; Congressional Budget Office 2019; Congressional Budget Office 2021). MaCurdy (2015) similarly emphasizes that low hourly wages are observed throughout the family-income distribution. Earlier simulation evidence likewise linked minimum-wage increases to family poverty outcomes: Mincy (1990) used CPS household data to model how coverage, compliance, and disemployment assumptions affect the poverty gap and the number of poor families. These studies make clear that a wage floor is not targeted by family income in the way a means-tested transfer is. The present

analysis asks a narrower historical question: given the workers and official families in the linked ASEC records, where would benchmark wage resources be placed under fixed work inputs and the observed family roster?

1.2 Employment, earnings, and distributional evidence

The labor-demand consequences of minimum wages remain an active empirical question. Early surveys emphasized potentially negative employment effects (Brown et al. 1982); case-study, border, and distributional approaches subsequently generated a wide range of estimates (Card and Krueger 1994; Dube et al. 2010; Meer and West 2016; Clemens and Wither 2019; Cengiz et al. 2019). Other work studies wage inequality and the shape of the earnings distribution (Lee 1999; Autor et al. 2016). Those margins matter for realized household resources, but they are not estimated in the mechanical benchmark used here.

Family-income and poverty studies move from worker outcomes to household or family outcomes. Estimates differ across samples, controls, counterfactuals, and estimators (Addison and Blackburn 1999; Neumark et al. 2005; Sabia 2008; Dube 2019). Burkhauser et al. (2025) provide the closest recent comparison. They revisit the poverty evidence with nearly four decades of March CPS data and several dynamic difference-in-differences estimators. They report both weak worker–poverty overlap and estimates close to zero. Their paper evaluates causal poverty responses and the sensitivity of those estimates to research design. This paper instead treats the weak overlap as the first component of an accounting identity and follows predicted wage resources through person targeting, gap-closing conversion, and objective-specific same-resource placement benchmarks.

The distinction matters for interpretation. A causal reduced form combines statutory wage changes with compliance, wage spillovers, employment and schedule adjustment, prices, taxes, transfers, and family behavior. Mechanical incidence conditions on prior-calendar-year income and work inputs and on the ASEC interview-year family roster. It cannot recover the realized effect, but it can show the poverty leverage of benchmark wage-resource placement under those fixed inputs. Conversely, a small or imprecise causal estimate cannot by itself reveal whether the constraint was reach, targeting, conversion, or offsetting behavior.

1.3 State and federal exposure

The main analysis studies the family incidence of state-driven increases in the effective minimum wage. This scope is reflected in the title and estimand; it is not justified as a source of quasi-experimental policy innovation. The no-state-reform counterfactual freezes the state floor but retains the observed federal schedule, preventing a binding federal increase in the same year from being attributed to the state policy. Appendix Table 7 audits the overlap. Federal-floor-only exposures provide a separate scope comparison. They disproportionately affect states without higher state floors and may reach families with different poverty profiles. Appendix Table 6 therefore reports federal-floor-only and all-effective-increase accounting alongside the state results.

The policy schedule spans 2006–2019; effective-rate reform years in the accounting are 2007–2019 and link to ASEC baseline income years 2006–2018. This fixed pre-pandemic window is chosen because it supplies a common set of harmonized official-resource variables, annual work inputs, and fully source-coded monthly statutory paths. It is a data-comparability boundary rather than an identifying restriction. Appendix Table 18 reports 2007–2014 and 2010–2019 reform-period sensitivities, while longer-run causal studies remain essential for other periods (Dube 2019; Burkhauser et al. 2025).

2 Data and Sample Construction

2.1 Monthly statutory schedule

The replication files contain a 50-state monthly schedule from January 2006 through December 2019. Annual state levels are initialized from U.S. Department of Labor tables (U.S. Department of Labor, Wage and Hour Division 2026a). Exact effective dates are then coded from state statutes, regulations, labor-department pages, and wage posters; the federal event sheet uses the Department of Labor’s historical federal schedule (U.S. Department of Labor, Wage and Hour Division 2026b). The workbook retains a primary source field, an optional secondary source, and coding notes for each dated event. Appendix Table 17 records the

coding hierarchy.

The binding regular statewide rate is

$$M_{smy} = \max\{M_{smy}^{\text{state}}, M_{my}^{\text{federal}}\}. \quad (1)$$

The schedule does not encode local ordinances or separate tipped, youth, training, small-employer, occupation-specific, and enterprise-coverage rules. It therefore describes a regular statewide benchmark rather than the legally applicable floor for a particular worker–job. Omitting subminimum schedules can make the benchmark exceed some workers’ applicable floors; omitting local ordinances can make it fall below others. The linked ASEC extract does not identify county or city, so workers cannot be assigned to local ordinances. The net direction of the resulting incidence difference cannot be signed without worker-level legal-coverage and location data. An effective-rate increase is state driven when a state change raises the binding regular statewide rate above the federal floor. A federal-floor-only exposure year has a federal increase and no state-driven increase. The file contains 224 state-driven and 76 federal-floor-driven monthly changes. Multiple changes in one state and year are retained as one monthly path.

The schedule spans 2006–2019, while linked effective-rate reforms occur in 2007–2019. Each reform year r uses income year $r - 1$ as its baseline. Let S_{sr0} be the state floor in December of year $r - 1$, and let F_{rm} be the federal floor in reform-year month m . The preferred no-state-reform path freezes the state component but retains the observed federal path:

$$\begin{aligned} M_{srm}^0 &= \max\{S_{sr0}, F_{rm}\}, \\ M_{srm}^b &= M_{srm} \frac{P_{r-1}}{P_r}, \\ M_{srm}^{0,b} &= M_{srm}^0 \frac{P_{r-1}}{P_r}. \end{aligned} \quad (2)$$

Here P_t is annual CPI-U (U.S. Bureau of Labor Statistics 2026). Applying the same price conversion to both paths makes their difference a state-policy increment rather than a change in the real value of the statutory floor. The paths coincide in months when the observed state floor remains at its prior-December level, including months in which a federal increase alone

changes the effective floor. Of the 212 linked state-driven cells, 61 occur in years with a federal statutory change and three contain a federal change that raises the binding effective floor. Appendix Table 7 reports this overlap. Appendix Table 18 reports the full effective-rate path relative to a fixed prior-December effective floor, the positive real-floor-change convention, the duration-weighted average effective floor over the baseline year, and an exclusion of cells whose effective floor changed during the baseline year.

2.2 ASEC workers and official families

The CPS ASEC supplies annual wage income, weeks worked, usual weekly hours, official-family cash resources, official poverty thresholds, family structure, full-sample weights, and 160 replicate weights. The linked records use ASEC survey years 2007–2019, which describe baseline income years 2006–2018. Variable harmonization follows Census, U.S. Bureau of Labor Statistics (BLS), and IPUMS CPS documentation (U.S. Bureau of Labor Statistics 2024; U.S. Census Bureau 2019; Flood et al. 2025). In the 2014 ASEC, the 5/8 legacy-income and 3/8 redesigned-income samples are each weighted to the full population. The preferred sample retains the redesigned 3/8 file, as recommended for comparisons with 2015 and later income measures, rather than adding two independently weighted populations (IPUMS CPS 2026a). The official income-to-poverty ratio is

$$IPR_{fy} = \frac{Y_{fy}}{Z_{fy}}, \quad (3)$$

where Y_{fy} is official family cash income and Z_{fy} is the official threshold. A person is officially poor when the ratio assigned through that person’s official family is below one.

For wage-and-salary workers ages 16–64 with positive wage income, weeks, and usual weekly hours, prior-income-year hourly pay is annual wage income divided by annual hours. Hourly wages outside \$0.50–\$200 are excluded. This annual implied wage can differ from a contemporaneous job wage because of multiple jobs, within-year wage changes, changes in hours, reporting error, and legal exclusions. Because the same annual-hours measure forms the wage denominator and scales predicted resources, Appendix tables examine hours

restrictions, baseline-work timing bounds, hours groups, and gain percentiles directly. The analytic extract used here does not include allocation flags for the annual wage-income, weeks, and hours inputs. The analysis therefore does not condition on allocation status. This limitation is one reason the inclusive incremental-gap measure is not treated as a headline estimate. Because the same reported inputs determine annual implied wages, allocation error can also affect membership in the primary interval; the primary estimates are accounting results conditional on the reported ASEC work and earnings inputs.

FAMID identifies official families and pools worker gains once within each family. ASECWT aggregates person reach, poverty headcounts, and censored FGT1 contributions. ASECFWT, which IPUMS derives from the official family reference person’s second-stage weight, weights family resource totals and placement-benchmark costs (IPUMS CPS 2026b). Appendix Table 8 compares the preferred family point weight with reference-person ASECWT, mean member ASECWT, and ASECWTH alternatives. Person-level replicate contributions use REPWTP1–REPWTP160.

IPUMS does not supply a replicate series corresponding directly to ASECFWT. The preferred constructed family-resource replicate therefore scales ASECFWT by the family reference person’s $\text{REPWTP}_b/\text{ASECWT}$ factor. Family person mass is always the sum of member REPWTP in replicate b , matching the person aggregation used for the full-sample allocation. Appendix Table 21 compares this family-reference factor with an alternative that scales ASECFWT by $\text{REPWT}_b/\text{ASECWTH}$ and reports a sign audit over all 160 replicates. Both constructions produce some nonpositive family person masses in the nonlinear placement sample; the family-reference construction also produces some nonpositive resource weights and gap costs. No replicate weight is truncated or reset. The resulting intervals for smooth quantities are described as constructed cross-sectional replicate-sensitivity intervals, not as official ASECFWT design-based confidence intervals. Nonlinear placement statistics instead receive leave-one-state-out composition ranges. Income, wages, hours, and weeks refer to the calendar year preceding the reform. Official-family composition and the associated poverty threshold come from the ASEC interview-year roster. That roster predates later-year reforms but can postdate January–March implementation; the accounting holds the observed roster

fixed and reports a sensitivity that requires the interview to precede the first state reform.

2.3 Monthly CPS wage diagnostics

The monthly basic CPS does not determine the scale of ASEC benchmark resources. It provides a separate check on whether the large component originating below the CPI-aligned no-state-reform floor is solely an artifact of annual implied wages. Appendix Table 19 aligns ages 16–64, current employment, and wage-and-salary status across sources. It reports one comparison for the broader worker sample, using weekly earnings divided by usual weekly hours in the monthly CPS, and another for hourly-paid workers, using reported hourly pay. EARNWEEK, HOURWAGE, and PAIDHOUR calculations use EARNWT, as required for outgoing-rotation earnings measures (IPUMS CPS 2026c). Hourly-paid status for ASEC records is taken from the last available nonmissing monthly observation in the baseline income year. The table reports annual and monthly sample sizes, the ASEC-to-monthly link rate, and hourly-status availability. It also places a worker-weighted ASEC gap calculation beside the official family-resource-weighted calculation so that a difference in weighting is not mistaken for a difference in wage measurement. Both monthly measures use the three months before the first state-driven reform step. Because the monthly calculation does not use annual hours, it diagnoses wage measurement rather than supplies an alternative resource estimand.

2.4 Linked exposure sample

The state analysis links each reform year to the preceding ASEC income year and consolidates within-year statutory steps into a state–income-year exposure cell. Table 1 reports the resulting linked person, official-family, and worker records. Appendix Table 9 traces the flow from 300 dated policy changes to the primary regular-statewide-floor-interval sample and the inclusive incremental-gap diagnostic. The analysis counts pooled reform-exposure records, not unique U.S. residents: a person represented in a state with reforms in multiple years can contribute to multiple reform-specific records.

3 Mechanical Accounting Framework

3.1 Month-specific predicted wage resources

Let w_{ifr} be worker i 's baseline hourly wage in official family f linked to reform year r , expressed in baseline income-year dollars. Let $H_{ifr} = h_{ifr}q_{ifr}$ be annual hours, where h is usual weekly hours and q is weeks worked. Let M_{srm}^b denote the effective minimum wage in month m of reform year r , converted to baseline dollars. The preferred counterfactual $M_{srm}^{0,b}$ freezes the prior-December state floor while retaining the observed reform-year federal path and applies the same CPI conversion as the observed path.

The dynamic mechanical wage path is

$$w_{ifrm}^{\text{mech}} = \max\{w_{ifr}, M_{srm}^b\}. \quad (4)$$

The incremental-gap gain under the observed path is

$$g_{ifr}^I = \frac{H_{ifr}}{12} \sum_{m=1}^{12} \left[\max\{w_{ifr}, M_{srm}^b\} - \max\{w_{ifr}, M_{srm}^{0,b}\} \right]. \quad (5)$$

Because both paths retain the same federal schedule and use the same CPI conversion, the bracketed term isolates the state component of the effective-rate path and is nonnegative. A binding federal increase in the same calendar year therefore enters both paths rather than being assigned to the state reform. Because ASEC reports annual rather than monthly work inputs, equation (5) allocates each worker's prior-income-year annual hours uniformly across the twelve months of the reform year. The monthly statutory paths are observed; the monthly work calendar is not. A sensitivity therefore carries prior-income-year weeks into the reform year, caps placement at 52/12 weeks per month, and allocates them first to the smallest or largest monthly policy gaps. These baseline-work timing bounds preserve annual weeks and usual weekly hours while relaxing uniform timing; they are not bounds on observed reform-year work calendars. Additional checks use year-round workers and hours restrictions.

Define the final observed and no-state-reform floors as $\overline{M}_{sr}^b = \max_m M_{srm}^b$ and $\overline{M}_{sr}^{0,b} =$

$\max_m M_{srm}^{0,b}$. The regular-statewide-floor interval indicator is

$$A_{ifr}^S = \mathbf{1}\{\overline{M}_{sr}^{0,b} \leq w_{ifr} < \overline{M}_{sr}^b\}, \quad (6)$$

and the regular-statewide-floor interval gain is

$$g_{ifr}^S = A_{ifr}^S g_{ifr}^I. \quad (7)$$

The interval is defined relative to the CPI-aligned reform-year no-state-reform floor. It is a wage interval around the regular statewide benchmark in prior-income-year dollars, not a worker-specific legal-coverage classification. This definition applies the month-specific state-policy increment to workers in that interval. The inclusive g^I diagnostic also assigns resources to wages reported below the CPI-aligned no-state floor. Neither object allows employment, hours, weeks, or nonwage income to change.

Predicted family resources under definition $j \in \{S, I\}$ are

$$G_{fr}^j = \sum_{i \in f} g_{ifr}^j, \quad (8)$$

and the mechanically updated ratio is

$$IPR_{fr}^{j, \text{mech}} = \frac{Y_{fy} + G_{fr}^j}{Z_{fy}}. \quad (9)$$

The phrase *mechanically produces* always refers to this fixed-work, fixed-family benchmark. It does not describe a realized treatment effect.

3.2 Actual reform sizes and fixed-support normalization

Observed reform paths are the main estimand. For comparison with quantities expressed per 10-log-point statutory change, define the average monthly log gap between the observed and no-state-reform paths:

$$I_r = \frac{1}{12} \sum_{m=1}^{12} [\log M_{srm} - \log M_{srm}^0]. \quad (10)$$

This definition uses the same monthly policy counterfactual as equation (5). It therefore accommodates multiple state steps and any later federal change that narrows the state-policy gap. The fixed-support intensity-normalized gain is

$$g_{ifr}^{j,FS} \equiv g_{ifr}^j \frac{0.10}{I_r}. \quad (11)$$

The left-hand side, $g_{ifr}^{j,FS}$, denotes the fixed-support normalized gain and is distinct from the observed-size gain g_{ifr}^j on the right-hand side. This operation preserves the workers reached by the observed reform and changes only the gain intensity. It is not a counterfactual that imposes a common 10-log-point increase in the statutory floor itself. Appendix Tables 12 and 13 report the normalization-factor distribution and separate actual-size results from this scale comparison.

3.3 Pooled reach, targeting, and conversion

Let p_{ifr} be the ASEC person weight for person i in family f and reform exposure r . The pooled valid person-exposure mass is

$$P = \sum_r \sum_f \sum_{i \in f} p_{ifr} \mathbf{1}\{IPR_{fy} \text{ valid}\}. \quad (12)$$

Because people in states with repeated reforms can enter more than once, P is not a count of unique U.S. residents. It is pooled person-weighted mass across reform-specific exposure records.

For gain definition j , pooled reach, targeting, and conversion are

$$\mathcal{R}^j = 100 \frac{\sum_{rfi} p_{ifr} \mathbf{1}\{G_{fr}^j > 0\}}{P}, \quad (13)$$

$$\mathcal{T}^j = 100 \frac{\sum_{rfi} p_{ifr} \mathbf{1}\{G_{fr}^j > 0, IPR_{fy} < 1\}}{\sum_{rfi} p_{ifr} \mathbf{1}\{G_{fr}^j > 0\}}, \quad (14)$$

$$\mathcal{C}^j = 100 \frac{\sum_{rfi} p_{ifr} \mathbf{1}\{IPR_{fy} < 1 \leq IPR_{fr}^{j,mech}\}}{\sum_{rfi} p_{ifr} \mathbf{1}\{G_{fr}^j > 0, IPR_{fy} < 1\}}. \quad (15)$$

The pooled mechanical headcount change in percentage points satisfies the exact identity

$$\Delta H^{j,\text{mech}} = \frac{\mathcal{R}^j \mathcal{T}^j \mathcal{C}^j}{10,000}. \quad (16)$$

The pooled estimand weights larger states and states with repeated reforms more heavily. Appendix Table 5 reports equal-reform and equal-state placement ratios.

3.4 Worker and resource incidence

Let $\omega_{ifr} = p_{ifr}$ denote the ASECWT attached to a worker record. For gain definition $j \in \{S, I\}$, worker incidence below the official poverty line is

$$WI^j = 100 \frac{\sum_{rfi} \omega_{ifr} \mathbf{1}\{g_{ifr}^j > 0\} \mathbf{1}\{IPR_{fy} < 1\}}{\sum_{rfi} \omega_{ifr} \mathbf{1}\{g_{ifr}^j > 0\}}, \quad j \in \{S, I\}. \quad (17)$$

In the analytic sample, $\mathbf{1}\{g_{ifr}^S > 0\} = A_{ifr}^S$. Table 1 reports the primary $j = S$ worker-incidence measure; the inclusive $j = I$ diagnostic appears in Appendix Table 22. Worker incidence describes workers, not all people in their families. For gain definition j , resource incidence is

$$GI^j = 100 \frac{\sum_{rf} a_{fr} G_{fr}^j \mathbf{1}\{IPR_{fy} < 1\}}{\sum_{rf} a_{fr} G_{fr}^j}. \quad (18)$$

where a_{fr} is ASECFWT, the official family weight. Figure 1 and Table 1 use equation (17) for workers and equation (18) for benchmark resources. Person targeting in equation (14) remains a third, distinct estimand.

3.5 Official poverty depth

The censored FGT1 contribution for a person in family f is

$$Q_{fy} = (1 - \max\{IPR_{fy}, 0\})_+. \quad (19)$$

For gain definition j , the mechanically updated contribution is

$$Q_{fr}^{j,\text{mech}} = \left(1 - \max\{IPR_{fr}^{j,\text{mech}}, 0\}\right)_+, \quad (20)$$

and the pooled mechanical reduction in the censored FGT1 index is

$$\Delta P_{\text{MW}}^{j,G} = \frac{100}{P} \sum_r \sum_f \sum_{i \in f} p_{ifr} (Q_{fy} - Q_{fr}^{j,\text{mech}}). \quad (21)$$

Here $(x)_+ = \max\{x, 0\}$. Equation (21) is the mechanically implied reduction in the censored FGT1 index, expressed in percentage points. The index averages family-level contributions with person weights. The zero-income cap prevents negative reported cash resources from producing normalized gaps above one. A censored FGT1 objective gives proportional credit for reductions in normalized shortfalls even when a family does not cross the poverty line (Sen 1976; Foster et al. 1984). It does not place convex marginal weight on deeper poverty; that feature belongs to higher-order FGT measures.

3.6 Objective-specific same-resource placement benchmarks

For each state s and reform year r , define the benchmark resource budget under gain definition j as

$$B_{sr}^j = \sum_{f \in (s,r)} a_{fr} G_{fr}^j. \quad (22)$$

The predicted allocation and its same-resource placement benchmark use exactly the same B_{sr}^j ; resources never move across states or reform years. Let $N_{fr} = \sum_{i \in f} p_{ifr}$ be the family's person mass and $D_{fr} = (Z_{fy} - Y_{fy})_+$ its official cash gap. The maintained resource-cost weight a_{fr} is official ASECFWT. Appendix Table 14 reports reference-person, mean-member, and household-weight alternatives. Person outcomes continue to use p_{ifr} . The headcount benchmark fills poor-family gaps in ascending order of

$$\kappa_{fr}^H = \frac{a_{fr} D_{fr}}{N_{fr}}. \quad (23)$$

This order minimizes the resource cost per weighted person crossing. The final survey-weighted family copy can be fractional.

Among families with $Q_{fy} > 0$, a full family-gap allocation costs $a_{fr}D_{fr}$ and removes $N_{fr}Q_{fy}$ person-weighted normalized-gap units. The censored-FGT1 benchmark orders continuously divisible survey copies by

$$\kappa_{fr}^G = \frac{a_{fr}D_{fr}}{N_{fr}Q_{fy}} \quad (24)$$

and allocates until the same reform-specific budget is exhausted. Under the survey-copy relaxation, the final partial copy contributes proportional cost and proportional FGT1 benefit. Resources never move across states or reform years.

For objective $o \in \{H, G\}$, the placement ratio is

$$\rho^{j,o} = 100 \frac{\Delta P_{\text{MW}}^{j,o}}{\Delta P_{\text{benchmark}}^{j,o}}. \quad (25)$$

It compares the predicted minimum-wage allocation only with its own objective-specific same-resource placement benchmark. This benchmark is not a realistic transfer policy: it assumes perfect family information, omits financing and administration, and does not measure total welfare. The ratio is a measure of mechanical alignment with the stated objective, not realized policy efficiency.

3.7 Constructed cross-sectional replicate-sensitivity intervals and reform-state composition

For smooth incidence and mechanical-allocation quantities, the analysis applies the CPS ASEC successive-difference replicate columns to construct cross-sectional replicate-sensitivity intervals (IPUMS CPS 2026d). Let $\hat{\theta}$ be one such full-sample statistic and $\hat{\theta}_b$ its value under replicate $b \in \{1, \dots, 160\}$. The variance calculation is

$$\hat{V}(\hat{\theta}) = \frac{4}{160} \sum_{b=1}^{160} (\hat{\theta}_b - \hat{\theta})^2. \quad (26)$$

The calculation uses the same replicate index b across annual ASEC files and the mean-squared-error convention. Reported brackets are $\hat{\theta} \pm t_{0.975,159} \sqrt{\widehat{V}(\hat{\theta})}$. The multiplier provides a conventional scale for the replicate perturbations; the resulting brackets are sensitivity intervals and are not interpreted as 95-percent confidence intervals. The calculation captures within-year cross-sectional sampling perturbations conditional on the pooled exposure construction; it does not separately model cross-year covariance or repeated respondents. For smooth person and family accounting quantities, the replicate family person mass is

$$N_{fr}^{(b)} = \sum_{i \in f} \text{REPWTP}_{ifr}^{(b)}.$$

Because no official family replicate series corresponds directly to ASECFWT, the preferred constructed family resource weight is

$$a_{fr}^{(b)} = \text{ASECFWT}_{fr} \frac{\text{REPWTP}_{f,\text{ref}}^{(b)}}{\text{ASECWT}_{f,\text{ref}}}.$$

Appendix Table 21 compares this construction with a household-factor alternative based on REPWT and ASECWTH. It also audits nonpositive weights, costs, and person masses. Smooth quantities receive constructed cross-sectional replicate-sensitivity intervals. The nonlinear placement problem is different: some replicates contain nonpositive family person masses, and the family-reference construction also contains some nonpositive gap costs. Those inputs invalidate the nonnegative cost–benefit ordering required by the benchmark. The paper does not truncate supplied replicate weights and does not report replicate intervals for placement benchmarks, placement ratios, or their differences. The constructed intervals for smooth quantities carry no formal coverage interpretation and are not an official replicate design for ASECFWT.

The replicate weights describe cross-sectional ASEC sampling variation. The pooled accounting can include the same CPSIDP in adjacent ASEC years, so its intervals do not incorporate longitudinal dependence created by repeated respondents. Appendix Table 16 first restricts the pooled sample to valid CPSIDP records without deduplication, then reports

earliest- and latest-record sensitivities. This bridge separates identifier availability from the additional effect of retaining one record per respondent. The exercises change the pooled estimand and do not replace the main accounting.

For nonlinear quantities, a leave-one-state-out exercise deletes each of the 34 complete reform-state histories in turn and recomputes both objective-specific benchmarks, both placement ratios, and their difference. The reported minimum–maximum range describes sensitivity to reform-state composition. It is not a confidence interval, does not treat states as a random sample, and supplies no causal uncertainty. Appendix Table 20 reports the two sensitivity exercises side by side.

4 Incidence and Mechanical Poverty Leverage

4.1 Workers, families, and predicted resources

Figure 1 locates the workers and predicted resources covered by the primary regular-statewide-floor interval across the official-family income-to-poverty-ratio distribution. The comparison uses prior-income-year earnings and work inputs together with the official-family roster observed at the ASEC interview. It is therefore an accounting of linked records, not a longitudinal family transition or an estimate of a policy effect.

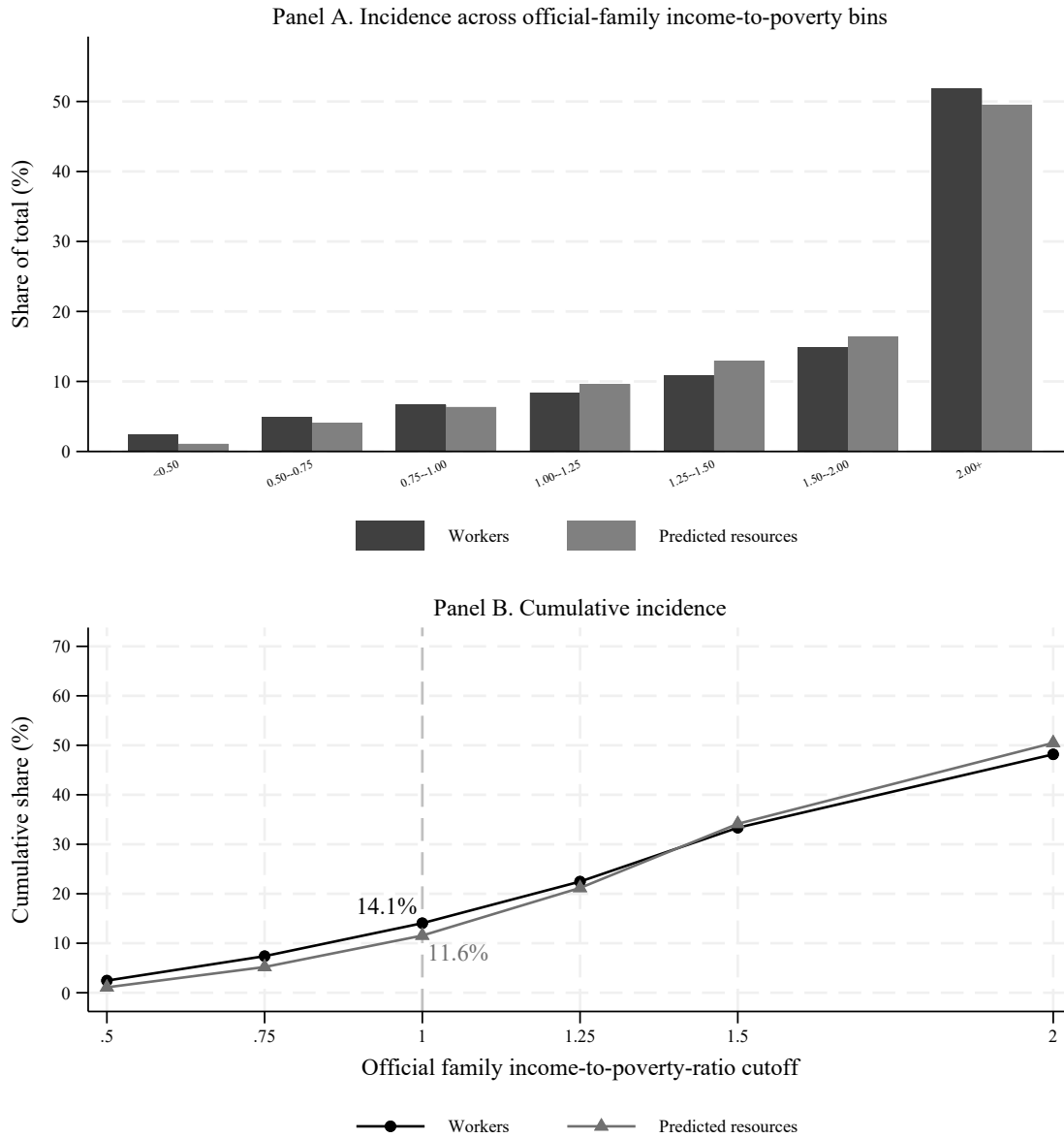


Fig. 1 Worker and predicted-resource incidence across linked official families

Notes: Panel A reports shares across official-family income-to-poverty-ratio bins; Panel B cumulates those shares through each cutoff. The vertical line marks the official poverty threshold. Workers have annual implied wages between the CPI-aligned no-state-reform floor and the maximum regular statewide floor observed in the reform year. The schedule does not encode local, tipped, youth, training, small-employer, occupation-specific, or enterprise-coverage rules. Worker incidence uses ASECWT; predicted-resource incidence uses ASECFWT.

Only 14.1 percent of workers in this interval are linked to families classified as poor, and 11.6 percent of the predicted resources are assigned to those families. The two percentages use different denominators: the first weights workers, whereas the second weights predicted wage resources. The person-based targeting component introduced below uses a third denominator, all persons in families receiving a positive predicted resource. Distinguishing workers reached, persons reached, and resources allocated prevents similar-looking incidence statistics from being treated as interchangeable.

Table 1 reports the policy counts, linked samples, incidence measures, and baseline poverty quantities. The intervals in the table are constructed cross-sectional replicate-sensitivity intervals. They apply the same CPS ASEC replicate index across annual files and condition on the pooled reform-exposure construction; they do not incorporate covariance from respondents observed in more than one ASEC year.

The low overlap is consistent with evidence that low-wage workers are distributed widely across family incomes (MaCurdy 2015; Congressional Budget Office 2014; Congressional Budget Office 2021; Burkhauser et al. 2025). The contribution here is to carry reform-specific worker incidence through family pooling, poverty-gap conversion, and two explicit same-resource placement benchmarks.

4.2 Reach, targeting, and conversion

Panel A of Table 2 decomposes the mechanically implied headcount change into reach, targeting, and conversion. Reach is the share of valid person-exposure mass in families receiving positive predicted resources. Targeting is the share of reached persons initially below the official line. Conversion is the share of those initially poor persons whose fixed-resource counterfactual crosses the line. Using unrounded sufficient statistics, their product divided by 10,000 equals the headcount reduction in percentage points.

Under the primary interval, predicted resources reach about 2.0 percent of person-exposure mass. About 14.6 percent of reached persons are initially poor, and about 2.8 percent of that poor mass crosses the line under the benchmark. The mechanically implied headcount change is 0.008 percentage point. These quantities hold employment, annual work inputs,

Table 1. Policy events, accounting samples, and direct incidence

Item	Estimate	Unit or constructed replicate-sensitivity interval
<i>Panel A. Policy and linked accounting samples</i>		
State-driven effective-rate increases	224	Reform events
Federal-floor-driven effective-rate increases	76	Reform events
Linked state reform exposure cells	212	State-income-year cells
States represented in linked state reform cells	34	States
Linked person-exposure records	815,154	Records
Linked official-family-exposure records	353,116	Records
Regular-statewide-floor-interval worker records	5,415	Records
<i>Panel B. Incidence of workers and actual-size predicted resources</i>		
Regular-statewide-floor-interval workers linked to poor families	14.056	[12.832, 15.280] percent
Regular-statewide-floor resources assigned to poor families	11.558	[10.030, 13.085] percent
<i>Panel C. Baseline poverty and benchmark mechanical changes</i>		
Baseline official poverty rate	12.774	[12.574, 12.975] percent
Baseline censored FGT1 poverty-gap index	6.317	[6.209, 6.424] pp
Regular-statewide-floor mechanical headcount reduction	0.008	[0.005, 0.011] pp

Notes: Family poverty combines prior-income-year official resources with the interview-year official-family roster and threshold. The primary gain definition covers annual implied wages between the CPI-aligned no-state-reform floor and the maximum regular statewide floor observed in the reform year. The schedule does not encode local, tipped, youth, training, small-employer, occupation-specific, or enterprise-coverage rules. Worker and person outcomes use ASECWT; family resources use ASECFWT. Worker incidence is worker based, Table 2 targeting is person based, and gain incidence is resource weighted. Brackets are constructed cross-sectional replicate-sensitivity intervals for smooth quantities, conditional on the pooled reform-exposure construction. They carry no formal coverage interpretation, apply the same replicate index across annual files, and do not account for repeated respondents across years. Appendix Table 21 documents the construction and its limits.

nonwage resources, and the observed official-family roster fixed.

Table 2. Primary mechanical identity and objective-specific same-resource benchmarks

Primary gain definition	Reach (%) [replicate-sensitivity]	Targeting (%) [replicate-sensitivity]	Conversion (%) [replicate-sensitivity]	Headcount reduction (pp) [replicate-sensitivity]
<i>Panel A. Actual-size reach–targeting–conversion identity</i>				
Regular-statewide-floor interval	1.960 [1.893, 2.026]	14.638 [13.257, 16.019]	2.807 [1.627, 3.986]	0.008 [0.005, 0.011]
<i>Panel B. Actual-size allocation relative to objective-specific same-resource benchmarks</i>				
Objective	Mechanical allocation [replicate-sensitivity]	Same-resource benchmark [leave-one-state-out range]	Placement ratio (%) [leave-one-state-out range]	
Official poverty headcount	0.008 [0.005, 0.011]	0.814 [0.638, 0.890]	0.989 [0.924, 1.056]	
Censored FGT1 poverty depth	0.005 [0.004, 0.005]	0.070 [0.051, 0.079]	6.792 [6.630, 6.971]	
<i>Panel C. Difference in placement ratios (percentage points)</i>				
Regular-statewide-floor interval	Censored FGT1 ratio minus headcount ratio			5.803 [5.598, 5.977]

Notes: The table reports the primary regular-statewide-floor-interval definition. In Panel A, unrounded reach times targeting times conversion, divided by 10,000, equals the headcount reduction in percentage points. Brackets are constructed cross-sectional replicate-sensitivity intervals for smooth person-weighted quantities, conditional on the pooled exposure construction; they do not carry a formal coverage interpretation and do not account for repeated respondents across ASEC years. Panel B reallocates the same ASECFWT-weighted family budget only within each state–reform-year. Benchmark and placement-ratio brackets are minimum–maximum ranges from deleting one complete reform-state history at a time and are not confidence intervals. The objective-specific same-resource benchmarks assume perfect family information, omit financing and administration, do not measure total welfare, and are not feasible policies. The inclusive incremental-gap diagnostic is reported in the Appendix.

Figure 2 shows the reform-specific dispersion behind each pooled component. Reforms are ranked independently in each panel, so an identical rank need not refer to the same state–year. The pooled horizontal line is person weighted and is not the unweighted mean of the reform-specific points.

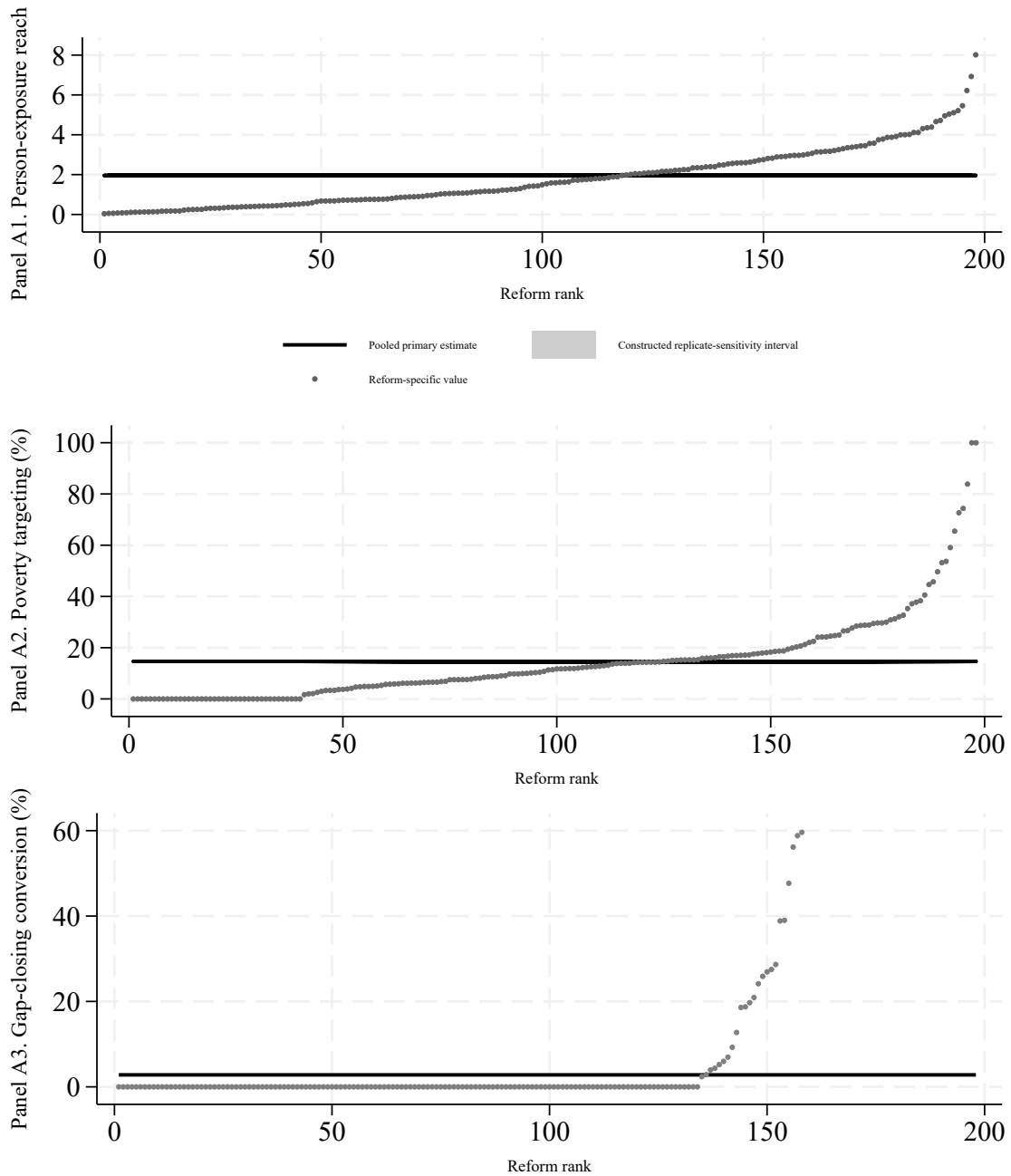


Fig. 2 Reform-specific components of benchmark mechanical poverty leverage

Notes: The panels rank linked state reform cells separately by person-exposure reach, poverty targeting, and gap-closing conversion under the primary regular-statewide-floor interval. Horizontal lines are pooled person-weighted estimates. Shaded bands are constructed cross-sectional replicate-sensitivity intervals conditional on the pooled exposure construction. Ranks are not matched across panels, and the displayed quantities are not causal effects.

The dispersion clarifies why no single component determines poverty leverage. A reform can reach a comparatively large population while assigning few predicted resources to poor families. Conversely, it can reach poor families whose remaining official poverty gaps are too large for the predicted resources to produce threshold crossings. The identity organizes these margins without assuming that they are independently manipulable policy parameters.

4.3 Same-resource placement benchmarks

Panel B of Table 2 compares the predicted allocation with two objective-specific benchmarks that reassign the same family-weighted resource total within each state–reform–year. The headcount benchmark maximizes person-weighted crossings of the official line. The censored-FGT1 benchmark instead maximizes reductions in normalized poverty depth. Under the primary definition, the predicted allocation attains about 1.0 percent of the headcount benchmark and 6.8 percent of the poverty-depth benchmark. The difference is positive throughout the leave-one-state-out calculations reported in Panel C.

Figure 3 displays the predicted allocation and its corresponding same-resource benchmark for each objective. The larger poverty-depth placement ratio means that predicted resources align more closely with narrowing shortfalls among poor families than with placing families immediately across the official line. It does not establish a realized treatment effect, rank welfare programs, or show inefficiency relative to a feasible transfer policy.

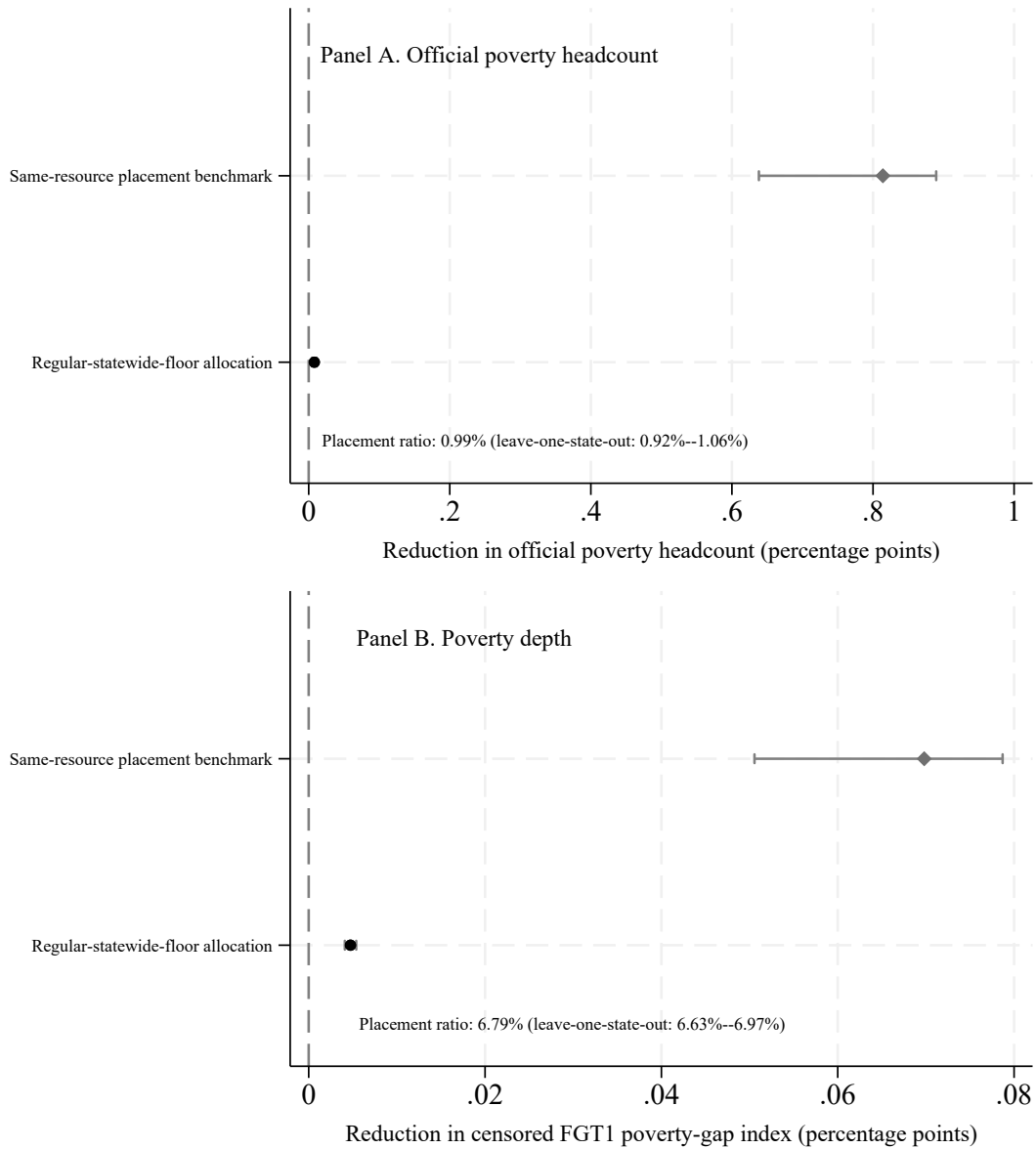


Fig. 3 Primary allocations and objective-specific same-resource placement benchmarks

Notes: Both panels use actual-size predicted resources under the primary regular-statewide-floor interval. Intervals around the mechanical allocations are constructed cross-sectional replicate-sensitivity intervals for smooth person-weighted quantities. Benchmark and placement-ratio ranges delete one complete reform-state history at a time; they are composition diagnostics, not confidence intervals. Each benchmark holds the resource total fixed within a state-reform-year and uses perfect family information and continuously divisible survey-weighted family copies. It omits financing and administration, does not measure total welfare, and is not a policy proposal. Headcount and censored FGT1 benchmarks optimize different objectives, and panel scales differ.

4.4 Resource scale and placement

Table 3 converts predicted resources to 2019 dollars and separates resource scale from placement. It reports the pooled budget, average amounts per positive-gain worker and reached official family, and the budget relative to the aggregate official poverty gap. The table then contrasts the share of baseline-poor person mass that could cross the line under perfect within-cell placement with the much smaller share crossing under the predicted allocation. Thus, limited headcount leverage can reflect both the amount of resources generated by the benchmark and where those resources appear in the official-family distribution.

Table 3. Predicted resource scale and mechanical poverty leverage

Measure	Primary estimate	Unit
Predicted resource budget	4,804.4	Million 2019 dollars across pooled exposures
Mean predicted resource per positive-gain worker	481	2019 dollars
Mean predicted resource per reached official family	498	2019 dollars
Predicted budget relative to aggregate official poverty gap	0.548	Percent
Headcount benchmark relative to baseline-poor person mass	6.373	Percent
Mechanical headcount change relative to baseline-poor person mass	0.063	Percent
Headcount placement ratio	0.989	Percent
Censored-FGT1 placement ratio	6.792	Percent

Notes: The table reports the primary regular-statewide-floor interval. Dollar quantities convert each income-year amount to 2019 dollars before pooling reform-exposure records. The resource budget uses ASECFWT; the mean worker amount uses ASECFWT; the reached-family mean divides the ASECFWT-weighted family resource total by reached official-family mass. The aggregate poverty gap is the ASECFWT-weighted sum of positive official-family resource shortfalls in the same pooled exposures. The headcount same-resource benchmark measures what share of baseline-poor person mass could cross the line under perfect within-cell placement; the placement ratio compares the predicted allocation with that benchmark. The inclusive incremental-gap diagnostic appears in Appendix Table 22.

Across the pooled reform-exposure records, the ASECFWT-weighted resource budget is about \$4.8 billion in 2019 dollars. Separately, the worker-weighted mean predicted gain is \$481 per positive-gain worker, while the family-weighted mean is \$498 per reached official family. The pooled budget equals 0.548 percent of the aggregate official poverty gap represented in

the linked sample. Within state–reform–year cells, the same resource total could place 6.373 percent of baseline-poor person mass across the line under the headcount benchmark; the predicted allocation places 0.063 percent across it. These are pooled exposure-accounting quantities rather than an annual national program budget.

The broader incremental-gap construction is deliberately not used as a headline result. It assigns predicted resources whenever the state-policy path narrows the gap to the regular statewide floor, including observations with annual implied wages below the CPI-aligned no-state-reform floor. Such observations can reflect incomplete compliance, tipped or otherwise exempt work, multiple jobs, within-year wage changes, or reporting and imputation error in annual earnings and work inputs. Appendix Table 22 reports that diagnostic, its wage-region origin, and its resource scale. Because most of its resources originate below the regular statewide floor, it is interpreted as an inclusive diagnostic rather than as newly mandated legal resources.

5 Sensitivity, Measurement, and Interpretation

5.1 Policy path and legal coverage

The preferred counterfactual freezes the prior-December state floor while retaining the observed federal schedule. The observed and no-state paths receive the same CPI conversion. Appendix Table 7 shows that 61 of 212 linked state-reform cells occur in years with a federal statutory change, but only three contain a federal change that raises the binding effective floor. Those cells account for less than 0.3 percent of the inclusive incremental-gap diagnostic budget and less than 0.2 percent of its mechanically implied crossing mass. Appendix Table 6 separately reports federal-floor-only and all-effective-rate scopes.

The primary interval is defined by the regular statewide floor, not by each worker’s complete legal minimum. The policy file does not encode local floors, tip credits, youth and training provisions, small-employer exemptions, occupation-specific rules, or enterprise coverage. A reported wage below the statewide floor can therefore reflect lawful exclusions or subminimum provisions as well as noncompliance or measurement error; a statewide floor can

also understate the applicable floor in a locality with a higher minimum. The net direction of coverage error is not identified. Appendix Table 18 excludes potentially tipped occupations, workers ages 16–19, and both groups together. These restrictions reduce obvious sources of mismatch but cannot recover worker-specific legal coverage.

The analytic extract used here does not include allocation flags for annual wage income, weeks, and usual weekly hours. The analysis therefore cannot condition on allocation status. This is one reason that the below-floor incremental-gap construction remains a supplemental diagnostic. Appendix Table 22 reports its concentration by annual implied-wage region rather than treating it as a second preferred specification.

5.2 Annual work timing and family-roster timing

Equation (5) allocates reported annual weeks uniformly across the twelve reform-year months. ASEC does not reveal when those weeks occurred. Appendix Table 18 therefore adds lower and upper baseline-work timing bounds: prior-income-year weeks, subject to a maximum of 52/12 per month, are carried into the reform year and assigned first to months with the smallest or largest policy gaps. The uniform benchmark lies between these extreme timing scenarios conditional on baseline annual work inputs. The same table reports year-round-worker and hours-cap restrictions. These calculations address sensitivity to the assumed timing of baseline work; they do not bound or reconstruct observed reform-year employment histories.

The bounds are empirically tight for the primary construction. Across lower, uniform, and upper placements, the mechanically implied headcount reduction rounds to 0.008 percentage point, the headcount placement ratio ranges from 0.99 to 1.00 percent, and the censored-FGT1 placement ratio ranges from 6.75 to 6.84 percent. The principal placement comparison therefore does not depend on assuming that annual weeks are literally uniform across months.

Earnings, weeks, and hours refer to the prior income year, whereas official-family membership and the poverty threshold come from the interview-year roster. For reforms implemented after the ASEC interview, the roster predates implementation; for early-year reforms it may postdate implementation. The accounting fixes the observed roster. Appendix Table 18

reports a restriction requiring the interview month to precede the first state reform and separate implementation-month samples. The 2014 benchmark uses the redesigned-income 3/8 panel; the same table substitutes the traditional 5/8 panel and excludes 2014 in separate checks.

5.3 Wage measurement

Annual implied wages divide wage income by reported annual weeks and usual weekly hours. Errors in any input can move a record across the regular-statewide-floor interval. Appendix Table 19 compares this measure with EARNWT-weighted monthly CPS weekly earnings and reported hourly wages in aligned worker samples. Wages and comparison floors are expressed in common price units within source, so the diagnostic concerns relative floor position rather than nominal levels across sources. The annual and monthly measures differ materially, and the linked hourly-paid sample is selective. The exercise documents the limitation; it does not validate the level of annual predicted resources because monthly records do not supply the same annual work total.

5.4 Weights and constructed replicate sensitivity

Person headcounts and censored FGT1 contributions use ASECWT; family resource costs use ASECFWT. Appendix Table 14 substitutes several family point weights. Appendix Tables 20 and 21 document the replicate construction and signed-input audit. Smooth statistics use ASECFWT scaled by the official-family reference person's REPWTP-to-ASECWT factor and person mass equal to the sum of member REPWTP. The same replicate index is applied across annual files. These calculations are reported as constructed cross-sectional replicate-sensitivity intervals because they neither use an official ASECFWT replicate series nor model cross-year covariance and repeated respondents.

Signed replicate inputs are not truncated. Some are incompatible with the nonnegative sorting problem that defines a same-resource placement benchmark, so no replicate-based interval is reported for nonlinear benchmarks or placement ratios. Their reported ranges

instead delete one complete reform-state history at a time. The leave-one-state-out exercise describes composition sensitivity and is not a confidence interval. Appendix Table 16 also compares the full pooled sample, all valid-CPSIDP records, and earliest- and latest-record samples. It changes the pooled estimand and does not substitute for longitudinal variance estimation.

5.5 Aggregation, normalization, and causal boundary

Appendix Table 5 compares pooled, equal-reform, and equal-state aggregation. Appendix Tables 12 and 13 normalize predicted resources to a common ten-log-point monthly observed-versus-no-state policy gap while holding worker support fixed. This is a scale comparison rather than a coherent statutory counterfactual, because a different floor would also change worker support and targeting.

The annual reform design is not used for causal inference. Appendix C documents large pre-trends and sparse clean post-reform support. Modern staggered-adoption methods address contamination from already-treated comparisons, but they cannot manufacture post-reform support or remove pre-existing trends (Goodman-Bacon 2021; Sun and Abraham 2021; Callaway and Sant’Anna 2021; de Chaisemartin and D’Haultfoeuille 2020). The reported accounting therefore does not establish realized earnings gains or causal poverty effects. It states what the coded regular statewide paths imply when employment, annual work inputs, nonwage resources, and the observed official-family roster are held fixed.

6 Conclusion

Minimum-wage law attaches to worker-jobs, while official poverty attaches to families. This paper follows predicted resources from historical state wage-floor paths through that institutional mismatch. The accounting uses prior-income-year wages, resources, weeks, and usual hours together with the official-family roster observed at the ASEC interview. Employment, annual work inputs, nonwage resources, and the observed roster remain fixed, so none of the reported changes is a causal treatment effect.

The primary result is modest but interpretable. In pooled reform-exposure records, 14.1 percent of workers in the regular-statewide-floor interval are linked to families classified as poor, and 11.6 percent of predicted resources within that interval are assigned to such families. The allocation reaches only about 1.0 percent of its objective-specific same-resource headcount benchmark but about 6.8 percent of its censored-FGT1 poverty-depth benchmark. Predicted wage resources therefore align more closely with narrowing some normalized shortfalls than with moving official families across the threshold.

The inclusive incremental-gap diagnostic generates larger mechanical poverty changes and placement ratios, but most of its resource total originates among annual implied wages below the CPI-aligned no-state-reform floor. Because those observations can reflect legal exclusions or subminimum rules, tipped work, multiple jobs, incomplete compliance, and measurement in annual wage income, weeks, or hours, the broader measure is not interpreted as newly mandated resources. Coverage restrictions, monthly CPS wage comparisons, baseline-work timing bounds, and interview-timing checks make this uncertainty visible without pretending to identify each worker’s applicable legal floor.

The same-resource comparisons are demanding alignment benchmarks, not feasible transfer programs. They assume perfect family information, divisible survey-weighted family copies, and no administrative or financing costs. Their value is diagnostic: they distinguish the scale of predicted resources from where those resources are placed relative to the poverty objective. The resource-budget accounting makes both margins observable rather than reducing a small poverty change to a single targeting statistic.

The policy conclusion is correspondingly bounded. Historical state minimum-wage paths place only limited direct mechanical leverage on the official poverty headcount under the primary wage interval, while showing somewhat greater alignment with poverty depth. Realized antipoverty effects can still differ through employment, hours, wage spillovers, tax and transfer interactions, prices, and family responses. Estimating those channels requires a credible causal design; the present contribution is to make the direct statutory-resource accounting transparent before those channels are layered on.

Statements and Declarations

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Competing Interests

The author has no relevant financial or non-financial interests to disclose.

Data Availability

The CPS microdata used in this study are available from IPUMS CPS subject to registration and its terms of use. The underlying IPUMS microdata cannot be redistributed by the author. The coded monthly regular-statewide-floor schedule, source documentation, and other nonconfidential materials necessary to reproduce the analysis from an authorized IPUMS extract will be made publicly available in a replication repository upon acceptance.

Code Availability

The data-management and analysis code used to produce the results reported in this article will be made publicly available in a replication repository upon acceptance.

Author Contributions

The author was responsible for the conceptualization, methodology, data curation, formal analysis, software, visualization, and writing of the manuscript.

A Estimand and Same-Resource Benchmark Audits

A.1 Exact pooled identities

Appendix Table 4 reports the unrounded components behind the actual-size headcount identities. For the primary regular-statewide-floor interval,

$$\frac{1.959811 \times 14.637925 \times 2.806670}{10,000} = 0.008052.$$

The second row applies the same identity to the inclusive incremental-gap diagnostic. Values displayed in the main table are rounded after the sufficient statistics are aggregated.

Table 4. Six-decimal audit of the actual-size mechanical identities

Gain definition	Reach (%)	Targeting (%)	Conversion (%)	Product (pp)
Regular-statewide-floor interval	1.959811	14.637925	2.806670	0.008052
Inclusive incremental-gap diagnostic	12.706942	21.232197	4.616404	0.124549

Notes: The product column equals reach times targeting times conversion divided by 10,000 using unrounded pooled sufficient statistics. All quantities use actual reform sizes, monthly statutory paths, and prior-income-year dollars.

A.2 Alternative aggregation across reforms and states

Appendix Table 5 distinguishes three descriptive estimands. Pooled accounting sums weighted person, resource, and same-resource benchmark quantities over all linked state–reform–year records. Equal-reform accounting averages cell-specific ratios. Equal-state accounting first pools cells within state and then averages state ratios. The similarity of the censored FGT1 ratios across the three rules shows that the objective-specific pattern is not an artifact of a few large or frequently reforming states.

Table 5. Placement ratios under alternative aggregation rules

Gain definition	Headcount placement ratio (%)			Censored FGT1 placement ratio (%)		
	Pooled	Equal reform	Equal state	Pooled	Equal reform	Equal state
Regular-statewide-floor interval	0.989	0.627	1.029	6.792	7.701	7.502
Inclusive incremental-gap diagnostic	4.156	3.276	3.734	12.846	13.011	13.586

Notes: Pooled ratios aggregate survey-weighted sufficient statistics across linked reform-exposure records, so larger states and states with repeated reforms receive more weight. Equal-reform and equal-state columns average reform-specific and state-specific ratios, respectively. Same-resource benchmarks remain internal to each state-reform-year.

B Construction, Scope, and Sample Audits

B.1 State and federal exposure scopes

Appendix Table 6 adds federal-floor-only cells and their union with state-driven cells. The comparisons are descriptive because the exposure groups are not exchangeable.

Table 6. State, federal-floor, and all effective-rate exposure accounting

Exposure scope	Gain definition	Cells	Reach (%)	Headcount reduction (pp)	Headcount ratio (%)	FGT1 reduction (pp)	FGT1 ratio (%)
State-driven	Regular-statewide-floor interval	212	1.960	0.008	0.989	0.005	6.792
State-driven	Inclusive incremental-gap diagnostic	212	12.707	0.125	4.156	0.071	12.846
Federal-floor-only	Regular-statewide-floor interval	69	2.640	0.011	1.043	0.007	11.498
Federal-floor-only	Inclusive incremental-gap diagnostic	69	11.047	0.093	3.314	0.067	17.839
All effective increases	Regular-statewide-floor interval	281	2.214	0.010	1.123	0.006	8.009
All effective increases	Inclusive incremental-gap diagnostic	281	12.389	0.124	4.095	0.073	13.648

Notes: Federal-floor-only cells contain a federal effective-rate increase and no state-driven increase in the same reform year. The all-effective-increase rows use the full effective-rate path in state-driven years and the federal-floor-only path in the remaining cells; they are not obtained by pooling the preferred state-policy gains reported in the first two rows. These are mechanical accounting comparisons, not causal estimates.

Appendix Table 7 distinguishes any same-year federal change from one that raises the binding effective floor and reports the mixed cells’ budget and crossing-mass shares.

Table 7. Federal-floor overlap within linked state-driven reform cells

Audit quantity	Value	Unit
Linked state-driven reform cells	212	Cells
Cells with a federal statutory-floor change in the same year	61	Cells
Cells with a binding federal-floor increase in the same year	3	Cells
Binding mixed-cell share of inclusive incremental-gap diagnostic budget	0.249	Percent
Binding mixed-cell share of mechanical crossing mass	0.137	Percent

Notes: A federal statutory-floor change records any increase in the federal minimum wage during the calendar year. A binding federal-floor increase is one that raises the state’s effective minimum wage after holding the contemporaneous state floor fixed. The preferred state-policy counterfactual freezes the prior-December state floor while retaining the observed reform-year federal path, so federal increments are not assigned to the state reform. Budget and crossing-mass shares refer to the three state-driven cells that also contain a binding federal increase and use inclusive incremental-gap diagnostic resources under the preferred state-policy counterfactual.

B.2 Price alignment, path construction, and placement rules

Appendix Table 8 summarizes price alignment, monthly path construction, and the within-cell same-resource placement rules.

B.3 Sample flow

Appendix Table 9 traces 300 dated changes through annual consolidation, ASEC linkage, and the two worker-gain samples.

B.4 Supplemental measurement and sampling diagnostics

Appendix E reports coverage restrictions, baseline-work timing bounds, interview timing, EARNWT-weighted monthly wage comparisons, constructed cross-sectional replicate-sensitivity intervals, nonlinear-input audits, the wage-region origin of the inclusive diagnostic, normalization, alternative weights, repeated-respondent checks, and policy sources.

Table 8. Same-resource benchmark interpretation and maintained accounting rules

Element	Rule
Policy counterfactual and price alignment	The observed effective path is compared with a no-state-reform path that freezes the prior-December state floor while retaining the observed reform-year federal path. Both paths use the same annual CPI-U conversion to baseline-income-year dollars.
Within-year path	The monthly policy increment is $\max\{w_{i0}, MW_{sm}^b\} - \max\{w_{i0}, MW_{sm}^{0,b}\}$; because ASEC reports annual work inputs, baseline annual hours are allocated uniformly across the twelve reform-year months.
Headcount benchmark	Within each state-reform-year, family gaps weighted by official ASECFWT are filled in ascending resource cost per person crossing.
Censored FGT1 benchmark	Within each state-reform-year, continuously divisible survey-weighted family copies are ranked by resource cost per unit of person-weighted censored normalized-gap reduction.
Interpretation	The objective-specific same-resource placement benchmark assumes perfect family information, excludes financing and administration, does not measure total welfare, and is not a policy proposal or a measure of realized efficiency.
Constructed cross-sectional replicate-sensitivity intervals	Smooth incidence and allocation quantities use the sum of member REPWTP1-REPWTP160 as family person mass. The preferred family-resource factor scales ASECFWT by the family reference person's REPWTP-to-ASECWT ratio. Nonlinear replicate intervals are withheld because some inputs are nonpositive.
Reform-state composition	Leave-one-state-out calculations delete each complete reform-state history in turn and report the resulting minimum-maximum range; the range is not sampling inference.

Notes: The headcount and censored FGT1 same-resource benchmarks optimize different objectives. Their levels are not directly comparable; each placement ratio compares the predicted mechanical allocation only with its own objective-specific benchmark.

Table 9. Accounting sample flow from policy changes to linked exposures

Sample-flow stage	Count	Unit
All effective minimum-wage changes in the monthly schedule	300	Reform events
State-driven changes	224	Reform events
Federal-floor-driven changes	76	Reform events
Linked state reform cells after within-year consolidation	212	State-income-year cells
Linked person-exposure records	815154	Records
Linked official-family-exposure records	353116	Records
Regular-statewide-floor-interval worker records	5415	Records
Positive incremental-gap worker records	39770	Records

Notes: Linked state reform cells require a pre-reform ASEC income year, a valid monthly statutory path, and valid official family resources. Federal-floor-only cells are analyzed separately in Appendix Table 6. Multiple changes in one state and reform year are consolidated into a single monthly-path exposure cell.

C Why Annual Event Studies Are Not the Identifying Design

This paper does not use annual poverty event studies as causal evidence. The reason is empirical rather than stylistic. State reforms are frequent enough that requiring clean pre- and post-reform windows removes most long-run support. Of 224 state-driven reforms, 48 have 36 clean pre-reform months, and only five are isolated within a plus-or-minus 24-month window. Once annual ASEC outcomes are aligned to income years and later reforms censor post-treatment observations, only 17 clean episodes enter the stacked designs. Two remain in the first complete post-reform income year and none remain in a second complete year.

Appendix Table 10 reports clean and matched-clean stacked diagnostics. In both designs, event-study leads jointly reject parallel pre-trends. The average post coefficients are therefore not interpreted as treatment effects. Matching on pre-reform outcomes does not resolve the rejection or the support problem.

Table 10. Why annual official-poverty event studies are not used for identification

Design	Average post (pp)	SE	Joint lead p-value	Episodes	First full year	Second full year
Clean stack	-0.482	0.291	0.000	17	2	0
Matched clean stack	-0.495	0.372	0.000	17	2	0
Policy-support diagnostic						Count
State-driven reforms						224
Reforms with 36 clean pre-months						48
Reforms isolated plus/minus 24 months						5

Notes: The outcome is the official poverty rate, and estimates are percentage-point responses per 10-log-point statutory change. The lead coefficients jointly reject parallel pre-trends in both clean and matched stacks. Of the 17 clean reform episodes, only the reported number remain for the first complete post-reform income year and none remain for a second complete year after censoring at later state reforms. The support diagnostics are reported here rather than in Table 1 because annual event studies are not treated as identifying evidence.

Modern work on staggered adoption shows why a conventional two-way fixed-effects event study would not repair these limitations (Goodman-Bacon 2021; Sun and Abraham 2021). Heterogeneity-robust estimators address contaminated comparisons across treatment cohorts (Callaway and Sant’Anna 2021; de Chaisemartin and D’Haultfoeuille 2020), but they cannot create untreated support or remove economically large pre-trends. Exposure designs based on

affected fractions or statutory gaps can introduce additional measurement and functional-form problems (Haanwinckel 2026). The appropriate response here is to narrow the estimand: the main analysis reports mechanical incidence, while Appendix C documents the compromised annual causal design.

D Auxiliary Panel Associations and Precision

Appendix Table 11 reports state and income-year fixed-effects associations per 10-log-point duration-weighted statutory change. State-clustered standard errors imply relatively tight confidence intervals for official poverty and FGT measures. Under a normal approximation, the 80-percent minimum detectable effect for the official poverty rate is 0.465 percentage points.

These precision calculations address a limited question: what annual coefficients would the available state panel be capable of distinguishing from zero under the reported auxiliary specification? They do not overcome the pre-trend and post-support failures documented in Appendix C. The coefficients are therefore not used as causal validation of the mechanical accounting. In particular, the family-resource association cannot establish that predicted wage gains were realized, and the poverty association is not interpreted as the observed counterpart of either the primary 0.008-point accounting change or the 0.125-point inclusive diagnostic.

Table 11. Auxiliary noncausal panel associations and precision

Outcome	Estimate	SE	95% CI low	95% CI high	80% MDE
Family wage earnings (thousand USD)	0.491	0.391	-0.276	1.258	1.096
Official family cash resources (thousand USD)	0.238	0.438	-0.620	1.097	1.227
Official poverty rate (pp)	-0.077	0.166	-0.403	0.248	0.465
FGT poverty gap (pp)	0.010	0.076	-0.138	0.159	0.212
FGT squared gap (pp)	0.023	0.057	-0.089	0.135	0.160

Notes: Coefficients are state and income-year fixed-effects associations per 10-log-point duration-weighted statutory change with state-clustered standard errors. They are not interpreted causally because event-study leads reject parallel pre-trends and clean post-reform support is sparse. The 80-percent minimum detectable effect is 2.802 times the standard error under a normal approximation and a two-sided 5-percent test. The table does not claim to rule out causal effects.

E Additional Accounting and Measurement Diagnostics

E.1 Reform-level heterogeneity in the inclusive diagnostic

Figure 4 relates reform-specific poverty targeting to the mechanically implied headcount change under the inclusive incremental-gap diagnostic. The relationship is descriptive. Targeting alone cannot determine a reform’s accounting result because reach, predicted resource amounts, and gap-closing conversion also vary.

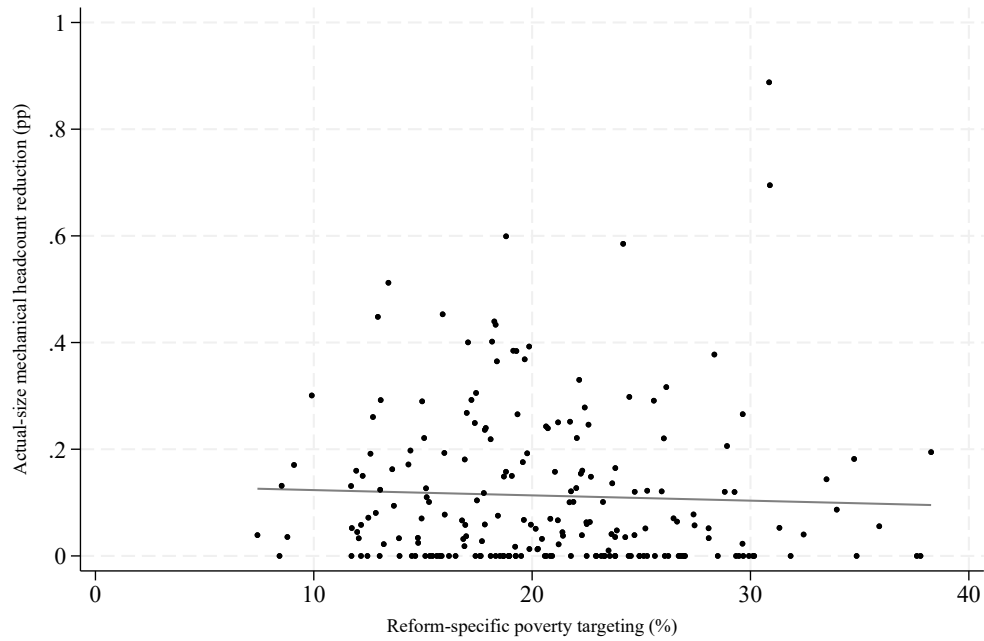


Fig. 4 Reform-specific targeting and mechanical headcount change in the inclusive diagnostic

Notes: Each point is a linked state–reform–year with a positive incremental-gap budget. The fitted line is descriptive. The vertical-axis quantity is calculated under fixed annual work inputs, nonwage resources, and the observed official-family roster and is not a causal poverty effect. The incremental-gap construction can assign predicted resources below the regular statewide floor and is reported as an inclusive diagnostic.

E.2 Fixed-support normalization

Table 12 describes the factor used to map the observed gain vector to a 10-log-point statutory change while keeping worker support fixed. Its wide distribution is why normalized resources are not presented as a coherent common statutory reform.

Table 12. Fixed-support intensity-normalization factors

Mean	10th percentile	25th percentile	Median	75th percentile	90th percentile
4.699	0.924	1.549	2.982	5.868	9.416

Notes: For reform cell r , the factor is $0.10/I_r$, where $I_r = 12^{-1} \sum_m [\log M_{sr m} - \log M_{sr m}^0]$ is the average monthly log gap between the observed effective path and the no-state-reform path. Multiplying actual-size gains by this factor preserves observed worker support; it is an intensity normalization, not a newly constructed 10-log-point statutory counterfactual.

Table 13 reports actual-size and fixed-support quantities together for both the primary interval and the inclusive diagnostic. The normalization rescales an observed-support gain vector; it is not a statutory counterfactual that imposes a common 10-log-point statutory change.

Table 13. Actual-size accounting and fixed-support intensity normalization

Gain definition	Headcount allocation	Headcount benchmark	Placement ratio (%)	FGT1 allocation	FGT1 benchmark	Placement ratio (%)
Regular-statewide-floor interval, actual size	0.008	0.814	0.989	0.005	0.070	6.792
Inclusive incremental-gap diagnostic, actual size	0.125	2.997	4.156	0.071	0.550	12.846
Regular-statewide-floor interval, fixed support ($g^{S,FS}$)	0.014	1.166	1.225	0.007	0.103	6.700
Inclusive incremental-gap diagnostic, fixed support ($g^{I,FS}$)	0.269	4.879	5.506	0.153	1.145	13.346

Notes: Actual-size rows are the main accounting. Fixed-support rows multiply each reform's observed gain vector by $0.10/I_r$ while leaving its worker support unchanged. They aid comparison with estimates reported per 10-log-point statutory change but are not coherent common statutory counterfactuals. All entries are percentage points except ratios.

E.3 Alternative family weights

Table 14 replaces official ASECFTW with reference-person ASECWT, mean member ASECWT, and ASECWTH as alternative family resource-cost point weights. It reports the exercise separately for the primary interval and the inclusive diagnostic. Person outcomes continue to use ASECWT under every specification.

Table 14. Same-resource placement benchmarks under alternative family resource-cost weights

Family resource-cost weight	Headcount benchmark (pp)	Headcount ratio (%)	Censored FGT1 benchmark (pp)	FGT1 ratio (%)
<i>Panel A. Primary regular-statewide-floor interval</i>				
Official ASECFWT	0.814	0.989	0.070	6.792
Reference-person ASECWT	0.814	0.989	0.070	6.795
Mean person ASECWT	0.818	0.984	0.062	7.679
ASECWTH sensitivity	0.814	0.989	0.069	6.840
<i>Panel B. Inclusive incremental-gap diagnostic</i>				
Official ASECFWT	2.997	4.156	0.550	12.846
Reference-person ASECWT	2.997	4.156	0.549	12.860
Mean person ASECWT	3.006	4.143	0.516	13.690
ASECWTH sensitivity	2.988	4.168	0.544	12.986

Notes: Each panel varies only the full-sample family resource-cost point weight while retaining ASECFWT for person headcounts and censored-FGT1 contributions. Official ASECFWT is the preferred point-weight specification. Panel A applies the primary regular-statewide-floor interval; Panel B applies the inclusive incremental-gap diagnostic. The table measures point-estimate sensitivity to other proxy choices, not alternative replicate constructions. Appendix Table 21 compares the family-reference and household replicate-factor constructions used for the constructed cross-sectional replicate-sensitivity intervals.

E.4 Annual-hours coupling

Table 15 reports annual-hours groups defined by weighted quartile cutoffs and the upper tail of individual benchmark gains. Weighted cutoffs retain all ties, so group masses need not equal 25 percent.

Table 15. Annual-hours coupling and the upper tail of benchmark worker gains

Group	Mean annual hours	Positive-gain workers (%)	Benchmark resources (%)	Below no-state floor within group (%)
<i>Panel A. Annual-hours groups defined by person-weighted quartile cutoffs</i>				
Annual-hours group 1	481	25.18	7.86	92.04
Annual-hours group 2	1267	25.51	21.01	89.81
Annual-hours group 3	2007	39.42	52.70	87.68
Annual-hours group 4	2837	9.90	18.42	90.07
<i>Panel B. Percentiles of the annual inclusive incremental-gap diagnostic</i>				
90th percentile		1,560		2019 US dollars
95th percentile		2,080		2019 US dollars
99th percentile		3,177		2019 US dollars

Notes: Panel A forms annual-hours groups at person-weighted quartile cutoffs among workers assigned positive resources in the inclusive incremental-gap diagnostic. All observations tied at a cutoff remain together, so the four group masses need not equal 25 percent. Resource shares use the official family resource-cost weight and convert each gain to 2019 dollars before pooling across income years. The final column reports the share of each group's resources assigned to annual implied wages below the CPI-aligned no-state-reform floor. Panel B reports person-weighted percentiles of individual annual diagnostic gains in 2019 dollars. These calculations address the mechanical coupling created when annual hours both divide annual earnings to form the implied wage and multiply the statutory hourly gap.

E.5 Repeated respondents

Table 16 compares the full pooled sample with a valid-CPSIDP pooled bridge and with earliest- and latest-record samples. The bridge distinguishes selection associated with identifier availability from the additional change caused by retaining one record per valid CPSIDP.

Table 16. Sensitivity to repeated respondents across pooled ASEC years

Sample rule and gain definition	Headcount allocation (pp)	Headcount ratio (%)	FGT1 allocation (pp)	FGT1 ratio (%)
<i>Panel A. CPSIDP coverage in the full pooled sample</i>				
ASECWT-weighted person–exposure mass with valid CPSIDP (%)		100.000		
<i>Panel B. Mechanical accounting under alternative pooled-record rules</i>				
Full pooled sample, regular-statewide-floor interval	0.008	0.989	0.005	6.792
Full pooled sample, inclusive incremental-gap diagnostic	0.125	4.156	0.071	12.846
Valid-CPSIDP pooled sample, regular-statewide-floor interval	0.008	0.989	0.005	6.792
Valid-CPSIDP pooled sample, inclusive incremental-gap diagnostic	0.125	4.156	0.071	12.846
Earliest CPSIDP record, regular-statewide-floor interval	0.009	1.065	0.005	6.931
Earliest CPSIDP record, inclusive incremental-gap diagnostic	0.143	4.620	0.077	13.264
Latest CPSIDP record, regular-statewide-floor interval	0.008	0.954	0.005	6.934
Latest CPSIDP record, inclusive incremental-gap diagnostic	0.131	4.237	0.073	12.926

Notes: The full pooled sample includes records regardless of CPSIDP availability and permits a person to enter through distinct reform exposures. Panel A reports the ASECWT-weighted share of pooled person–exposure mass with a positive, nonmissing CPSIDP; its denominator is all state-scope person–exposure mass in the main sample. The valid-CPSIDP pooled rows retain every record with a valid identifier and therefore isolate selection associated with identifier availability without deduplicating respondents. The earliest and latest rows begin from that valid-identifier sample and retain one ASEC exposure record per CPSIDP, ordered by ASEC year and reform year. Person masses, family gains, and same-resource benchmarks are rebuilt after every restriction, so deduplicated rows need not reproduce complete original family membership. These are sensitivity checks, not longitudinal estimates or replacements for the main accounting.

E.6 Policy-source audit

Table 17 records the schedule’s sources, legal scope, and coding hierarchy. The event workbook distributed with the replication files contains the state and federal source fields and coding notes used to reconstruct the monthly path.

Table 17. Minimum-wage schedule sources and coding conventions

Element	Source and coding rule
Federal minimum wage	U.S. Department of Labor historical federal minimum-wage changes; exact effective dates are retained in the federal event sheet.
State annual levels	U.S. Department of Labor state minimum-wage tables provide the initial annual level check.
State effective dates	State statutes, regulations, labor-department pages, and wage posters are recorded in event-level source, secondary-source, and coding-note fields.
Binding statewide rate	For each state and month, the effective rate is the maximum of the regular statewide state floor and the federal floor.
Excluded legal variants	Local ordinances and separate tipped, youth, training, small-employer, occupation-specific, and enterprise-coverage schedules are not coded as the regular statewide floor.
Conflict resolution	Exact dated state-source events govern the monthly path; annual DOL levels are used as cross-checks, and discrepancies remain auditable in the workbook’s source and coding-note fields.
Replication file	The released state-month workbook contains the full 2006–2019 schedule together with state-event and federal-event source fields.

Notes: The schedule describes regular statewide statutory floors, not legal coverage for each worker–job observation. Accordingly, the paper refers to workers in the regular-statewide-floor interval or workers assigned positive benchmark gains and avoids legal-coverage labels.

E.7 Coverage, baseline-work timing, and interview timing

Table 18 separates sensitivities for the inclusive incremental-gap diagnostic from those for the primary interval. It reports coverage restrictions, extreme timing assignments of baseline annual weeks across reform-year monthly policy gaps, interview timing, counterfactual paths, work-input restrictions, and the 2014 split-panel checks.

E.8 Annual and monthly wage diagnostics

Table 19 compares annual implied wages with EARNWT-weighted monthly CPS wage measures in aligned worker samples.

E.9 Sampling and reform-state composition sensitivities

Tables 20 and 21 distinguish constructed cross-sectional replicate-sensitivity intervals for smooth quantities from leave-one-state-out ranges for nonlinear placement statistics and audit

Table 18. Policy-path, baseline-work timing, coverage, and measurement sensitivities

Specification	Cells	Below comparison floor: workers (%)	Below comparison floor: resources (%)	Headcount change (pp)	Headcount ratio (%)	FGT1 ratio (%)
<i>Panel A. Inclusive incremental-gap diagnostic sensitivities</i>						
State-policy counterfactual, federal path retained	212	85.97	88.94	0.125	4.16	12.85
Full effective-rate path in state-driven years	212	84.85	87.85	0.131	4.27	12.98
Positive real-floor-change benchmark	212	86.72	87.90	0.094	4.06	12.77
CPI-aligned baseline-year average floor	212	82.88	87.56	0.154	4.55	13.06
No baseline-year effective-floor change	64	78.50	84.22	0.190	5.37	13.66
January–March implementation	170	86.22	88.76	0.146	4.41	13.04
January–June implementation	176	86.44	88.80	0.142	4.38	13.06
July–December implementation	36	84.37	90.27	0.061	2.90	11.07
Reform years 2007–2014	109	86.31	89.12	0.109	3.81	14.49
Reform years 2010–2019	151	86.07	88.83	0.118	4.08	11.94
Annual hours at most 3,000	212	85.87	88.86	0.117	4.06	13.33
Usual weekly hours at most 80	212	85.96	88.92	0.124	4.16	12.88
At least 45 weeks worked	212	84.94	88.55	0.113	4.11	11.71
50–52 weeks worked	212	84.87	88.51	0.108	4.00	11.52
Full-time, 50–52 weeks	212	83.77	88.22	0.088	3.72	10.86
2014 legacy 5/8 income sample	212	86.01	88.93	0.131	4.35	13.03
Exclude 2014 ASEC baseline	195	86.38	89.14	0.128	4.24	12.91
<i>Panel B. Primary regular-statewide-floor-interval sensitivities</i>						
Regular-statewide-floor interval, uniform work placement	212	–	–	0.008	0.99	6.79
Regular-statewide-floor interval, lower baseline-work timing bound	212	–	–	0.008	1.00	6.75
Regular-statewide-floor interval, upper baseline-work timing bound	212	–	–	0.008	0.99	6.84
Exclude potentially tipped occupations	212	–	–	0.007	0.89	6.70
Exclude workers ages 16–19	212	–	–	0.008	1.02	6.87
Exclude potentially tipped occupations and ages 16–19	212	–	–	0.007	0.92	6.75
ASEC interview precedes first state reform	42	–	–	0.006	1.16	6.35

Notes: Panel A reports the inclusive incremental-gap diagnostic; Panel B reports the primary regular-statewide-floor interval. The below-floor columns are not applicable to Panel B by construction. The preferred state-policy row freezes the prior-December state floor, retains the federal path, and applies the same CPI conversion to both paths. Alternative path and period rows use the stated definitions. Work-input and coverage rows retain the full person and family denominators but set gains to zero outside the restriction. Potentially tipped occupations are identified from occupation codes; the data do not identify actual tip-credit coverage. The lower and upper baseline-work timing bounds carry prior-income-year annual weeks into the reform year, cap placement at 52/12 weeks per month, and assign weeks first to the smallest or largest monthly policy gaps while retaining usual weekly hours. They are extreme timing scenarios conditional on baseline annual work inputs, not bounds on observed reform-year work calendars. The interview-timing row requires the ASEC interview month to precede the first state reform month. The main 2014 sample uses the redesigned 3/8 panel. The extract does not include allocation flags for annual wage income, weeks, and hours.

Table 19. Annual and recent monthly wage diagnostics in aligned worker samples

Comparison sample	Diagnostic	ASEC main-specification weighting (%)	ASEC worker weighting (%)	Monthly CPS EARNWT (%)
<i>Panel A. Wage-distribution diagnostics</i>				
Employed wage-and-salary workers	Below comparison floor among valid wage observations	9.95	9.95	5.64
Employed wage-and-salary workers	Below comparison floor among observations below the source-specific new floor	85.59	85.59	65.62
Employed wage-and-salary workers	Incremental-gap amount assigned below the comparison floor	88.76	88.96	66.67
Hourly-paid wage-and-salary workers	Below comparison floor among valid wage observations	10.05	10.05	5.76
Hourly-paid wage-and-salary workers	Below comparison floor among observations below the source-specific new floor	83.90	83.90	51.74
Hourly-paid wage-and-salary workers	Incremental-gap amount assigned below the comparison floor	86.95	87.27	51.07
<i>Panel B. Alignment and sample support</i>				
ASEC broad sample	Valid annual wage observations		311,500	
ASEC hourly-paid sample	Valid annual observations classified as hourly paid		40,788	
Monthly CPS broad sample	Valid recent-month wage observations		172,850	
Monthly CPS hourly-paid sample	Valid recent-month reported-hourly-wage observations		109,770	
ASEC-monthly linkage	Person-weighted share of the ASEC broad sample		37.63%	
Hourly-paid status availability	Person-weighted share among linked ASEC records		65.71%	

Notes: Both sources include employed wage-and-salary workers ages 16–64; hourly-paid rows also require reported hourly-paid status. ASEC wages divide annual wage income by weeks and usual weekly hours. Its comparison floor is the CPI-aligned maximum no-state-reform floor, and its new-floor cutoff is the maximum observed effective floor in the reform year. Monthly CPS rows use wages and the old and new effective rates at the first state reform step, based on the three preceding months. Thus, the source-specific new-floor cutoffs need not coincide in years with multiple state steps. Each source expresses wages and floors in common within-source price units. Monthly earnings and hourly-pay statistics use EARNWT. In the ASEC main-specification column, incidence rows use ASECWT and resource rows use ASECFWT; the worker column uses ASECWT throughout. Monthly gaps omit annual hours and diagnose wage measurement rather than reproduce the resource estimand. Linkage precedes the hourly-paid restriction, using the last nonmissing baseline-year monthly status.

the signed inputs used in the replicate calculations.

Table 20. Constructed cross-sectional replicate-sensitivity intervals and leave-one-state-out ranges

Estimand	Estimate	Constructed replicate-sensitivity interval	Leave-one-state-out range
Regular-statewide-floor interval: Workers in baseline-poor families (%)	14.056	[12.832, 15.280]	[13.787, 14.655]
Regular-statewide-floor interval: Resources assigned to baseline-poor families (%)	11.558	[10.030, 13.085]	[11.249, 11.875]
Regular-statewide-floor interval: Mechanical headcount reduction (pp)	0.008	[0.005, 0.011]	[0.006, 0.009]
Regular-statewide-floor interval: Headcount same-resource benchmark (pp)	0.814	–	[0.638, 0.890]
Regular-statewide-floor interval: Headcount placement ratio (%)	0.989	–	[0.924, 1.056]
Regular-statewide-floor interval: Censored FGT1 reduction (pp)	0.005	[0.004, 0.005]	[0.004, 0.005]
Regular-statewide-floor interval: Censored FGT1 same-resource benchmark (pp)	0.070	–	[0.051, 0.079]
Regular-statewide-floor interval: Censored FGT1 placement ratio (%)	6.792	–	[6.630, 6.971]
Regular-statewide-floor interval: FGT1 minus headcount placement ratio (pp)	5.803	–	[5.598, 5.977]
Inclusive incremental-gap diagnostic: Workers in baseline-poor families (%)	23.897	[23.299, 24.496]	[23.135, 24.867]
Inclusive incremental-gap diagnostic: Resources assigned to baseline-poor families (%)	22.807	[22.069, 23.546]	[22.441, 24.510]
Inclusive incremental-gap diagnostic: Mechanical headcount reduction (pp)	0.125	[0.109, 0.140]	[0.099, 0.137]
Inclusive incremental-gap diagnostic: Headcount same-resource benchmark (pp)	2.997	–	[2.522, 3.165]
Inclusive incremental-gap diagnostic: Headcount placement ratio (%)	4.156	–	[3.933, 4.313]
Inclusive incremental-gap diagnostic: Censored FGT1 reduction (pp)	0.071	[0.068, 0.073]	[0.055, 0.077]
Inclusive incremental-gap diagnostic: Censored FGT1 same-resource benchmark (pp)	0.550	–	[0.411, 0.603]
Inclusive incremental-gap diagnostic: Censored FGT1 placement ratio (%)	12.846	–	[12.712, 13.275]
Inclusive incremental-gap diagnostic: FGT1 minus headcount placement ratio (pp)	8.691	–	[8.412, 9.342]

Notes: Constructed intervals use all 160 ASEC successive-difference replicate columns, apply the same replicate index across annual files, and use the mean-squared-error convention and multiplier 4/160. Reported brackets equal the full-sample estimate plus or minus $t_{0.975,159}$ times the replicate standard error; this multiplier supplies a conventional scale and is not assigned a frequentist coverage interpretation. Person mass is the sum of member REPWTP, and family resource totals scale ASECFWT by the family reference person's REPWTP-to-ASECWT factor. These are constructed cross-sectional replicate-sensitivity intervals conditional on the pooled reform-exposure construction, not official ASECFWT replicate confidence intervals. They do not account for repeated respondents across ASEC years or separately model cross-year covariance. Dashes identify nonlinear statistics for which signed replicate inputs violate the allocation problem's nonnegative cost and benefit requirements. Leave-one-state-out ranges delete each complete reform-state history in turn and are not confidence intervals.

Table 21. Family-level SDR input audit and replicate-factor comparison

Diagnostic	Preferred family-reference factor	Household-factor	QA alternative
<i>Panel A. Replicate-record and replicate-cell QA counts accumulated over 160 replicates</i>			
Negative source replicate-weight records	110		0
Nonpositive constructed family resource weights	110		0
Nonpositive family person masses	103		103
Nonpositive costs for families with positive official gaps	30		0
Nonpositive family weights attached to positive predicted gains	0		0
Nonpositive family person masses among valid IPR records	103		103
Replicate-cell incidences with any nonpositive input	88		82
Replicate-cell incidences affecting a placement-benchmark input	82		82
<i>Panel B. Reporting treatment</i>			
Smooth totals, shares, and ratios remain algebraically defined	Yes		Yes
Smooth SDR intervals used in the main tables	Yes	No; QA alternative only	
Nonlinear placement-benchmark or ratio replicate interval	Not reported		Not reported
Treatment of nonpositive replicate inputs	Not truncated		Not truncated
Benchmark and ratio composition summary	Leave-one-state-out range		

Notes: The preferred family-reference construction scales ASECFTW by the official-family reference person's REPWTP-to-ASECWT ratio. The household alternative scales ASECFTW by REPWT-to-ASECWT. Both use the sum of member REPWTP as family person mass. Counts are accumulated across 160 replicate estimations and therefore are replicate-record or replicate-cell incidences, not unique observations or cells. Signed replicate weights remain valid sensitivity inputs for smooth totals and ratios, and no constructed family resource weight attached to a positive predicted gain is nonpositive. The nonlinear allocation problem is different: neither construction yields uniformly positive family person masses in the analytic sample, and the family-reference method also yields some nonpositive resource weights and gap costs. Sorting, cost-per-benefit ratios, and fractional placement require nonnegative costs and benefits, so nonlinear replicate intervals are not reported. No supplied replicate weight is truncated or reset. Leave-one-state-out ranges summarize sensitivity to reform-state composition.

E.10 Inclusive diagnostic by annual implied-wage region

Table 22 shows that the inclusive incremental-gap diagnostic is concentrated among annual implied wages below the CPI-aligned no-state-reform floor.

Table 22. Wage-region origin of the inclusive incremental-gap diagnostic

Annual implied-wage region	Positive-gain workers (%)	Predicted resources (%)	Poor-family resource share (%)	Stand-alone headcount change (pp)
Below CPI-aligned no-state floor	85.967 [85.541, 86.394]	88.940 [88.393, 89.487]	24.206 [23.386, 25.026]	0.115 [0.100, 0.130]
CPI-aligned regular-statewide-floor interval	14.033 [13.606, 14.459]	11.060 [10.513, 11.607]	11.558 [10.030, 13.085]	0.008 [0.005, 0.011]
At or above maximum regular statewide floor	0.000 [0.000, 0.000]	0.000 [0.000, 0.000]	–	0.000 [0.000, 0.000]
<i>Resource scale by gain definition</i>				
Regular-statewide-floor interval	4,804.4 million 2019 dollars		481 dollars per worker	0.989% placement ratio
Inclusive incremental-gap diagnostic	43,322.0 million 2019 dollars		607 dollars per worker	4.156% placement ratio

Notes: The inclusive incremental-gap diagnostic assigns predicted resources wherever the state-policy path raises the regular statewide floor’s wage gap, including observations below the CPI-aligned no-state-reform floor. It is a diagnostic rather than a measure of newly mandated legal resources. Brackets are constructed cross-sectional replicate-sensitivity intervals conditional on the pooled exposure construction. Positive-gain worker and resource shares sum to 100 across the first two wage-region rows. The stand-alone calculations need not add because worker resources are pooled within official families.

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