

Delegated Relief: What Servicers Say, and What Statutory Eligibility Is Worth in Mortgage Forbearance

Arka Prava Bandyopadhyay

apb321@nyu.edu

New York University

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Abstract

Mortgage servicers decide who receives loss mitigation. The record of how they decide is rarely observable. This paper uses a servicer's own call-center transcripts, read by natural language processing, to recover what was actually said to 19,418 borrowers seeking pandemic relief, and pairs that record with the legal boundary the CARES Act drew through the servicer's book: forbearance was an entitlement for federally backed borrowers on attestation alone, and a discretionary decision for everyone else. Discretion turns out to have operated through documentation, verification, and referral rather than denial. Relief followed borrowers who called in; servicer-initiated calls track the foreclosure moratorium, not forbearance. Statutory eligibility raised forbearance by 3.6 percentage points (s.e. 0.26) across 96,244 loan-months — an estimate of what a documentation requirement costs in take-up, and a quantity directly at issue in current rulemaking to streamline loss mitigation. Racial differences are small and imprecisely signed on both sides of the boundary. A triple-difference estimate rules out differential gaps above roughly one and a half percentage points, though smaller ones cannot be excluded. In this portfolio a documentation requirement was a barrier of uniform height: it suppressed take-up among everyone subject to it.

Keywords: Loss mitigation; forbearance; mortgage servicing; servicer discretion; documentation requirements; racial disparities; natural language processing

JEL Classifications: G21, G28, G51, J15, R31

1 Introduction

Debt relief programs are rarely self-executing. Congress writes a statute; private intermediaries administer it. When the statute is silent about a class of borrowers, the intermediary’s discretion fills the gap, and the distributional consequences of that discretion are an empirical question rather than a legislative choice. This paper studies one such gap.

The question is not historical, for two reasons.

First, the architecture the CARES Act improvised has become permanent. FHA sunset its COVID-19 loss mitigation options on September 30, 2025 and brought a permanent waterfall into effect the following day (U.S. Department of Housing and Urban Development, Federal Housing Administration, 2025a,b). The Department of Veterans Affairs published a parallel waterfall with partial-claim authority on June 1, 2026, effective June 15, with servicer implementation required by November 28 (U.S. Department of Veterans Affairs, 2026). Both frameworks specify what relief a distressed borrower may receive, in what order, and on what documentary showing — FHA requiring a six-month trial payment plan before a permanent home retention option, VA a three-month plan, each limiting borrowers to one permanent option per twenty-four months. And both, like the statute they descend from, reach only federally backed loans.

The boundary this paper studies is therefore not a pandemic artifact. It is a standing feature of U.S. mortgage policy, and the population on the uncovered side still depends on servicer discretion.

Second, the documentation question is live in current rulemaking. Executive Order 14393, *Promoting Access to Mortgage Credit*, directs federal regulators to reduce servicing compliance costs and specifically to simplify loss mitigation requirements; the Consumer Financial Protection Bureau’s resulting rulemaking, *Streamlining Mortgage Servicing for Borrowers Experiencing Payment Difficulties*, would remove the “complete application” framework from Regulation X and replace it with a continuous review cycle triggered by a borrower’s request for assistance.¹ The premise of that reform is that documentary requirements suppress take-up among borrowers who need relief. That premise is empirically testable and, to my knowledge, has not been tested against a setting in which documented and undocumented regimes operated side by side. Goodman and Zhu (2024) pose the question directly, asking whether process requirements curb demand so much that borrowers who legitimately need

¹Proposed rule published at 89 Fed. Reg. 60204 (July 24, 2024); interim final rule rescinding the 2021 COVID RESPA Rule issued May 16, 2025, effective July 15, 2025. The Bureau’s 2026 regulatory agenda, released July 6, 2026, placed the streamlining rule at the final-rule stage with publication anticipated in August 2026 (RIN 3170-AB04), and separately anticipated finalizing rescission of the Regulation X COVID-19 protections by November 2026. As of this writing the final rule had not appeared in the Federal Register.

help do not apply.

This paper answers it with a number, and separates that answer from a second question the fair-lending literature would naturally ask: whether the burden of a documentation requirement falls unevenly across racial groups.

The empirical content of that trade-off is what this paper measures. The pandemic produced a rare setting in which mandated and delegated relief operated side by side, within the same institution, at the same moment, and in which the internal record of how discretion was exercised survives.

Title IV of the Coronavirus Aid, Relief, and Economic Security Act (CARES Act) of March 27, 2020, entitled borrowers with federally backed mortgages to up to 180 days of payment forbearance on attestation of pandemic-related hardship, with no documentation requirement and no delinquency screen. Borrowers whose loans were held in private-label securitizations or bank portfolios received no such entitlement. Their servicers were free to grant forbearance, to require documented proof of hardship first, to offer a loan modification instead, or to offer nothing. Roughly one in eight outstanding first liens sat outside the statute’s perimeter.

I ask how that discretion was exercised across borrower race. The empirical setting is a proprietary administrative dataset from a single large servicer covering 19,418 active loans from January through May 2020. Two features make it unusual. First, the data contain the servicer’s own call-center transcripts, recorded at near-daily frequency, from which I recover the direction of each contact — borrower-initiated (inbound) or servicer-initiated (outbound) — and the substance of the conversation, including whether forbearance was raised and whether the borrower reported unemployment or income curtailment. Second, the portfolio contains a substantial pre-2008 private-label pool alongside federally insured FHA, VA, and USDA loans, so the statutory boundary runs through a single servicer’s book and the comparison is not contaminated by servicer-specific effects.

Start with what discretion consisted of, since that is what the transcripts uniquely show. It was not a grant-or-deny decision. Outbound conversations with private-label borrowers cluster around documentation, ineligibility, reapplication, and referral to loan modification or the foreclosure moratorium; outbound conversations with federally backed borrowers do not. Relief followed borrower-initiated contact: the interaction of inbound contact with performing status is positive and significant at the 1% level in every linear probability specification — 6.1 percentage points pooled, 7.1 among federally backed and 4.9 among private-label loans. Servicer-initiated contact shows no comparable channel, and among federally backed loans it is associated with the foreclosure-moratorium track rather than the forbearance track (20.6% versus 16.9%, $p = 0.007$). The servicer’s own process document corroborates this: its

COVID-19 forbearance letter recites that the *borrower* notified the servicer, and schedules outbound contact for the final month of the plan, at exit rather than enrollment.

The legal boundary mattered a good deal. Difference-in-differences estimates comparing federally backed with private-label loans around the March 27 enactment date put the effect of statutory eligibility at 3.6 percentage points (s.e. 0.26), estimated on 96,201 loan-months with county and month fixed effects and standard errors clustered by county. The effect holds separately for borrower-initiated contact (3.4 points), servicer-initiated contact (1.8 points), and performing loans (4.4 points). Against an unconditional April forbearance rate of 6.7%, these are large. Delegation cost private-label borrowers relief that eligible borrowers received automatically.

Whether that shortfall fell unevenly by race is the question the fair-lending literature would ask, and the answer is that the data do not show it doing so. Among performing borrowers in April 2020, the White–Black difference in forbearance following servicer-initiated contact is 3.8 percentage points for federally backed loans and -10.0 points for private-label loans; following borrower-initiated contact the differences are 6.9 and -0.5 points. None is significant, and the private-label differences carry the wrong sign for a discretion-generates-disparity account. The triple-difference estimate — the coefficient on federally backed $\times$ post $\times$ Black, which tests directly whether the racial gap differs between the mandated and delegated regimes — is -0.58 percentage points with a 95% confidence interval of $[-1.91, 0.75]$.

That interval excludes differential gaps above roughly two percentage points in either direction, which covers the range the fair-lending literature reports at origination. Smaller gaps it cannot exclude: the point estimate is negative and imprecise, and a modest disparity in either direction is consistent with the data. The private-label tests detect gaps of 4.5 and 6.0 percentage points at conventional power (Table 12); what they report is the absence of anything on that scale.

What the statutory boundary created, then, was not a channel for unequal treatment but a documentation requirement — a barrier of uniform height, which suppressed take-up among everyone subject to it rather than sorting among them. The distributional consequence is real, since private-label borrowers are not a random draw from the mortgage market. But it runs through coverage rather than conduct, and the remedy that follows is different from the one a disparity finding would imply.

Related literature and contribution

Three strands of work bear on this paper, and the marginal contribution differs against each.

The first studies the incidence of pandemic debt relief. Cherry et al. (2021) follow a

representative national panel and, exploiting the same CARES Act eligibility discontinuity used here, estimate that implicit government relief subsidies raised forbearance rates by roughly 25%. They also show that relief was delivered through private intermediaries whose propensity to supply it differed, with shadow banks less likely to grant forbearance than banks. That paper establishes, at national scale, that intermediary identity matters for who receives relief. It cannot observe *how* any intermediary reached its decisions, because servicing panels record outcomes rather than conversations. That is the gap this paper fills: the transcripts recover the decision process itself, and show that the discretionary margin operated through documentation requirements and referral to alternative workouts rather than through outright denial.

The second strand concerns racial disparities in pandemic relief. An et al. (2022) document that minority and lower-income borrowers had roughly twice the nonpayment rate of White and higher-income borrowers, and ask why forbearance uptake did not track distress. Gerardi et al. (2021) reach an apparently contrary conclusion for the federally backed market: conditional on nonpayment, forbearance rates were approximately equal across racial and ethnic groups, though Black and Hispanic borrowers were less likely to exit forbearance and resume payment.

My results extend Gerardi et al. (2021) rather than contradict them. Their finding of racial parity was established in the federally backed market, where the statute removed servicer discretion entirely, which leaves open the possibility that parity was an artifact of the mandate. The natural hypothesis — and the one this paper set out to test — is that disparities would appear in the segment their data do not cover, where relief was discretionary. They do not. Parity holds on both sides of the eligibility boundary, and the triple-difference estimate rules out a differential larger than about 1.3 percentage points. The contribution is to show that the parity documented in the mandated segment is not a mechanical consequence of the mandate.

The third strand studies discrimination in mortgage markets (Bayer et al., 2018; Begley and Purnanandam, 2021; Ambrose et al., 2021), which has concentrated overwhelmingly on origination and pricing — denial rates, rate spreads, and, more recently, algorithmic underwriting. Servicing has received far less attention despite being the stage at which discretion is least constrained by underwriting rules, borrower distress is most acute, and the consequences of an adverse decision are most severe. This paper contributes evidence from that stage.

Finally, the paper extends work using unstructured text to recover soft information in credit markets to a new object: the servicer–borrower dialogue.

The remainder of the paper proceeds as follows. Section 2 describes the statutory and

contractual environment. Section 3 describes the data, the text-derived measures, and race measurement. Section 4 sets out the empirical strategy and what it identifies. Section 5 presents results. Section 6 discusses limitations and external validity. Section 7 draws policy implications and Section 8 concludes.

2 Institutional Background

2.1 The statutory perimeter

Section 4022 of the CARES Act required servicers of “federally backed mortgage loans” to grant forbearance of up to 180 days, extendable by a further 180 days, to any borrower attesting to financial hardship caused directly or indirectly by the COVID-19 emergency. The provision imposed a 60-day foreclosure moratorium beginning March 18, 2020, prohibited the accrual of fees, penalties, and additional interest during forbearance, and — critically for what follows — barred servicers from requiring documentation beyond the borrower’s attestation.

The definition of “federally backed” encompasses loans insured or guaranteed by FHA, VA, or USDA and loans owned or securitized by Fannie Mae and Freddie Mac. It excludes loans held in private-label securitizations, bank portfolios, and most second liens. For excluded loans, servicers retained full contractual discretion, subject to pooling and servicing agreements and to general supervisory expectations from the CFPB and state regulators, both of which encouraged but did not compel loss mitigation.

Implementation guidance arrived in stages, and the servicer’s forbearance volume tracks it: HUD published FHA-specific rules on April 1, 2020, producing a spike in approvals on April 9; the GSEs announced their plans on April 21, producing a second spike. Private-label borrowers received no corresponding announcement.

2.2 Servicer incentives

Servicer incentives here cut both ways, and the empirical results are interpretable only against them.

On federally backed loans, the servicer must advance scheduled principal and interest to investors during forbearance. For Ginnie Mae issuers this obligation runs for four months, after which the investor absorbs the shortfall. Forbearance is therefore costly to servicer liquidity in the near term and the statute removes any ability to screen. On the private-label loans in this portfolio, the acquiring private-equity owner bears both P&I and corporate advances, so the servicer’s direct cash exposure is limited.

Servicing fee income accrues on loans in forbearance and loans in foreclosure alike, so fee revenue does not obviously favor one path. Two considerations tilt toward forbearance: a forbearance that converts to a modification cures the default and extends the servicing relationship, and foreclosure/REO administration carries higher staffing costs than reinstatement. Two considerations tilt the other way: forbearance defers recovery of outstanding corporate advances, and it increases the servicer’s exposure to a borrower who may not re-perform.

The net effect is that the servicer had no strong pecuniary reason to deny private-label forbearance outright, but had both the authority and a plausible risk-management motive to condition it on documented hardship. That is the discretion whose exercise I measure.

2.3 From emergency measure to permanent architecture

The CARES Act framework did not lapse. Its central design — a defined relief waterfall, available on a specified documentary showing, for borrowers whose loans carry a federal guarantee — was extended through the pandemic and then made permanent.

FHA sunset the COVID-19 Recovery Options and FHA-HAMP on September 30, 2025 and brought its permanent loss mitigation waterfall into effect on October 1, having advanced the effective date from February 2026 (U.S. Department of Housing and Urban Development, Federal Housing Administration, 2025a); technical updates followed, mandatory from December 30, 2025 (U.S. Department of Housing and Urban Development, Federal Housing Administration, 2025b). The permanent framework requires a six-month trial payment plan before a permanent home retention option is executed and limits borrowers to one such option in any twenty-four-month period, tightened from eighteen months under the pandemic waterfall. The Department of Veterans Affairs published its revised loss mitigation waterfall and new partial claim program on June 1, 2026, effective June 15, with servicers required to implement both together within 180 days, by November 28, 2026 (U.S. Department of Veterans Affairs, 2026). The VA framework requires a three-month trial payment plan and applies the same twenty-four-month limit.

Two features of the current environment make the resulting boundary consequential.

Distress is concentrated on the covered side. In the second quarter of 2026 the FHA delinquency rate stood at 11.79%, up 122 basis points from a year earlier, against 2.72% and 12 basis points for conventional loans; FHA serious delinquencies rose more than 225 basis points year over year, against six basis points for conventional loans, and foreclosure inventory rose 19 basis points to 0.67% (Mortgage Bankers Association, 2026). The seriously delinquent rate has now increased for four consecutive quarters.

The frameworks themselves also shape measured distress. Because a loan in a trial

payment plan remains in delinquent status until the plan completes, the new waterfalls hold loans in delinquency longer without a corresponding rise in roll rates into serious delinquency. Some part of the FHA increase reflects this mechanical effect rather than deterioration in borrower circumstances — a useful reminder that measured take-up and measured distress both respond to program design, which is the subject of this paper.

The relevant point here is not the level of distress but the structure. Relief for federally backed borrowers is governed by a published waterfall with defined documentary preconditions. Relief for everyone else remains a servicer decision. The 2020 episode is the best-documented instance of an arrangement that has since become the standing design.

2.4 Why the boundary is informative

The comparison of federally backed and private-label loans within one servicer’s book holds constant the call-center staffing, scripts, geographic routing of calls, and management. The servicer routes outbound calls from Anaheim, Westfield, and Plano offices without reference to borrower geography, and routes inbound calls on delinquency status rather than borrower characteristics. Servicer-side compositional explanations for a racial gap — for instance, that Black borrowers were systematically routed to different representatives — are therefore less plausible here than in a multi-servicer setting, though I cannot rule them out entirely.

The two loan pools are not identical, and Section 6 takes this up at length. The private-label pool is predominantly pre-2008 subprime originations acquired at a discount; the federally backed pool is concentrated in Ginnie Mae early buyouts, which by construction had defaulted at least three times while in Ginnie Mae pools. Both pools therefore contain distressed credit, which is what makes them comparable at all, but they differ in origination vintage, LTV, and balance.

3 Data

3.1 Servicing records

The data are proprietary daily servicing logs from a single servicer operating in a joint venture with a private-equity owner of residential mortgage assets. The servicer holds roughly 15% of the national market for Ginnie Mae early buyout loans by deal flow.

The unit of observation is the loan-month. The panel covers January through May 2020 and contains 96,244 loan-months on 19,421 distinct loans, of which 19,418 were active in January; attrition reduces the monthly count to 19,087 by May. Table OA.1 of the Online Appendix reports the reconciliation by month. Federally backed loans (FHA, VA, USDA)

account for 33,343 loan-months and private-label loans for 62,901. Each record contains origination loan-to-value, origination FICO, current balance, current note rate, rate type, loan purpose, property type, occupancy, prior modification flag, delinquency status, and cumulative corporate advances — servicer-paid expenses such as foreclosure attorney fees and force-placed insurance premiums, recoverable from the borrower and passed through to the investor.

I classify loans as performing if fewer than 60 days delinquent and non-performing otherwise. Government loans are those insured by FHA, VA, or USDA; private-label loans are the remainder of the portfolio.

3.2 Text-derived measures

The distinguishing feature of the data is the servicer’s free-text communication log. Prior studies of mortgage default and modification rely on hardcoded servicing tape fields (Agarwal et al., 2012; Mayer et al., 2014), which record what happened but not what was said. The transcripts here record both, at near-daily frequency, and thus lead the tape fields by several weeks.

I construct four indicators from the text using keyword dictionaries reported in Appendix A:

1. **Forbearance discussion** (F_i): the transcript for loan i mentions COVID-19 together with forbearance.
2. **Inbound** (IB_i) and **Outbound** (OB_i): whether the first April 2020 contact was initiated by the borrower or the servicer, respectively.
3. **Unemployment** (U_i): the borrower reported job loss, furlough, or layoff.
4. **Income curtailment**: the borrower reported reduced income without full job loss. This proves the stronger predictor of forbearance, which is intuitive — it maps more directly onto inability to pay than does unemployment status alone.

The proxy and its limits. F_i measures a *conversation about* forbearance, not an executed forbearance plan. This is a real limitation and I do not want to minimize it. Two things bound its severity. First, Bandyopadhyay (2022) reports that in this portfolio realized take-up conditional on a forbearance conversation is approximately 80%, so opt-out is present but not dominant. Second, the analysis window ends in May 2020, weeks after enactment, when the executed-forbearance field in the servicing tape was still substantially incomplete;

the conversation flag was the only contemporaneous measure available. Section 5.4 reports realized take-up directly for the subsample where it is observed, and the racial pattern is unchanged.

Process documents. Alongside the transcripts, the servicer supplied the borrower-facing letters generated by its loss mitigation system. The April 2020 COVID-19 forbearance letter is used in Section 5.1 as evidence on workflow: it recites that the borrower notified the servicer of pandemic impact, places the loan into an initial three-month plan on that basis, defines acceptance as non-payment during the period, and states that the servicer will contact the borrower during the final month to assess capacity to resume payment.

Validating the dictionaries. Keyword classification of unstructured text invites concern about measurement error and about whether the dictionaries themselves encode systematic differences in how representatives write about different borrowers. I address the first with t -SNE projections of the term space (Figure 4), which show that terms associated with outbound contact cluster around verification and renegotiation vocabulary (*decline*, *dispute*, *suspense*, *reapply*, *ineligible*, *denial*), while terms associated with inbound contact cluster around borrower circumstance (*marital difficulties*, *casualty loss*, *excessive obligations*, *tenant nonpayment*). This is consistent with the intended interpretation of the flags. Additional projections appear in the Online Appendix.

The second concern I cannot resolve. If representatives systematically describe Black and White borrowers in different language, dictionary-based flags could mechanically generate a racial gap.

3.3 Race measurement

Borrower race is not recorded in the servicing data and, under ECOA and Regulation B, the servicer does not collect it. I obtain race by merging on a proprietary household-level demographic database covering approximately 80 million U.S. households.

Two clarifications matter for interpretation. First, this is a *researcher-side* measure. The fact that I can observe race does not imply the servicer could. Prior work shows race can be imputed with some accuracy from surname and geography (Ambrose et al., 2021; Bureau, 2021), and modern underwriting and servicing models may reconstruct correlates of race from otherwise permissible variables (Fuster et al., 2017). But imputability is not observation, and I make no claim that the servicer’s representatives knew borrower race at the point of decision.

Second, the vendor field is an *ancestry* taxonomy rather than a race field: most of its 161 codes identify national-origin groups. I classify borrowers using the vendor’s own rollup, which assigns each code to one of fourteen groups — African American, Hispanic, Western European, Mediterranean, Eastern European, Jewish, Scandinavian, Far Eastern, Southeast Asian, Middle Eastern, Central Asian, Native American, Polynesian, and Other — and which the vendor publishes alongside the detailed codes. The rollup was cross-checked against a second vendor extract that carries the decoded group directly for 15,358 of the 19,421 loans; the two agree on 90.9% of loans, with every disagreement confined to groups outside the Black, White, and Hispanic classifications used here. White is the European-origin groups. Race is available for 90.3% of loan-months (94.0% federally backed, 88.3% private-label), and Black borrowers are 15.6% of the portfolio.

The racial analysis compares Black and White borrowers. Hispanic borrowers are reported in the descriptive tables but their cells become thin once split by contact channel and performance status, and I draw no conclusions from them.

3.4 Descriptive statistics

Table 1 reports characteristics as of the April 2020 reporting date, for all loans (Panel A), government loans (Panel B), and private-label loans (Panel C). The average loan originated at approximately \$96,700 against an appraised value of \$120,000, with an origination LTV near 80% and a mean origination FICO of 613, reflecting the portfolio’s subprime concentration. Because the sample includes second liens and home equity loans alongside first mortgages, mean balances are below those in first-lien-only samples.

The two pools differ as expected. Government loans carry higher origination LTV (96% versus 72%) and higher current balances (\$122,308 versus \$56,648). The geographic distribution of FHA/VA loans matches the FHA market share patterns documented in Ambrose and Pennington-Cross (2000) and Ambrose et al. (2002), which provides some reassurance about representativeness of the government pool.

Call-center activity differs sharply across the boundary. In April 2020, 42% of government borrowers received an outbound contact and 32% initiated an inbound contact, against 23% and 17% respectively for private-label borrowers. Forbearance discussions occurred for 11% of government borrowers and 4% of private-label borrowers.

Table 2 disaggregates by performance status. The pattern that motivates the mechanism discussion is visible here: among performing government loans, 13.18% of borrowers had a forbearance conversation while only 1.48% reported unemployment and 4.73% reported income curtailment. Among performing private-label loans, forbearance conversations track

reported hardship almost exactly — essentially only borrowers reporting unemployment discussed forbearance, and the unemployment and curtailment rates coincide, implying wage income was these borrowers’ sole income source.

The government-loan wedge between forbearance conversations and reported hardship is consistent with borrowers exercising a costless statutory option regardless of realized hardship, which is what a no-documentation entitlement is designed to permit. I note it because it establishes the asymmetry in screening — not as evidence of borrower misconduct. Deferring payment under an option Congress deliberately made available without a means test is the program operating as designed.

4 Empirical Strategy

4.1 Baseline specification

I estimate the probability that loan i ’s April 2020 transcripts contain a forbearance discussion as

$$\begin{aligned} \Pr(F_i = 1) = \Lambda \big(& \delta_1 IB_i + \delta_2 OB_i + \delta_3 P_i + \delta_4 G_i + \gamma_1 B_i \\ & + \delta_5 (IB_i \times P_i) + \delta_6 (IB_i \times G_i) + \gamma_2 (IB_i \times B_i) + \gamma_3 (IB_i \times B_i \times P_i) \\ & + \delta_7 (OB_i \times P_i) + \delta_8 (OB_i \times G_i) + \gamma_4 (OB_i \times B_i) + \gamma_5 (OB_i \times B_i \times P_i) \\ & + \delta_9 (P_i \times G_i) + \delta_{10} U_i + X_i' \beta + \kappa_c \big), \end{aligned} \quad (1)$$

where $\Lambda(\cdot)$ is the logistic CDF; IB_i and OB_i indicate borrower- and servicer-initiated first contact in April 2020; P_i indicates a performing loan; G_i indicates a federally backed loan; B_i indicates a Black borrower; U_i collects the unemployment and income curtailment indicators; X_i contains origination LTV, origination FICO, current balance, current rate, rate type, and corporate advances; and κ_c are county fixed effects absorbing local variation in pandemic severity, shutdown timing, and labor market exposure.

The parameters of interest are γ_1 through γ_5 . The level term γ_1 captures the unconditional racial difference. The interactions γ_2 and γ_3 capture differences on the borrower-initiated margin — the request channel. The interactions γ_4 and γ_5 capture differences on the servicer-initiated margin — the channel where servicer discretion operates most directly, and where the CARES Act imposed a mandate on the government side but not on the private-label side.

I estimate equation (1) separately on the full sample, the government subsample, and the

private-label subsample, with and without county fixed effects. Standard errors are clustered at the county level throughout, and every table note reports the clustering level.

Reporting. Logit coefficients are not directly interpretable as effect sizes, and the paper’s substantive claims are about magnitudes. I therefore report linear probability estimates alongside the logit and Firth coefficients throughout; the LPM coefficients are marginal effects in probability units and are the quantities quoted in the text.

4.2 Difference-in-differences

The cross-sectional specification conditions on contact, which is itself an outcome. To isolate the effect of the statutory entitlement, I estimate a difference-in-differences model on the monthly panel, treating federally backed loans as treated and private-label loans as control, with the CARES Act enactment at the end of March 2020 as the treatment date:

$$F_{it} = \alpha + \beta (G_i \times \text{Post}_t) + \lambda G_i + \tau_t + X'_{it}\theta + \kappa_c + \varepsilon_{it}, \quad (2)$$

where Post_t equals one for April and May 2020, τ_t are month effects, and remaining terms are as above.

Parallel trends. Identification requires that forbearance discussion rates in the two pools would have evolved similarly absent the Act. The pre-period runs from January through March 2020, giving three pre-treatment months.

To test whether the racial gap itself differs across the eligibility boundary, I estimate the triple difference

$$F_{it} = \alpha + \beta_3(G_i \times \text{Post}_t \times B_i) + \beta'_2 Z_{it} + \tau_t + X'_{it}\theta + \kappa_c + \varepsilon_{it}, \quad (3)$$

where Z_{it} collects the three two-way interactions and the level terms. The parameter β_3 is the paper’s primary estimand for the racial question: it measures how much the Black–White forbearance gap changed for federally backed borrowers, relative to private-label borrowers, after enactment. A discretion-generates-disparity hypothesis predicts $\beta_3 > 0$.

What the DiD does and does not identify. Equation (2) identifies the effect of statutory eligibility on forbearance discussion. It does *not* identify the racial gap. The racial comparison in this paper is a within-private-label, cross-sectional comparison, and I report it as a conditional association rather than a causal effect.

4.3 Interpretation

The estimand is a difference in forbearance outcomes between Black and White borrowers, conditional on loan observables and county, estimated separately within each regime and differenced across them.

The design supports a statement about the *relative* performance of mandated and delegated relief. Both regimes are administered by the same servicer, measured the same way, over the same weeks, with the same call routing and staffing, so features that would bias a levels comparison — measurement error in the race variable, coarseness in a binary forbearance indicator — apply equally to each regime and difference out of the triple-difference estimate.² This is the comparison the policy question requires.

I use “racial disparity” throughout rather than “disparate treatment” or “taste-based discrimination.” Both are terms of art, the first legal and the second economic, with evidentiary requirements this design does not meet in either direction.

5 Results

5.1 Communication and forbearance

Table 3 reports estimates of equation (1) without race interactions, for the full sample with and without county fixed effects and for the two subsamples separately. Logit and linear probability estimates appear side by side; where the maximum-likelihood logit separates, the coefficient is suppressed rather than printed.

The dominant regularity is that forbearance follows *borrower-initiated* contact. The interaction of inbound contact with performing status is positive and significant at the 1% level in every linear probability specification — 6.1 percentage points in the pooled sample with county fixed effects, 7.1 points among federally backed loans, and 4.9 points among private-label loans — and positive in every logit specification. A performing borrower who called the servicer was substantially more likely to end up in forbearance, on both sides of the eligibility boundary.

Servicer-initiated contact does not work the same way, and this is where the estimates depart from what a simple informational story would predict. The interaction of outbound contact with performing status is small and insignificant in every column. In the federally

²Section 3.3 reports the merge rate and the ancestry crosswalk. Classical measurement error in a binary regressor attenuates toward zero, so the racial estimates are best read as bounds on any larger underlying disparity rather than as point estimates of a true gap. A disparity operating through a margin not observed here — the terms offered, or the speed of resolution, rather than whether a plan was opened — would likewise not register.

backed subsample, outbound contact enters *negatively* and significantly on its own (-2.6 percentage points, $p < 0.01$), and the interaction of outbound contact with federally backed status is negative in the pooled sample (-4.7 points, $p < 0.01$).

The obvious explanation is selection: servicers call borrowers who are already in difficulty, and those borrowers are poor candidates for forbearance. Table 5 shows the first half of that story holds emphatically. Federally backed borrowers who received outbound contact in April 2020 were far more distressed on current measures — a delinquency bucket of 3.23 against 1.81, a performing share of 28.5% against 67.4%, and materially higher corporate advances — while being statistically indistinguishable on origination FICO, origination LTV, and note rate. Outbound contact is targeted at current distress, not at borrower type.

The second half does not follow. Adding delinquency-bucket fixed effects to the specification does not attenuate the negative coefficient; it moves from -0.051 to -0.065 . Selection on observed delinquency depth therefore does not account for the result, and I cannot claim it does.

Two readings remain. Servicers may select on information absent from the servicing record — a judgment about who is unlikely to self-cure — in which case the coefficient is still selection, merely unobserved. Or outbound contact may genuinely route borrowers toward workouts other than forbearance. The evidence favors the second.

Outcome composition. Table 6 decomposes the transcript flags by contact direction. Among federally backed loans, outbound contact is associated with less forbearance than inbound contact (8.3% versus 13.7%, $p < 0.001$), less COVID assistance of any kind (4.2% versus 8.0%), and a lower rate of forbearance letters actually being issued (0.8% versus 2.1%). It is associated with *more* foreclosure-moratorium activity (20.6% versus 16.9%, $p = 0.007$). Conditional on outbound contact the odds of a moratorium flag are 2.7 times those without it. Outbound contact is not simply less productive than inbound contact; it is associated with a different destination.

The pattern is not explained by borrower intent. If outbound-contacted borrowers were simply giving up on their homes, the moratorium track would follow mechanically. They are not: they state an intent to retain the property at a *higher* rate than borrowers who call in (39.0% versus 31.8%, $p < 0.001$), and stated intent to liquidate is statistically indistinguishable across the two groups (1.6% versus 1.1%, $p = 0.190$). They are, however, markedly more likely to refuse to state a reason for default (18.1% versus 13.3%, $p < 0.001$), which is consistent with reluctant engagement rather than with a considered decision to exit.

The servicer’s own process document. The COVID-19 forbearance letter this servicer issued in April 2020 opens by reciting that the borrower “notified” the servicer of pandemic impact, and places the loan into forbearance on that basis. Enrollment is documented as

borrower-initiated. The same letter states that the servicer will reach out during the final month of the forbearance period to assess the borrower’s ability to resume payment. In the servicer’s documented workflow, outbound contact is positioned at *exit* and status review, not at enrollment — which is what the regression coefficient reflects.

None of this is dispositive. The comparisons are associational and the transcript flags are contemporaneous rather than sequenced, so a servicer selecting on private information would generate much the same pattern. But routing is the simpler reading, and it is the one the servicer’s own workflow describes.

This qualifies any claim that servicer outreach was the vehicle through which the statute delivered relief. It does not bear on the racial estimates, which are the paper’s subject and which are estimated separately by contact direction.

That has a bearing on how the eligibility effect in Section 5.2 should be understood. If statutory eligibility raised forbearance by 3.6 percentage points but servicer-initiated contact did not deliver it, the mechanism most consistent with the evidence is that eligibility changed what happened when borrowers asked — an entitlement granted on attestation rather than a discretionary review — rather than prompting servicers to reach out more effectively.

The raw transcripts make the two tracks visible directly. Table 7 reports, for the 1,079 private-label loans with communication logs in April through June 2020, the share whose log contains each term, by forbearance status. Non-recipients’ logs concern the foreclosure track: “foreclosure” appears in 26.5% of them against 8.3% for recipients, “attorney” in 18.8% against 4.2%, “moratorium” in 13.8% against 1.7%. They also concern documentation — “document” at 21.2% against 9.2%, “affidavit” at 6.6% against 0.8% — which is the verification margin in the servicer’s own words. Recipients’ logs are about the forbearance request and little else. What discretion consisted of, in this window, was routing: borrowers who did not obtain forbearance were in conversations about foreclosure and paperwork, and borrowers who did were not. A companion study follows both groups past this window and finds the tracks led where the vocabulary points.

Table 4 splits the sample by loan purpose. Cash-out refinances are a majority of the private-label pool in this portfolio: 11,613 of the 12,499 private-label loan-months in April 2020. They are distressed loans acquired at deep discount in 2017 whose borrowers have nonetheless largely performed since acquisition, and they are unusual enough that pooling them without comment would let a narrow subportfolio drive the private-label results. The inbound-times-performing pattern holds in both the cash-out and non-cash-out private-label samples.

Finally, reported income curtailment is a stronger predictor of forbearance than reported unemployment. Binary unemployment flags, which are what most datasets offer, understate

liquidity distress relative to a graded income measure.

5.2 The effect of statutory eligibility

Table 14 reports estimates of equation (2). Statutory eligibility raised the probability of a forbearance outcome by 3.58 percentage points (s.e. 0.26) on the full panel of 96,201 loan-months, with county and month fixed effects and standard errors clustered by county. Restricting to borrower-initiated contacts gives 3.43 points (s.e. 0.80); to servicer-initiated contacts, 1.79 points (s.e. 0.45); to performing loans, 4.40 points (s.e. 0.36). Against an unconditional April forbearance rate of 6.7%, these are large effects.

Parallel trends, and why the conventional test does not apply here. Figure 3 plots the event-study coefficients. The estimated interactions are -0.0006 in January and $+0.0001$ in March, against $+0.0678$ in April. A joint test of the pre-treatment interactions rejects at conventional levels, and I report that rather than suppress it, but the rejection is mechanical rather than substantive. COVID-19 forbearance did not exist before the CARES Act: the outcome is identically zero in 38,748 January and February loan-months. A pre-trend test on an outcome with no pre-treatment variation is degenerate, and the microscopic coefficients that generate the rejection — two orders of magnitude below the treatment effect — reflect near-zero standard errors rather than differential trends.

The honest statement is that parallel trends cannot be tested in this setting, and that identification rests instead on the two groups being comparable in their response to the pandemic shock. Section 6 discusses what that assumption requires and where it is vulnerable. Readers who find it unpersuasive should treat the difference-in-differences estimates as descriptive of the divergence between the two pools after enactment.

5.3 Racial neutrality within and across regimes

5.3.1 Unconditional differences

Tables 9 and 10 report forbearance rates by loan type, performance status, and race for April 2020, with Wilson confidence intervals and Fisher exact tests on the White–Black comparison.

Among performing borrowers, the White–Black difference following servicer-initiated contact is $+3.8$ percentage points for federally backed loans ($p = 0.329$) and -10.0 points for private-label loans ($p = 0.155$). Following borrower-initiated contact the differences are $+6.9$ points ($p = 0.180$) and -0.5 points ($p = 1.000$). No comparison reaches significance. The private-label differences are negative or zero — Black borrowers obtained forbearance at rates

at least as high as White borrowers in the segment where the servicer retained discretion — though the private-label cells excluding cash-out refinances hold only two forbearance events among sixteen to twenty Black borrowers, and those cells cannot support inference in either direction.

Hispanic borrowers are reported in the tables for completeness. Their cells are adequately sized in the pooled samples but thin once split by contact channel and performance status, and I draw no conclusions from them.

5.3.2 Conditional estimates

Table 13 adds the Black indicator and its interactions to equation (1). The coefficient of interest, $\text{outbound} \times \text{performing} \times \text{Black}$, is -0.524 in the pooled Firth estimate with a profile interval of $[-1.32, 0.27]$, and -0.808 among private-label loans with interval $[-1.97, 0.36]$. The corresponding linear probability estimates, which are directly interpretable as marginal effects and immune to the numerical problems discussed below, are -2.9 percentage points ($p = 0.214$) and -4.0 points ($p = 0.118$).

I report logit, Firth penalized-likelihood, and linear probability estimates side by side. The three agree in sign and in significance throughout, with one exception worth flagging. In the federally backed subsample the maximum-likelihood logit returns a standard error of 242.9, indicating near-separation, and the Firth profile interval for that cell was unstable across estimation runs. I do not rely on it. The linear probability estimate for the same cell (-1.4 points, $p = 0.759$) and the unconditional test ($+0.7$ points, $p = 1.000$) are both clean nulls and are what I report.

5.3.3 The triple difference

The specification that answers the paper’s question is equation (3), which asks whether the racial gap differs across the eligibility boundary after enactment. The coefficient on $\text{federally backed} \times \text{post} \times \text{Black}$ is -0.0058 (s.e. 0.0068, $p = 0.392$), with a 95% confidence interval of $[-1.91, +0.75]$ percentage points (Table 11).

The interval excludes differential racial gaps larger than roughly two points in either direction, and therefore excludes the double-digit disparities that a discretion-generates-disparity hypothesis would predict. It does not exclude a modest gap, and the point estimate is negative — if anything, the mandated regime narrowed relative racial differences less than the discretionary one. On the subsample for which the vendor supplies the decoded group directly, the estimate is -1.24 points ($p = 0.072$), so the result is sensitive to race measurement at the margin and should be read as a bound rather than a point null.

5.3.4 Statistical power

A null result is only informative if the design could have detected the effect in question. Table 12 reports minimum detectable effects at 80% power and 5% significance. In the private-label performing sample, the outbound test detects a gap of 4.5 percentage points (196 Black and 556 White borrowers) and the inbound test 6.0 points (185 and 486). The federally backed tests are weaker, detecting 8.5 and 11.5 points, because those cells are smaller.

The private-label tests — the ones that matter, since that is where discretion operated — are therefore well powered against the disparities the fair-lending literature typically reports at origination, which are commonly in the range of five to fifteen percentage points. They found nothing.

The point estimates are not exactly zero, and nothing here rules out small disparities. What the intervals exclude are gaps of the magnitude that would matter for policy.

5.4 Requests versus approvals

An et al. (2022) observe that minority borrowers exhibited higher nonpayment but not correspondingly higher forbearance uptake, and note that regressions of forbearance on borrower characteristics cannot separate

$$\Pr(F | X) = \Pr(\text{Request} | X) \times \Pr(\text{Approval} | \text{Request}, X). \quad (4)$$

The transcripts speak to the first term directly: the contact-direction flags identify who raised forbearance and in which direction the conversation ran. The estimates in Section 5.3 are reported separately by contact channel for this reason, and the racial null holds on both — borrower-initiated and servicer-initiated alike. Whatever suppressed private-label take-up operated on both margins and did so without racial sorting.

The second term cannot be estimated here. The servicing extract available for this study records forbearance conversations, not executed plans, so $\Pr(\text{Approval} | \text{Request})$ is not separately identified. What happened to borrowers after the conversation — whether forbearance led to modification or to foreclosure — is the subject of a companion study using the servicer’s loss-mitigation records through February 2021.

5.5 Validating the text measures against raw transcripts

The flags used throughout are derived from keyword dictionaries applied to the servicer’s communication log, and a natural objection is that the dictionaries themselves could be race-

correlated: if representatives describe Black and White borrowers in systematically different language, a text-derived flag could generate a racial pattern mechanically. Raw transcript text for March–May 2020 is available for 5,302 panel loans, of which 3,767 have both a race classification and an April outcome (757 Black, 3,010 White, 288 with a forbearance conversation).

Table 8 reports, for each dictionary term, the share of Black and White borrowers whose transcripts contain it, holding the forbearance outcome fixed. Across 56 term-by-outcome comparisons, six differ by race at the 5% level against 2.8 expected by chance, and one at the 1% level. The pattern of the differences matters more than their count. Terms describing borrower *circumstances* — marital difficulty, death, casualty loss, unemployment, income — do not differ by race in any comparison. The differences that do appear all concern the *contact process*: “no contact” appears in 49% of transcripts for Black borrowers who obtained forbearance against 33% for White ($p = 0.029$), and “outbound” is more frequent in Black borrowers’ records generally (19% versus 15%, $p = 0.010$).

This is not representatives characterizing borrowers differently. It is the servicer attempting outbound contact more often with Black borrowers and recording failure to reach them more often — a difference in the contact process rather than in its description, and one consistent with the outbound routing evidence in Section 5.1. The dictionaries are neutral on the dimension that would threaten the racial estimates. The contact process is not symmetric, and that asymmetry is itself a finding.

Two caveats. The transcript sample is drawn from loans that subsequently cured and is not representative of the portfolio. And the check validates the flags’ neutrality; it does not by itself identify why outbound attempts reached Black borrowers less often.

5.6 Robustness

The results are stable across the specification choices that matter.

Estimator. Logit, Firth penalized likelihood, and linear probability estimates agree in sign and significance for every race interaction (Table 13), with the federally backed exception noted in Section 5.3.

Separation. Numerical separation, which rendered earlier estimates on this portfolio uninterpretable, is diagnosed explicitly rather than assumed away. Where the maximum-likelihood logit misbehaves I report Firth profile intervals and linear probability estimates, and I base no claim on a separated coefficient.

Sparse geography. Counties contributing fewer than five loan-months are dropped from the main specifications; including them does not change the racial estimates, though it does

destabilize the logit.

Fixed-effects level. County fixed effects are used throughout, with state fixed effects substituted where the parameter count makes penalized likelihood infeasible. The substitution does not move the race interactions materially.

Race definition. Race is assigned from the household ancestry codes using the crosswalk described in Section 3.3. Defining White as Western European ancestry only, rather than all European-origin groups, changes the White sample by roughly a tenth and leaves the estimates substantively unchanged.

Cash-out refinances. These account for a majority of the private-label pool and are reported both included and excluded. Excluding them shrinks the private-label performing cells to fewer than twenty Black borrowers, at which point no inference is possible in either direction; the adequately powered tests reported in Section 5.3 therefore use the full sample.

6 Limitations and External Validity

The estimates describe one servicer’s book over ten weeks, and their scope should be read accordingly.

One servicer, one portfolio. The sample is a single servicer’s book, concentrated in Ginnie Mae early buyouts and pre-2008 private-label subprime loans. Early buyout pools are adversely selected by construction. The portfolio is not representative of the national mortgage market, and the estimated magnitudes should not be extrapolated to it. The single-servicer design purchases internal validity — no unobserved servicer heterogeneity — at the cost of external validity. Whether other servicers exercised discretion similarly is an open question that the data cannot answer.

Treatment and control differ. Government and private-label loans in this portfolio differ in vintage, origination LTV, balance, and delinquency history. Both pools contain distressed credit, which is the basis for comparison, but they are not balanced.

Forbearance is measured through conversation. F_i records a conversation about forbearance rather than an executed plan (Section 3.2). The executed-plan field was substantially incomplete in April and May 2020; it is observable retrospectively now, and re-running the analysis on it would be the single most useful extension.

Race is measured by external merge. The vendor field is an ancestry taxonomy mapped to race through the vendor’s own rollup, matching 90.3% of loan-months (94.0% federally backed, 88.3% private-label). Classical measurement error attenuates toward zero, which for a null result cuts against the paper’s conclusion rather than for it.

Parallel trends cannot be verified. As discussed in Section 5.2, the outcome does not

exist before treatment, so the pre-period carries no information about differential trends. The difference-in-differences estimates therefore rest on an untestable assumption that the two pools would have responded similarly to the pandemic absent the statute. Given that the pools differ in vintage, LTV, and balance, this is the paper’s most vulnerable assumption, and readers should weight the estimates accordingly.

Bounds, not point nulls. The confidence intervals exclude disparities above roughly one to six percentage points depending on specification. They do not exclude smaller ones, and they speak to this servicer and this window. The finding is that discretion *can* be exercised without measurable racial sorting, not that it always is.

Finally, the analysis window is April–May 2020, weeks after enactment. This is a strength for identification — borrower anticipation is minimal and the results are not contaminated by later program modifications, extensions, or the eventual exit waves — and a limitation for external validity, since servicer behavior plausibly changed as guidance matured and as regulatory scrutiny of pandemic servicing intensified.

7 Discussion and Policy Implications

The results speak to a design choice that recurs whenever household relief is delivered through private intermediaries, and that is being actively relitigated in mortgage servicing.

7.1 Coverage, not treatment

The CARES Act did not discriminate, and neither — on this evidence — did the servicer administering the segment Congress left uncovered. What the statute did was decline to legislate for a class of borrowers, and the measured consequence was 3.6 percentage points less relief for that class.

This is a distributional consequence, but of a particular kind. It operates through who is covered rather than through how covered borrowers are treated, and the two call for different remedies. Fair-lending supervision targets differential treatment within a regime; it does not reach a gap generated by the perimeter of the regime itself. A relief statute can be perfectly neutral in its administration and still allocate relief unequally across the population, if eligibility correlates with borrower characteristics — as private-label status does.

7.2 Prescription versus flexibility in loss mitigation

This is the live question. The current rulemaking direction — simplifying loss mitigation requirements, restoring servicer flexibility in structuring workouts, and rescinding the COVID-era procedural safeguards — rests on a premise that prescriptive process rules impose compliance costs disproportionate to the borrower protection they deliver. That premise may well be right on average. The evidence here bears on its distributional tail: the operative difference between the two regimes in 2020 was not whether forbearance existed but whether the servicer could require proof of hardship, and that single margin of discretion is where the disparity appeared.

The proposal’s own rationale runs partly through take-up: the Bureau observes that investors have increasingly favored loss mitigation options requiring little or no documentation, and that streamlined options can raise the rate at which borrowers resume payment while lowering servicer costs. The estimates here are consistent with that reasoning and put a number on it. In this portfolio, the discretionary regime — whose operative feature was a documentation requirement — delivered 3.6 percentage points less forbearance than the mandated one.

The evidence here bears on the magnitude of that trade-off rather than on its fairness. Attestation-based eligibility eliminates screening at the acknowledged cost of extending relief to borrowers who did not need it; documentation-based eligibility restores screening at a cost of 3.6 percentage points of take-up. In this episode the documentation standard did not sort borrowers by race — it suppressed take-up among all of them. Two measures follow, the first now more important than the second:

1. **Specify what documentation suffices.** An enumerated safe harbor for hardship evidence preserves the servicer’s ability to screen while removing the case-by-case latitude through which uneven application operates. This is achievable within a streamlined framework and does not reintroduce the procedural complexity the rulemaking seeks to remove.
2. **Require demographic reporting on loss mitigation outcomes.** Servicers need not collect race data for this to work: regulators can perform the merge, as is already routine for HMDA-based fair lending analysis. Reporting approval rates by group at the loss mitigation stage would make disparities of the kind documented here visible without imposing a collection burden or a substantive mandate.

7.3 The finding applies to the framework now in force

Nothing in the mechanism is pandemic-specific, and the institutional arrangement it describes has outlived the emergency that produced it. As Section 2.3 sets out, FHA and VA now operate permanent loss mitigation waterfalls that specify relief and its documentary preconditions for federally backed borrowers, while borrowers outside those channels continue to depend on servicer discretion. Forbearance has also become the standard first-line workout following federally declared disasters, with the same coverage architecture and, in the VA framework, an explicit disaster modification that dispenses with a complete application. Given the frequency of disaster declarations and the concentration of distress in government-backed portfolios (Mortgage Bankers Association, 2026), the population exposed to the discretionary regime is not shrinking.

The estimate in Section 5.2 is therefore a parameter of the current system rather than a historical curiosity: it says that in one large portfolio, moving from a documented, discretionary regime to an attestation-based entitlement raised forbearance by 3.6 percentage points. The estimate in Section 5.3 says the documented regime did not allocate that shortfall unevenly by race.

7.4 Delegated implementation

More generally, the episode illustrates the cost of implementing household relief through private intermediaries under time pressure. The design question is not whether to use intermediaries — there was no alternative in March 2020 — but what reporting and standardization obligations should attach when they are used, and whether those obligations should be calibrated to the segment where the statute itself provides no floor.

8 Conclusion

The CARES Act made mortgage forbearance a right for borrowers with federally backed loans and left it a servicer decision for everyone else. Using a servicer whose book spans both sides of that boundary, and the servicer’s own call transcripts, I ask what delegation did.

It reduced relief. Statutory eligibility raised forbearance by 3.6 percentage points, and the effect holds across contact channels and performance status. Borrowers outside the perimeter received substantially less of the relief the pandemic made necessary, not because their circumstances differed but because their loans did.

It did not, however, distribute that shortfall unevenly by race. The triple-difference estimate of the differential gap is -0.58 percentage points with a confidence interval of $[-1.91, 0.75]$, and the private-label tests detect gaps of four to six points. Disparities of the magnitude the fair-lending literature reports at origination are ruled out; a modest one is not.

The transcripts indicate why the two results are consistent. Relief ran through borrowers who initiated contact, not through servicer outreach, and servicer discretion operated through a documentation standard rather than a grant-or-deny decision: outbound conversations with private-label borrowers concerned verification, ineligibility, and referral to alternative workouts. A documentation requirement suppresses take-up among everyone subject to it. It is a barrier of uniform height, and in this portfolio it appears to have been applied without racial sorting.

The policy implication differs from the one a disparity finding would have supported. The problem at this boundary was not unequal treatment requiring a fair-lending remedy; it was unequal coverage requiring a coverage remedy. As loss mitigation rules are rewritten to restore servicer flexibility, the relevant question is not primarily whether discretion will be exercised unevenly across groups — in this episode it was not — but how much relief the discretionary regime delivers at all relative to a mandated one, and whether the borrowers left in that regime are the ones least able to absorb the difference.

These conclusions bear on a standing arrangement rather than a closed episode. The permanent FHA waterfall that took effect in October 2025 and the VA framework phased in during 2026 reproduce the CARES design: defined relief on a defined documentary showing, for federally backed borrowers only. Distress is currently concentrated in exactly those portfolios. The parameter estimated here — what the documentary margin costs in take-up — is a parameter of the system now in operation.

Two limits deserve emphasis. The estimates come from one servicer’s distressed book and should not be read as national. And a null in this portfolio and this ten-week window is not a general finding about servicer conduct; it is evidence that discretion *can* be exercised neutrally, not that it always is. The second limit cuts both ways: it also means the 2020 result cannot be assumed to hold under the waterfalls now in force, whose documentary requirements differ from those the servicer applied in April 2020. Whether the permanent frameworks allocate relief as evenly is an open question, and one the supervisory reporting proposed in Section 7 would answer.

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Table 1: Summary Statistics, April 2020

Panel A reports all loans; Panel B federally backed (FHA/VA/USDA) loans; Panel C private-label loans. Statistics are as of the April 2020 servicer reporting date.

PANEL A: All Loans	count	mean	sd	median	min	max	range	skew	kurtosis
Covid_Forbearance_Flag	19159	6.74%	0.251	0	0	1	1	3.451	9.911
Unemployed_Flag	19159	3.48%	0.183	0	0	1	1	5.079	23.8
Prior_Unemployment_Flag	19159	0.64%	0.08	0	0	1	1	12.359	150.755
FC_Moratorium_Flag	19159	6.74%	0.251	0	0	1	1	3.451	9.911
Curtailment_of_Income_Flag	19159	4.86%	0.215	0	0	1	1	4.198	15.628
Inbound_Borrower_Flag	19159	22.31%	0.416	0	0	1	1	1.33	-0.23
Outbound_Borrower_Flag	19159	29.38%	0.455	0	0	1	1	0.906	-1.18
DIq	19159	1.429	2.049	0	0	5	5	0.987	-0.837
original_balance	19159	96791.008	84115.159	70947.24	1972.51	747750	745777.49	2.353	8.299
original_appraisal	19159	119747.758	97459.965	90000	3122.24	978200	975077.76	2.821	11.993
original_fico	19159	612.567	67.522	613	372	847	475	0.244	-0.13
current_rate	19159	0.066	0.029	0.058	0	0.197	0.197	0.427	-1.026
orig_ltv	19159	80.504	23.657	88.585	2.704	139.349	136.645	-1.082	0.527
log(current_balance)	19159	10.801	1.08	10.898	0.01	13.584	13.574	-0.692	1.589
Gov	19159	0.348	0.476	0	0	1	1	0.64	-1.591
corporate_adv	19159	6.897	1.767	7.174	0.482	12.197	11.715	-1.34	3.213
rem_term	19159	190.817	112.797	202	6	526	520	0.061	-1.032
mod_flag	19159	0.354	0.478	0	0	1	1	0.611	-1.627
N	19159								
PANEL B: Government Loans									
Covid_Forbearance_Flag	6660	11.13%	0.31	0	0	1	1	2.47	4.11
Unemployed_Flag	6660	5.23%	0.22	0	0	1	1	4.02	14.19
Prior_Unemployment_Flag	6660	1.07%	0.1	0	0	1	1	9.53	88.79
FC_Moratorium_Flag	6660	12.57%	0.33	0	0	1	1	2.26	3.1
Curtailment_of_Income_Flag	6660	7.76%	0.27	0	0	1	1	3.16	7.96
Inbound_Borrower_Flag	6660	32.43%	0.47	0	0	1	1	0.75	-1.44
Outbound_Borrower_Flag	6660	41.80%	0.49	0	0	1	1	0.33	-1.89
DIq	6660	2.26	2.17	1	0	5	5	0.25	-1.73
original_balance	6660	142333.67	78248.12	126424	21825	730987	709162	1.78	5.35
original_appraisal	6660	149441.5	84418.46	131000	23000	890000	867000	1.96	6.66
original_fico	6660	617.38	65.13	628	376	813	437	-0.02	-0.21
current_rate	6660	0.04	0.01	0.04	0.01	0.1	0.09	1.76	4.47
orig_ltv	6660	95.92	7.55	98.11	9.97	130.77	120.79	-2.83	13.79
log(current_balance)	6660	11.51	0.71	11.59	0.01	13.41	13.4	-1.8	15.37
corporate_adv	6660	5.95	2.41	6.21	0.48	11.47	10.99	-0.58	-0.07
rem_term	6660	274.79	61.87	283	7	446	439	-1.44	2.72
mod_flag	6660	0.69	0.46	1	0	1	1	-0.83	-1.31
N	6660								
PANEL C: Non-Government Loans									
Covid_Forbearance_Flag	12499	4.40%	0.21	0	0	1	1	4.45	17.77
Unemployed_Flag	12499	2.54%	0.16	0	0	1	1	6.03	34.33
Prior_Unemployment_Flag	12499	0.42%	0.06	0	0	1	1	15.4	235.33
FC_Moratorium_Flag	12499	3.63%	0.19	0	0	1	1	4.96	22.56
Curtailment_of_Income_Flag	12499	3.31%	0.18	0	0	1	1	5.22	25.22
Inbound_Borrower_Flag	12499	16.91%	0.37	0	0	1	1	1.77	1.12
Outbound_Borrower_Flag	12499	22.75%	0.42	0	0	1	1	1.3	-0.31
DIq	12499	0.99	1.83	0	0	5	5	1.56	0.65
original_balance	12499	72523.93	76741.5	50993.05	1972.51	747750	745777.49	3.63	17.55
original_appraisal	12499	103925.67	100213.71	74000	3122.24	978200	975077.76	3.48	16.07
original_fico	12499	610	68.63	605	372	847	475	0.38	-0.05
current_rate	12499	0.08	0.03	0.08	0	0.2	0.2	-0.29	-0.69
orig_ltv	12499	72.29	25.17	77.54	2.7	139.35	136.65	-0.53	-0.28
log(current_balance)	12499	10.42	1.05	10.47	3	13.58	10.58	-0.38	1.48
corporate_adv	12499	7.4	0.98	7.37	5.02	12.2	7.18	0.74	0.9
rem_term	12499	146.07	108.18	125	6	526	520	0.91	0.31
mod_flag	12499	0.17	0.38	0	0	1	1	1.72	0.96
N	12499								

Table 2: Summary Statistics by Loan Type and Performance Status

Panels separate performing (fewer than 60 days delinquent) from non-performing loans within the federally backed and private-label pools.

PANEL A: Performing and Govt-backed loans									
	count	mean	sd	median	min	max	range	skew	kurtosis
Covid_Forbearance_Flag	3657	13.18%	0.34	0	0	1	1	2.18	2.74
Unemployed_Flag	3657	1.48%	0.12	0	0	1	1	8.04	62.7
Prior_Unemployment_Flag	3657	0.57%	0.08	0	0	1	1	13.08	169.05
FC_Moratorium_Flag	3657	0.16%	0.04	0	0	1	1	24.62	604.17
Curtailment_of_Income_Flag	3657	4.73%	0.21	0	0	1	1	4.26	16.18
Inbound_Borrower_Flag	3657	24.99%	0.43	0	0	1	1	1.15	-0.67
Outbound_Borrower_Flag	3657	27.89%	0.45	0	0	1	1	0.99	-1.03
Dlq	3657	0.53	1.03	0	0	5	5	3.26	11.37
original_balance	3657	134613.71	74617.33	118907	21825	653015	631190	1.73	4.72
original_appraisal	3657	141636.78	81805.48	125000	23000	890000	867000	2.06	7.35
original_fico	3657	615.57	66.86	625	376	813	437	0.04	-0.27
current_rate	3657	0.04	0.01	0.04	0.02	0.1	0.08	1.93	6.45
orig_ltv	3657	95.89	7.71	98.16	9.97	124.39	114.42	-3.07	16.2
current_balance	3657	110525.8	70592.05	96412.37	522.02	669748.08	669226.06	1.61	4.53
corporate_adv	3657	603.63	2370.6	187.44	0.62	95760.35	95759.73	25.17	853.19
rem_term	3657	271.36	66.4	281	9	446	437	-1.44	2.36
mod_flag	3657	0.73	0.44	1	0	1	1	-1.05	-0.89
N	3657								
PANEL B: Performing and Non-Govt-backed loans									
Covid_Forbearance_Flag	10157	4.54%	0.21	0	0	1	1	4.37	17.08
Unemployed_Flag	10157	1.19%	0.11	0	0	1	1	9	78.94
Prior_Unemployment_Flag	10157	0.30%	0.05	0	0	1	1	18.32	333.5
FC_Moratorium_Flag	10157	0.01%	0.01	0	0	1	1	100.75	10150
Curtailment_of_Income_Flag	10157	2.64%	0.16	0	0	1	1	5.91	32.92
Inbound_Borrower_Flag	10157	13.22%	0.34	0	0	1	1	2.17	2.71
Outbound_Borrower_Flag	10157	14.43%	0.35	0	0	1	1	2.02	2.1
Dlq	10157	0.2	0.77	0	0	5	5	5.33	29.86
original_balance	10157	65724.2	61534.64	49605.26	2025.39	709600	707574.61	3.57	19.04
original_appraisal	10157	94719.04	83717.5	70000	3122.24	978200	975077.76	3.61	18.49
original_fico	10157	613.93	68.78	610	402	847	445	0.35	-0.04
current_rate	10157	0.08	0.03	0.09	0	0.2	0.2	-0.4	-0.52
orig_ltv	10157	72.22	24.81	77.05	2.7	139.35	136.65	-0.47	-0.34
current_balance	10157	48789.55	56157.64	32970.4	19.14	699407.76	699388.62	3.65	20.23
Gov	10157	0	0	0	0	0	0	NA	NA
corporate_adv	10157	1867.42	2898.92	1404.94	150	99951.25	99801.25	14.74	364.16
rem_term	10157	140.73	105.79	119	6	494	488	0.96	0.45
mod_flag	10157	0.16	0.36	0	0	1	1	1.88	1.52
N	10157								
PANEL C: Non-Performing and Govt-backed loans									
Covid_Forbearance_Flag	3003	8.62%	0.28	0	0	1	1	2.95	6.68
Unemployed_Flag	3003	9.79%	0.3	0	0	1	1	2.7	5.32
Prior_Unemployment_Flag	3003	1.67%	0.13	0	0	1	1	7.55	55.04
FC_Moratorium_Flag	3003	27.67%	0.45	0	0	1	1	1	-1.01
Curtailment_of_Income_Flag	3003	11.46%	0.32	0	0	1	1	2.42	3.85
Inbound_Borrower_Flag	3003	41.49%	0.49	0	0	1	1	0.35	-1.88
Outbound_Borrower_Flag	3003	58.74%	0.49	1	0	1	1	-0.35	-1.87
Dlq	3003	4.37	1.02	5	2	5	3	-1.46	0.71
original_balance	3003	151734.91	81484.54	135695	24600	730987	706387	1.81	5.79
original_appraisal	3003	158945.96	86563.52	140500	25000	800000	775000	1.88	6.13
original_fico	3003	619.58	62.89	631	392	810	418	-0.11	-0.12
current_rate	3003	0.04	0.01	0.04	0.01	0.09	0.08	1.57	2.71
orig_ltv	3003	95.95	7.35	98.05	38.16	130.77	92.61	-2.5	10.12
current_balance	3003	136656.28	81399.43	121333.65	0.01	665499.18	665499.17	1.68	4.87
corporate_adv	3003	4969.19	8068.78	2128.66	7.03	89295.04	89288.01	4.02	23.74
rem_term	3003	278.97	55.59	284	7	418	411	-1.33	2.76
mod_flag	3003	0.64	0.48	1	0	1	1	-0.59	-1.65
N	3003								
PANEL D: Non-Performing and Non-Govt-backed loans									
Covid_Forbearance_Flag	2342	3.80%	0.19	0	0	1	1	4.83	21.33
Unemployed_Flag	2342	8.41%	0.28	0	0	1	1	2.99	6.97
Prior_Unemployment_Flag	2342	0.94%	0.1	0	0	1	1	10.17	101.37
FC_Moratorium_Flag	2342	19.34%	0.4	0	0	1	1	1.55	0.41
Curtailment_of_Income_Flag	2342	6.23%	0.24	0	0	1	1	3.62	11.1
Inbound_Borrower_Flag	2342	32.92%	0.47	0	0	1	1	0.73	-1.47
Outbound_Borrower_Flag	2342	58.84%	0.49	1	0	1	1	-0.36	-1.87
Dlq	2342	4.39	1.05	5	2	5	3	-1.48	0.64
original_balance	2342	102013.65	118082.73	59776.37	1972.51	747750	745777.49	2.57	7.06
original_appraisal	2342	143853.8	145765.45	89950	13070	975000	961930	2.5	7.11
original_fico	2342	592.96	65.29	585	372	845	473	0.51	0.03
current_rate	2342	0.07	0.03	0.07	0	0.15	0.15	0.13	-1.04
orig_ltv	2342	72.62	26.66	79.42	4.3	138.5	134.2	-0.74	-0.13
current_balance	2342	90729.69	115243.56	49559.65	45.29	793649.71	793604.42	2.56	7.04
corporate_adv	2342	8367.93	14368.58	4529.73	170	198219.24	198049.24	7.02	70.28
rem_term	2342	169.24	115.18	158	6	526	520	0.68	-0.16
mod_flag	2342	0.24	0.43	0	0	1	1	1.2	-0.57
N	2342								

Table 3: Forbearance and Servicer Communication

Estimates of equation (1) without race interactions, April 2020. Each sample reports a maximum-likelihood logit and a linear probability model; LPM coefficients are marginal effects in probability units. “sep.” marks logit coefficients affected by quasi-complete separation, which are not interpretable and are suppressed. Blank cells are terms absorbed by the subsample definition. Standard errors, clustered by county, in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Term	All loans: logit	All loans: LPM	All loans (county FE): logit	All loans (county FE): LPM	Federally backed: logit	Federally backed: LPM	Private-label: logit	Private-label: LPM
Inbound	0.330** (0.168)	-0.0031 (0.0106)	0.298* (0.170)	-0.0042 (0.0106)	sep.	0.0053 (0.0067)	0.179 (0.245)	0.0039 (0.0103)
Outbound	0.053 (0.167)	0.0013 (0.0078)	0.054 (0.160)	0.0009 (0.0078)	sep.	-0.0256*** (0.0059)	0.142 (0.236)	-0.0041 (0.0081)
Inbound × Performing	0.539*** (0.149)	0.0617*** (0.0122)	0.534*** (0.152)	0.0607*** (0.0123)	0.432 (0.172)	0.0711*** (0.0220)	0.653** (0.267)	0.0486*** (0.0137)
Outbound × Performing	-0.003 (0.164)	-0.0081 (0.0101)	-0.009 (0.164)	-0.0070 (0.0101)	(0.299)	0.0017 (0.0179)	-0.325 (0.272)	-0.0139 (0.0110)
Inbound × Fed. backed	-0.240 (0.148)	0.0180 (0.0133)	-0.184 (0.150)	0.0206 (0.0133)	0.089 (0.322)	0.0053 (0.0067)		
Outbound × Fed. backed	-0.489*** (0.167)	-0.0459*** (0.0109)	-0.521*** (0.171)	-0.0471*** (0.0110)		-0.0256*** (0.0059)		
Performing	0.346** (0.162)	0.0081 (0.0072)	0.335** (0.163)	0.0074 (0.0072)	sep.	-0.0008 (0.0063)	0.500** (0.227)	0.0177** (0.0075)
Federally backed	0.695*** (0.170)	0.0347*** (0.0099)	0.636*** (0.176)	0.0320*** (0.0101)				
Performing × Fed. backed	-0.265 (0.170)	0.0000 (0.0110)	-0.286 (0.175)	-0.0019 (0.0112)	sep.	-0.0008 (0.0063)		
Unemployed	-0.062 (0.157)	-0.0046 (0.0110)	-0.061 (0.158)	-0.0042 (0.0110)	sep.	-0.0202 (0.0171)	0.182 (0.236)	0.0093 (0.0150)
Income curtailment	0.486*** (0.114)	0.0461*** (0.0118)	0.466*** (0.116)	0.0440*** (0.0118)	0.426 (6.657)	0.0449*** (0.0173)	0.530*** (0.188)	0.0429*** (0.0160)
Observations	19,156		19,156				12,499	

Table 4: Forbearance by Loan Purpose

Equation (1) estimated separately by loan purpose and loan type, April 2020. Cash-out refinances are 11,613 of the 12,499 private-label loan-months and are reported separately so that this subportfolio does not silently drive the private-label results. Construction and conventions as in Table 3.

Term	Non-cash-out, fed. backed: logit	Non-cash-out, fed. backed: LPM	Non-cash-out, private: logit	Non-cash-out, private: LPM	Cash-out, private: logit	Cash-out, private: LPM
Inbound	sep.	0.0035 (0.0069)	-1.058 (1.031)	-0.0368 (0.0334)	0.387 (0.252)	0.0121 (0.0108)
Outbound	sep.	-0.0251*** (0.0061)	0.849 (0.627)	0.0446 (0.0293)	0.017 (0.266)	-0.0005 (0.0087)
Inbound × Performing	0.467 (4.836)	0.0727*** (0.0229)	1.849 (1.156)	0.1177* (0.0640)	0.467 (0.285)	0.0392*** (0.0147)
Outbound × Performing	0.036 (3.560)	-0.0106 (0.0178)	-2.077** (1.030)	-0.1277** (0.0497)	-0.126 (0.311)	-0.0058 (0.0120)
Inbound × Fed. backed	sep.	0.0035 (0.0069)				
Outbound × Fed. backed	sep.	-0.0251*** (0.0061)				
Performing	sep.	-0.0000 (0.0065)	1.328* (0.694)	0.0788** (0.0379)	0.428* (0.234)	0.0139* (0.0074)
Federally backed						
Performing × Fed. backed	sep.	-0.0000 (0.0065)				
Unemployed	-0.260 (0.229)	-0.0228 (0.0175)	-0.540 (0.854)	-0.0266 (0.0372)	0.279 (0.241)	0.0146 (0.0162)
Income curtailment	0.430 (3.949)	0.0452*** (0.0175)	0.520 (0.801)	0.0279 (0.0511)	0.526*** (0.195)	0.0443*** (0.0166)
Observations	6,243		886		11,613	

Table 5: Who Receives Servicer-Initiated Contact?

Balance on observables by outbound contact status, federally backed loans, April 2020.

	Contacted	Not contacted	Std. diff.	p
Delinquency bucket (0–5)	3.228	1.806	+0.703	<0.001
Share performing	0.285	0.674		<0.001
Log corporate advances	6.674	5.605	+0.470	<0.001
Log current balance	11.627	11.458	+0.246	<0.001
Origination FICO	617.5	617.3	+0.002	0.946
Origination LTV	95.8	96.0	−0.014	0.593
Current note rate	0.042	0.042	−0.016	0.547
Observations	2,135	4,525		

Federally backed loans, April 2020, by whether the servicer initiated contact. Borrowers who received outbound contact are sharply more distressed on current measures — delinquency, corporate advances, performing status — and indistinguishable on origination characteristics. Adding delinquency-bucket fixed effects to the specification does not attenuate the negative outbound coefficient, which moves from −0.051 to −0.065; observable selection on delinquency depth therefore does not account for it.

Table 6: Where Does Servicer-Initiated Contact Lead?

Transcript flag composition by direction of first contact, federally backed loans, April 2020.

Transcript flag	Outbound	Inbound	Neither	p (OB vs IB)
COVID forbearance discussed	8.29%	13.67%	11.66%	<0.001
COVID assistance discussed	4.22%	8.04%	0.85%	<0.001
Forbearance letter issued	0.80%	2.14%	0.95%	0.001
Foreclosure moratorium	20.56%	16.87%	8.35%	0.007
Reason for default refused	18.08%	13.31%	1.00%	<0.001
Stated intent to retain property	38.97%	31.81%	2.41%	<0.001
Stated intent to liquidate	1.64%	1.07%	0.10%	0.190
<i>Observations</i>	2,135	1,405	3,989	
Federally backed loans, April 2020, by direction of first contact. Cells are the share of loans in the column whose transcripts carry the flag; p -values are Fisher exact tests comparing outbound with inbound. Outbound contact is associated with <i>less</i> forbearance and <i>more</i> foreclosure-moratorium activity than inbound contact, while borrowers reached by outbound contact state an intent to retain the property at a higher rate than those who call in.				

Table 7: The Two Tracks in the Communication Log, April–June 2020

Term frequency in private-label servicer notes by forbearance status, entry window.

Term	Forbearance	No forbearance	Ratio	p
<i>Foreclosure track</i>				
foreclos	8.3%	26.5%	0.3×	0.000***
attorney	4.2%	18.8%	0.2×	0.000***
moratorium	1.7%	13.8%	0.1×	0.000***
referral	0.8%	5.5%	0.2×	0.024**
noi	2.5%	5.5%	0.5×	0.193
<i>Documentation / verification</i>				
document	9.2%	21.2%	0.4×	0.001***
affidavit	0.8%	6.6%	0.1×	0.007***
proof	1.7%	5.7%	0.3×	0.079*
verification	0.0%	3.0%	0.0×	0.065*
paystub	0.0%	2.4%	0.0×	0.099*
hardship	0.8%	2.4%	0.3×	0.506
<i>Exit vocabulary</i>				
loan mod	0.8%	11.1%	0.1×	0.000***
modification	0.8%	8.0%	0.1×	0.001***
reinstate	2.5%	11.5%	0.2×	0.001***
trial	3.3%	12.9%	0.3×	0.001***
deferral	0.8%	3.4%	0.2×	0.166
<i>Engagement</i>				
requested	97.5%	16.7%	5.8×	0.000***
inbound	33.3%	33.9%	1.0×	1.000

Share of private-label loans whose servicer communication log for April–June 2020 contains each term, by whether an April 2020 forbearance conversation was recorded. Transcripts are available for 1,079 private-label loans (120 with forbearance). Fisher exact p -values. Non-recipients’ logs concern the foreclosure track and documentation; recipients’ logs concern the forbearance request itself, and “requested” is near-universal among them because the forbearance indicator is built from it. Exit vocabulary is rare for both groups in this window; its later emergence is documented in the companion outcome study. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 8: Dictionary Neutrality: Term Frequency by Race, Holding Outcome Fixed
Share of loans whose March–May 2020 transcript contains each term, by borrower race and April forbearance outcome.

Dictionary	Term	Forb.	Black	White	Difference	p
outbound	no contact	1	0.491	0.330	+0.160**	0.029
outbound	outbound	1	0.200	0.116	+0.084	0.120
covid	pandemic	1	0.127	0.047	+0.080*	0.055
hardship	income	1	0.309	0.262	+0.047	0.502
outbound	offered	1	0.182	0.137	+0.044	0.400
covid	moratorium	1	0.145	0.103	+0.042	0.348
inbound	inbound	1	0.473	0.515	-0.042	0.653
outbound	outbound	0	0.194	0.153	+0.041***	0.010
inbound	excessive obligations	1	0.109	0.073	+0.036	0.406
inbound	received	1	0.255	0.227	+0.027	0.723
inbound	received	0	0.302	0.278	+0.024	0.205
outbound	no contact	0	0.376	0.398	-0.021	0.320
outbound	next due	1	0.000	0.021	-0.021	0.587
inbound	inbound	0	0.316	0.296	+0.021	0.289

Share of loans whose March–May 2020 transcript contains each term, by borrower race, holding the April forbearance outcome fixed. 56 term-by-outcome comparisons on 3,767 loans (757 Black, 3,010 White); 6 differ at 5% against 2.8 expected by chance, 1 at 1%. Fourteen largest absolute differences shown. Terms describing borrower circumstances do not differ by race; the differences that appear concern the contact process itself. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 9: Forbearance Following Borrower-Initiated Contact, by Loan Type, Performance Status, and Race

Non-cash-out loans, April 2020. Rates are forbearance discussions as a share of loans in the cell. Intervals are Wilson 95% confidence intervals. The Fisher exact test compares White and Black borrowers within performing loans. Hispanic cells are reported but are too small for inference.

Loan type	Status / race	Forbearance	Loans	Rate	95% CI
FHA/VA/USDA	Non-performing Black	10	122	8.2%	[4.5, 14.4]
	Hispanic	6	80	7.5%	[3.5, 15.4]
	White	56	566	9.9%	[7.7, 12.6]
	Performing Black	13	89	14.6%	[8.7, 23.4]
	Hispanic	15	66	22.7%	[14.3, 34.2]
	White	71	330	21.5%	[17.4, 26.3]
Private-label	Non-performing Black	0	12	0.0%	[0.0, 24.2]
	Hispanic	0	12	0.0%	[0.0, 24.2]
	White	2	50	4.0%	[1.1, 13.5]
	Performing Black	2	16	12.5%	[3.5, 36.0]
	Hispanic	3	8	37.5%	[13.7, 69.4]
	White	3	25	12.0%	[4.2, 30.0]

Wilson confidence intervals. FHA/VA/USDA performing: White–Black gap = 6.9 pp, Fisher exact $p = 0.180$ (13 events among 89 Black borrowers); Private-label performing: White–Black gap = -0.5 pp, Fisher exact $p = 1.000$ (2 events among 16 Black borrowers).

Table 10: Forbearance Following Servicer-Initiated Contact, by Loan Type, Performance Status, and Race

Non-cash-out loans, April 2020. Construction as in Table 9.

Loan type	Status / race	Forbearance	Loans	Rate	95% CI
FHA/VA/USDA	Non-performing Black	13	193	6.7%	[4.0, 11.2]
	Hispanic	10	152	6.6%	[3.6, 11.7]
	White	72	993	7.3%	[5.8, 9.0]
	Performing Black	6	87	6.9%	[3.2, 14.2]
	Hispanic	10	66	15.2%	[8.4, 25.7]
	White	41	382	10.7%	[8.0, 14.2]
Private-label	Non-performing Black	2	37	5.4%	[1.5, 17.7]
	Hispanic	2	23	8.7%	[2.4, 26.8]
	White	7	98	7.1%	[3.5, 14.0]
	Performing Black	2	20	10.0%	[2.8, 30.1]
	Hispanic	2	10	20.0%	[5.7, 51.0]
	White	0	30	0.0%	[0.0, 11.4]
Wilson confidence intervals. FHA/VA/USDA performing: White–Black gap = 3.8 pp, Fisher exact $p = 0.329$ (6 events among 87 Black borrowers); Private-label performing: White–Black gap = -10.0 pp, Fisher exact $p = 0.155$ (2 events among 20 Black borrowers).					

Table 11: Triple Difference: Does the Racial Gap Differ Across the Eligibility Boundary?

Estimates of equation (3). The coefficient of interest is federally backed $\times$ post $\times$ Black. County and month fixed effects; standard errors clustered by county.

Term	Coefficient	Std. error	95% CI	p
Gov $\times$ Post $\times$ Black	-0.0058	(0.0068)	[-0.0191, 0.0075]	0.392
Gov $\times$ Post	0.0372***	(0.0029)	[0.0315, 0.0428]	0.000
Gov $\times$ Black	-0.0009	(0.0010)	[-0.0028, 0.0010]	0.348
Post $\times$ Black	0.0002	(0.0030)	[-0.0057, 0.0060]	0.953
gov	-0.0115***	(0.0014)	[-0.0141, -0.0088]	0.000
post	0.0232***	(0.0015)	[0.0202, 0.0261]	0.000
black	-0.0003	(0.0009)	[-0.0020, 0.0014]	0.750
Observations	86,863			
Linear probability model. The coefficient of interest is Gov $\times$ Post $\times$ Black, which tests whether the racial gap in forbearance differs across the statutory boundary after enactment. County fixed effects; standard errors clustered by county. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.				

Table 12: Minimum Detectable Effects

Establishes what magnitude of disparity the racial tests could have detected.

Sample	n Black	n White	White forbearance rate	MDE (pp)
Private-label, performing, outbound	194	562	5.5%	4.5
Private-label, performing, inbound	182	499	9.0%	6.0
Federally backed, performing, outbound	93	418	11.5%	8.5
Federally backed, performing, inbound	93	354	21.5%	12.0
Minimum detectable effect at 80% power and 5% two-sided significance, April 2020, vendor-assigned race.				

Table 13: Forbearance, Servicer Communication, and Borrower Race

Estimates of equation (1) with a Black borrower indicator and its interactions, reported for four samples and two estimators. Firth penalized likelihood is used because maximum-likelihood logit exhibits quasi-complete separation in the smaller subsamples; the linear probability model gives marginal effects and is immune to that problem. Firth cells show the coefficient with a 95% profile-likelihood interval beneath; a blank interval means the profile was not computed for that term. LPM cells show the coefficient with a standard error beneath. All specifications include county fixed effects and standard errors clustered by county. Maximum-likelihood logit estimates agree in sign and significance throughout and are omitted for space. In the federally backed subsample the Firth profile interval is numerically unstable across estimation runs and is not interpreted; see Section 5.3. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Term	All loans: Firth	All loans: LPM	Federally backed: Firth	Federally backed: LPM	Private-label: Firth	Private-label: LPM	Private-label (ex cash-out): Firth	Private-label (ex cash-out): LPM
Black	-0.086	-0.0036 (0.0054)	-0.159	-0.0155 (0.0144)	-0.005	0.0000 (0.0055)	-0.267	-0.0222 (0.0251)
Inbound $\times$ Black	-0.344 [-1.05, 0.32]	-0.0216 (0.0210)	-0.283*** [-0.54, -0.05]	-0.0237 (0.0311)	-0.310 [-1.53, 0.78]	-0.0157 (0.0261)	-0.293 [-5.40, 2.71]	-0.0413 (0.0490)
Outbound $\times$ Black	0.329 [-0.25, 0.87]	0.0159 (0.0160)	0.285 [0.04, 0.56]	0.0238 (0.0271)	0.460 [-0.45, 1.29]	0.0188 (0.0198)	0.458 [-1.44, 2.11]	0.0203 (0.0567)
Inbound $\times$ Perf. $\times$ Black	0.470 [-0.33, 1.30]	0.0257 (0.0265)	0.123*** [0.08, 0.14]	-0.0106 (0.0517)	0.730 [-0.52, 2.08]	0.0432 (0.0355)	0.406 [-3.38, 5.83]	0.0371 (0.1300)
Outbound $\times$ Perf. $\times$ Black	-0.524 [-1.32, 0.27]	-0.0288 (0.0231)	-0.287 [-0.33, 0.20]	-0.0143 (0.0442)	-0.808 [-1.97, 0.36]	-0.0395 (0.0253)	0.872 [-2.00, 3.73]	0.0468 (0.0874)
Observations	19,156		6,657		12,499		886	

Each sample reports three estimators. Logit is maximum likelihood, with standard errors in parentheses; “sep.” marks coefficients affected by quasi-complete separation, which are not interpretable. Firth is penalized likelihood, with 95% profile-likelihood intervals in brackets and stars from penalized likelihood-ratio tests; a blank interval means the profile was not computed for that term. LPM is a linear probability model whose coefficients are marginal effects in probability units, with standard errors in parentheses. All specifications include county fixed effects; standard errors are clustered by county. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 14: Difference-in-Differences: Effect of Statutory Eligibility on Forbearance

Estimates of equation (2). Federally backed loans are treated; private-label loans are control; the post period begins with CARES Act enactment at end-March 2020. Columns (3)–(4) restrict to inbound contacts and (5)–(6) to outbound contacts.

Sample	Gov $\times$ Post	Std. error	p	N
All contacts	0.0358***	(0.0026)	0.000	96,201
Inbound only	0.0343***	(0.0080)	0.000	15,178
Outbound only	0.0179***	(0.0045)	0.000	22,239
Performing only	0.0440***	(0.0036)	0.000	68,495

Linear probability estimates of equation (2). County and month fixed effects; standard errors clustered by county. These estimates identify the effect of statutory eligibility on forbearance, not the racial gap.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 15: Covariate Balance: Federally Backed versus Private-Label

Standardized differences above 0.25 in absolute value conventionally indicate imbalance.

Variable	Federally backed	Private-label	Std. diff.
Orig Ltv	95.92	72.27	1.272
Orig Fico	617.42	610.00	0.111
Balance	11.52	10.43	1.227
Rate	0.04	0.08	-1.795
Corp Advances	5.79	7.39	-0.833
Performing	0.527	0.810	-0.629
Inbound contact	0.234	0.117	0.310
Outbound contact	0.340	0.174	0.387
Reported unemployment	0.059	0.020	0.198
Observations	33,343	62,901	

Standardized difference is the difference in means divided by the pooled standard deviation. Values above 0.25 in absolute terms conventionally indicate imbalance that reweighting should address.

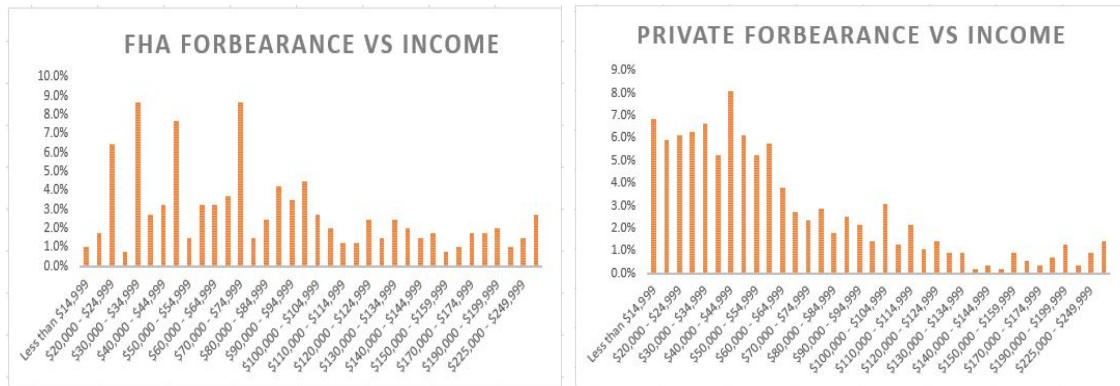


Figure 1: Forbearance Discussions and Loan Counts by County Median Income

Federally backed loans (left) and private-label loans (right). Forbearance discussions among federally backed borrowers occur across the income distribution; among private-label borrowers they concentrate at lower incomes, consistent with a binding hardship-documentation requirement.

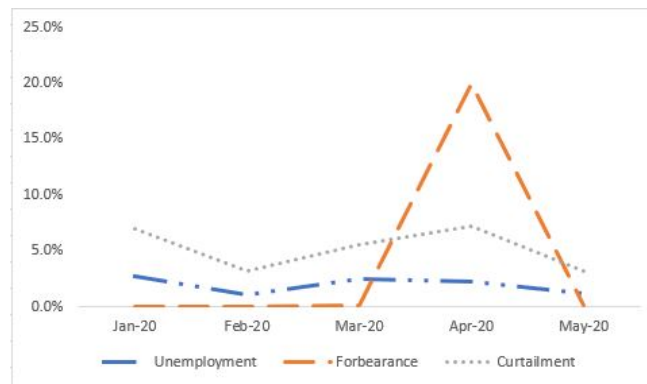


Figure 2: Performing Loans: Monthly Trend in COVID, Unemployment, and Income Curtailment Flags

Forbearance discussions among performing federally backed borrowers exceed reported unemployment and income curtailment, consistent with take-up of an attestation-based entitlement.

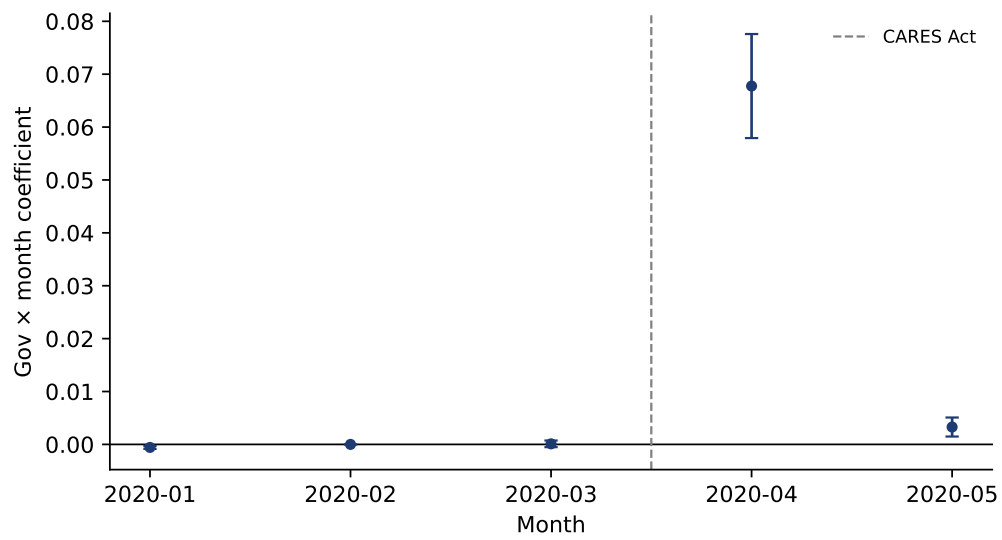


Figure 3: Event Study: Federally Backed $\times$ Month

Linear probability coefficients with 95% confidence intervals, February omitted. COVID-19 forbearance is identically zero in January and February, so the pre-period carries no information about differential trends; see Section 5.2.

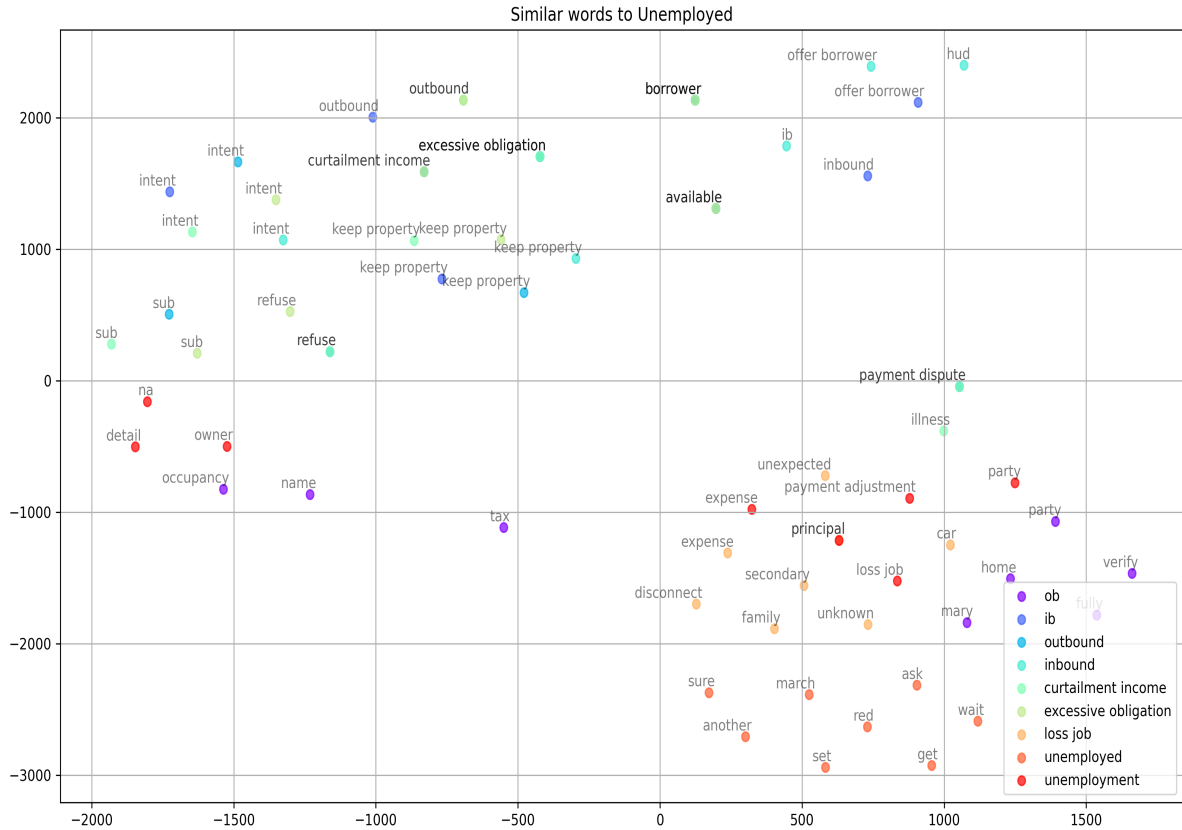


Figure 4: t -SNE Projection of Terms Associated with Unemployment, Inbound, and Outbound Contact

Outbound-associated terms (violet) cluster around renegotiation and verification vocabulary; inbound-associated terms (sea green) cluster around borrower requests for modification and relief. Remaining clusters relate occupancy and income curtailment to stated borrower intent. Additional projections appear in the Online Appendix.

A Keyword Dictionaries

Employment disruption. “unemployed”; “out of work”; “laid off”; “furlough”; “lost job”; “suspended”; “insufficient” in conjunction with “conditional liquidation”; “not available” in conjunction with “payment”; “loss” in conjunction with “request”; “didn’t” in conjunction with “work”.

Inbound contact. “inbound”; “IB”; “received”; “borrower is writing”; “was contacted”; “reached out to” (excluding constructions naming the borrower as recipient); “borrower” in conjunction with “informed” or “indicated”; “request”; “marital difficulties”; “death of family member”; “excessive obligations”; “casualty loss”; “payment dispute”; “tenant not paying”; “prior bk”. Records containing “OB” or “outbound” are excluded.

Outbound contact. “outbound”; “OB”; “called borrower”; “asked”; “replied”; “responded”; “response”; “decline”; “no contact”; “next due”; “will be asked”; “offered borrower”; “LM Program”; “LMStatus.”; servicer-branded template language; “COVID19 Forbearance Letter 712 Requested from vendor”. Records containing “IB” or “inbound” are excluded.

B Illustrative Deferred Cash Flow

To fix magnitudes, consider the aggregate payment deferral implied by first-round forbearance uptake. Approximately four million borrowers with federally backed mortgages had entered forbearance by the end of May 2020. Applying the mean government-loan principal-and-interest payment from Table 1 of \$721.18 over a six-month initial forbearance term gives

$$4,000,000 \times \$721.18 \times 6 \approx \$17.3 \text{ billion}$$

in deferred principal and interest.

Three qualifications are essential. First, this is deferred cash flow, not loss: forbearance postpones payments and the great majority are ultimately recovered through reinstatement, repayment plans, deferral to loan maturity, or modification. Second, the incidence falls on servicers during the advance period and on investors thereafter, not on the federal fisc except through the guarantee. Third, this is an accounting identity applied to aggregate counts, not an estimate of program cost, which would require modeling re-performance and the counterfactual of no relief.

Supplementary Files

This is a list of supplementary files associated with this preprint. Click to download.

- [onlineappendix.pdf](#)