Preschool Politics: Policy-Adaptive Political Preferences and the Crosscutting Feedback Effects of Partway Funded Transformative Social Benefits

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Abstract

Preschool Politics: Policy-Adaptive Political Preferences and the Crosscutting Feedback Effects of Partway-Funded Transformative Social Benefits

*Natural and Quasi-experimental Evidence from New York Parents*

Sophie Jacobson

2021

Flipping the expectation that public opinion drives democratic policymaking, I argue lived experience with current policy gives rise to citizens’ beliefs about appropriate roles for government. My original panel study leverages random allocation of limited Universal Public Preschool (“UPK”) seats in New York State for proper empirical tests of policy feedback theorized at the citizen-level. Building on prior work, I expect divergent experiences with the valuable and visible public preschool benefit to change parents’ childcare attitudes over time, systematically splitting preferences among citizens with similar and substantial stake in public preschool policy.

Difference-in-differences analyses with complimentary natural and quasi-experimental samples yield causal evidence that Universal Pre-K transforms parent preferences: one semester with non-means tested public preschool generates expectations of greater government responsibility for childcare among parents whose children win a seat.

Views among peers who purchase private preschool for UPK-eligible children also adapt with life. Relatively well-resourced parents in this group are distinctly supportive of publicly provided childcare options in September, then become markedly less so after months invested in individual solutions for their own.
In theory and actuality, partial public investment in preschool entitlements inspires very different politics along the financial dimension of childcare responsibility. Parents with children in preschool — private and public programs similarly — become more likely over a semester to believe childcare costs should be covered by government. As anticipated, the remaining eligible parents with access to neither a public nor private seat are unmoved by the school calendar.

Affirming positive policy experiences can be politically meaningful in an age of ideological polarization and government distrust, my study bridges the policy feedback literature’s rich foundations with recent research mostly focused on the Affordable Care Act, and emergent concern over (non)findings. In doing so, I reveal the oddity of wholly-private responsibility for childcare to be critical in the reproduction of gender, race, and class disparities in the United States. Above all, my findings establish universal preschool as a partial remedy with transformative potential — for American democracy as much as educational equity and our economic vitality.
Preschool Politics: Policy-Adaptive Political Preferences & the Crosscutting Feedback Effects of Partway-Funded Transformative Social Benefits

Natural and Quasi-experimental Evidence from New York Parents

A Dissertation
Presented to the Faculty of the Graduate School
Of
Yale University
in Candidacy for the Degree of
Doctor of Philosophy

by
Sophie Jacobson
Dissertation Advisor: Jacob S. Hacker
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motivate me to glean and share your insights so that other families might benefit from your experiences of parenting, with or without a public hand.

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I. INTRODUCTION

The United States stands alone among modern nations in leaving childcare up to each family. Conventional notions of legislative responsiveness in American politics scholarship suggest that citizens’ distinctive social policy attitudes must account for the nation’s exceptionally-private allocation of childcare responsibilities. From this familiar vantage point, an absence of audible demand from parents is presumed evidence that most American families thrive without meaningful public policy support prior to kindergarten. I argue for an opposite set of causal dynamics, with profound implications for the quality of democratic citizenship in the United States today.

Extending past scholarship on American social policy and citizen-level political feedback, I focus on childcare as a case to develop and test the claim that personal experiences with policy beget desires over government’s role. My original panel study leverages quasi-experimental allocation of ¼ as many State-funded Universal Public Preschool (“UPK”) seats as age-eligible children in New York for proper empirical tests of political feedback dynamics theorized to reshape individual beliefs and behavior through impactful encounters with policy. Taking time seriously, I find large and divergent attitudinal effects when, where, and for whom my hypothesis of policy-adaptive political preferences predicts. In turn, I gain rare insight into mechanisms

\[^1\] Downs (1957) and Meltzer and Richard (1981) offer foundational accounts attributing the distribution of citizens’ intrinsic unidimensional political interests, ideological and economic respectively, with law-like influence over democratic politics and governance. Most empirical behavioral research in American politics implicitly presumes this political model, while normative accounts often define democracy with reference to legislators’ responsiveness to constituent opinion. Hacker and Pierson (2014) comprehensively detail this perspective’s influence in the subfield. For another take on an overlapping set of concerns with mainstream methods and theory in modern political science, see Shapiro (2005).
responsible for the citizen-level political effects observed here, and in the policy feedback literature generally.

Difference-in-differences analyses with complimentary natural and quasi-experimental samples yield credibly causal evidence of substantial attitude change among parents with direct experience of New York’s public preschool program. Altogether, findings mostly match a set of empirical expectations which follow from theorizing citizens’ political preferences and propensity for engagement as adaptive to impactful encounters with government.\(^2\) Over months, lived experience with a child in universal public (private) preschool gives rise to new beliefs about the appropriate allocation of public versus private responsibility for providing childcare, and as a separate matter, paying for it. Building upon generations of scholarship on feedback in American social policy, I develop and test this theory – political adaptation through lived policy experience – against two prominent and plausible alternatives which expect covariation in policy attitudes and preschool uptake among eligible parents: A) self-selection by ideology, party, or political affect into preferred arrangements (Lerman et al. 2017), or psychic states amenable to positive feedbacks (Lerman and McCabe 2017; Lerman 2018; McCabe 2016b); and, B) maximization among program beneficiaries, offset by retaliatory negative shifts in support, attitudinal or electoral, among

\[\]

\(^2\) Whether grounded in teamsmanship, ideology, or material interest, most American politics scholarship presumes the inverse, a behavioral model of democratic politics and governance writ large. Highly stable personal traits like partisan identification and socioeconomic background, often presumed to be exogenous and fixed, are taken as fundamental in the sense of synonymous with citizen preferences mapped into left-right political space. Regular elections dictate that public policy generally reflects the wishes of popular majorities if the system is a democracy.
“burdenficiaries” (Hopkins and Parish 2019; Hobbs and Hopkins 2021a; Jacobs and Mettler 2018).

Simply put, one semester with a child in non-means tested public preschool generates expectations of greater government responsibility for childcare. Compared to themselves a few weeks into the school year, parents with the good fortune of a universal preschool seat—"Universal Pre-K Parents”—become twice as likely by January to believe government rather than family members, paid caretakers, employers or some other private party ought to provide care for children as young as two. On the more fringe matter of publicly provided infant-care, average support increased by 75% after four months of similar experience for a 3-4 year-old.

Among the parents studied here who purchase private preschool for UPK-eligible children—“Private Pre-K Parents”—childcare provision preferences evolve along an opposite path over the same period. Relatively well-resourced parents in this group standout for household income about twice the average reported by peer groups in this study, and less obviously, the strongest baseline support for publicly provided childcare. At the school year’s start parents paying for preschool were most likely to prefer public options. But, as I argue, attitudes adapt with the circumstances of life. Private Pre-K

3 Michener (2019) coins this helpful term for extra-burdened or losing citizens with respect to some aspect of policy. Conceptually inverse to beneficiaries, the policy winners conventionally at the focus of feedback research, “burdenficiaries” are citizens most harmed or disadvantaged by the rules due to positioning within the polity, generally compounded by citizens’ economic (tax burdens) or social rank (over-policing).

4 Respondents are coded as preferring public provision \( \text{Provide}^p_i \leq K = 1 \) if “public schools” or “government agencies” is designated best over private options: family members, private childcare providers, private childcare centers, employers, or “someone else” with some nongovernment source typed in. Notably, the online survey instrument allows an option to specify separate preferred arrangements for young children of different ages.
Parents grow markedly less supportive of public childcare by midyear. Considering public arrangements for children ages two and three, one semester diminished Private Pre-K Parents’ support by about a third.

More striking yet, life with a child in private preschool eroded the mid-schoolyear chances of continuing to endorse the consensus notion that 5-year-olds are best served in public schools. Compared to their September views, Private Pre-K Parents become much less likely to prefer public kindergarten, over private alternatives. This policy-undermining negative feedback in support for public kindergarten appears strongest among parents who tried their luck for a Universal Pre-K seat before resorting to a private backup, falling 15 percentage points in about four months; this difference in degree and not kind is rooted in similar experience once preschool starts, with actual and potential applicants among Private Pre-K Parents converging to the same reduced January rate of support for public kindergarten. Insofar as mid-schoolyear attitudes inform next fall’s enrollment choices, adverse attitudinal effects presented in here should have consequential behavioral corollaries which directly undermine New York’s Universal Pre-K program. Most-literally, when forced exit for preschool leads families to form new loyalties, habits, or community ties, and in turn, delay or decline return to public education, the system responsible for administering short supplied preschool benefits sees an already insufficient support base shrink.6

5 Due to the relatively small number of Lottery Losers (n=16), I do not formally test for a magnitude difference in the negative attitudinal feedbacks observed between Private Pre-K Parents in my samples.

6 This holds considering support in term of tax dollars or enrolled students. Forthcoming research empirically tests predicted behavioral feedbacks in 2020-2021 enrollment with panel results extended through February 2021. COVID-19 notwithstanding, preliminary findings strongly
Financial responsibility for childcare is a different political matter. While diverging in beliefs about optimal provision of early care and education over months, *Universal Pre-K Parents* and *Private Pre-K Parents* converge by mid-schoolyear on heightened conviction that childcare costs should be covered by the government. Given annual preschool tuition far exceeding college in New York, like most states, this should hardly be surprising. My hypothesis of political adaptation through policy experience anticipates heightened parent demand for the government to assume financial responsibility after several months experience with costly formal preschool. More important than personally paying out-of-pocket, I argue that life with early childhood education is the dose-dependent treatment precipitating in parents qualitatively new beliefs about the type of arrangement very young children need to thrive – the most expensive, comprehensive, school-like kind.

As expected, childcare attitudes among a remaining group of UPK-eligible parents whose preschoolers do not participate in formal education, “No Pre-K Parents” to be consistent, are unaffected by the school calendar. Comparing parents grouped by preschool arrangement – public, private, or none – typical families of each kind rely upon bundled arrangements to meet regular childcare needs before children age into school.8

suggest an enrollment effect along the lines I propose and that attitudinal changes reported here endure.

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8 For the purposes of Universal Pre-K eligibility, and this study, the terms “preschooler” and “preschool-aged child” refer to children ages 3 or 4 according to *birthday cutoff criteria* established for kindergarten in their respective school districts. Students who meet kindergarten eligibility are not eligible to enroll in UPK instead. By extension, children more than two years beneath the cut for kindergarten eligibility in a district are too young under the State’s UPK
Prior to the COVID-19 pandemic, families of all traits looked similar in two respects reflecting the magnitude of wholly-private childcare responsibilities. Mothers all but always reported spending large portions of each day on childcare, notwithstanding wide differences in preschool access, employment, and relationship status, among other important factors. Second, families of all kinds also typically relied on grandparents, or nearby relatives, for regular childcare. What distinguishes No Pre-K Parents from peers for purposes of this study is an absence of formal early education in the mix for a UPK-eligible three or four-year-old. This subset defined by preschool nonaccess undergoes neither a common early-September shift in daily parenting experiences nor shared adaptation in childcare attitudes unfolding over months.

Adaptive Political Preferences in Preview: a citizen-level theory of political change through policy

The pattern and pace of belief change across preschool arrangements measured by my panel provides leverage for adjudicating between theoretical accounts of attitude formation in recent policy feedback research, and the most fundamental challenge thereof, conventional behavioral interpretations rooting redistributive politics (Meltzer and Richard 1981), and democratic governance writ large (Downs 1957), in citizens’ given political preferences. Childcare attitudes predicted and presented here suggest a

eligibility criteria. Many but not all school districts simplify matters with criteria which coincide with the calendar year (i.e., children born in 2015 are kindergarten-eligible in the fall of 2020, while children born in either 2016 or 2017 have claims to UPK).

Section V provides a more thorough account of childcare use among other material and psychological trends of interest, with an emphasis on average differences across preschool arrangement and over time.
mechanism of political adaptation, rather than self-selection (Alternative A) or maximization (Alternative B) links preschool access to patterned preferences over the provision and financing of childcare among Universal Pre-K-eligible New York parents.10

New policy interests are not instantiated upon (un)lucky draw in the Universal Pre-K lottery. Nor do Universal Pre-K Parents (Private Pre-K Parents) realize clear benefits from the outset and emerge as distinct proponents of public (private) preschool when first surveyed in mid-to-late September. Over months, evolving expectations of public responsibility in the realm of childcare emerge through parents’ experience with the valued preschool benefit, or conversely, paying dearly for a substitute.

Notably, I do not find that Private Pre-K Parents opt for purchased alternatives to UPK on account of conservative political ideology,11 aversion to government, or elite tastes.12 Instead, time spent in private arrangements erodes once strong support among natural proponents for truly-universal public preschool – dual-breadwinners with sufficient need and means to privately cope despite exorbitant market rates (Morgan 2017). By empirically isolating direct personal policy experience as the “treatment”

10 See Figure 1 in Section III for a corresponding visualization of differences in the timing, pace, and pattern of theorized evolution in parents’ support for publicly-provided options according to preschool access. My theory of political adaptation is depicted in the bottom panel, with the conventional behavioral model of partisan/ideological policy support at the top (Alternative A), and in the middle the account I label “maximization” (Alternative B), an instantaneous feedback mechanism purportedly responsible for positives among those benefitted as well as observed negative effects among those extra-burdened by a policy.

11 Due to the surprisingly large number of self-identified ideological conservatives who choose “other” or “no preference” instead of identifying as Republicans, I use 7-point political ideology where 7 is most conservative as a proxy for partisanship and interchange the terms.

12 Only 10 out of 182 parents who completed surveys in September 2019 and January 2020 are excluded from analysis for known or potential aversion. Section IV and appendix IV B provide more details on random and nonrandom parent samples, and the processing of my data prior to analysis.
responsible for policy’s attitudinal effects, my results bridge the policy feedback literature’s rich foundations emphasizing instances of democratic empowerment among individuals and groups through impactful public programs with a newer generation of quantitative feedback scholarship; focused primarily on early implementation of the Affordable Care Act for stronger research designs, recent feedback studies have thus far reported modest, not-infrequently missing, policy-reinforcing (“positive”) effects among Americans benefitted by federal healthcare reforms (Campbell 2020b; Hopkins and Parish 2019; Hobbs and Hopkins 2021b; Haselswerdt 2017).

Belief change through policy experience is not immediate or sudden on my account. Nonetheless, policy feedback theory and past findings ground my predictions that, relative to social programs generally and the Affordable Care Act (“ACA”) specifically, universal preschool will be capable of generating large and rapidly unfolding belief and behavior changes among parents impacted by the program. For many Americans, I argue, life with a child in universal public preschool plausibly rises to a transformative citizenship experience. My conclusions about the magnitude and pace of attitudinal feedback are particular to universal preschool policy, and unrepresentative of social policy feedback dynamics. This is most true in regard to contemporary American governance, marked by minimal direct social provision through transfers or in-kind services (Hacker 2002; Mettler 2018; Mettler 2011).

My findings should assuage emergent concerns that ideological polarization and distrust in government preclude positive feedback effects in contemporary American politics (Mettler 2019; Hacker and Pierson 2019) — to an extent. Partisanship of course impacts views on childcare policy, but this essential piece of political identity does not dominate or disrupt the positive (split) attitudinal effects of personal experiences among Universal Pre-K Parents (Private Pre-K Parents) in my panel.
As detailed in the penultimate Discussion Section (VII), the attitudinal impacts of one semester with universal preschool are unchanged when theoretically important traits and potential over-time confounders suggested by prior research are added to the unadjusted difference-in-differences specifications presented in my main results. More productively, I leverage insights from the case of universal preschool to suggest that polarization is mostly a supply-side problem for policy feedback (Hacker and Pierson 2018). Citizens respond as theorized to generous, direct, life-changing public policies, in part because programs meeting these criteria are so seldomly produced by American government. Asymmetric polarization and affective mass partisanship are aggravating factors particularly helpful in explaining the modern GOP’s ever-more unpopular federal policy agenda (Hacker and Pierson 2020). More fundamentally, “unified” Democratic government in President Biden’s first years underscores basic sources of drag and bias in federal policymaking, like American federalism itself (Riker 1987), the United States’ notoriously fragmented political institutions, and consequential instances of counter-majoritarianism, constitutional and customary varieties conspicuously premised in preserving white privilege.

Recent American politics scholarship also documents record high government “distrust” as a detriment to the formation of policy feedback. On one account, Americans of all stripes are convinced our government is incompetent. Priors about government are so negative as to spur systematic misattribution of credit to private parties when

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13 I control for income, Republican identification, single parenthood, and a race dummy coded 1 for parents identifying as nonwhite or more than one race. As for coincident changes that might threaten inference if preschool groups are impacted differently, I add ideology (7-point, increasing with conservatism), government trust (measured on a 4-pt. scale from strongly disagree to strongly agree), and subjective economic security measured in months able to subsist without income and without serious hardship.
Americans perceive their public services to be well-provided (Lerman 2018; McCabe 2016a). Along somewhat different lines, lagging trust is cast as an increasingly common perception that government serves only the extremely wealthy and “welfare” dependent. As Suzanne Mettler demonstrates, a disconnect exists in modern politics between Americans’ record-level animosity toward government and federal benefits more extensive than ever (Mettler 2018), albeit submerged (Mettler 2011).

However defined, government distrust is understood in recent literature to disrupt positive feedback processes in manners parallel to partisanship. In some studies, distrust (partisanship) skews attribution such that public programs do not get due credit for positive experiences, and by the same logic, incur undue blame when private service providers fall below citizens’ low expectations of what government might offer (Lerman 2018; McCabe 2016b). In other studies, beneficiaries recognize and appreciate particular policies from which they benefit, but distrust in government and (or) Republican partisanship overwhelms measurable improvement in attitudes toward the Affordable Care Act (Jacobs and Mettler 2018), or towards the lawmakers, party, and government associated with commonly used social benefits and tax breaks, broadly speaking (Mettler 2018).

Mirroring national norms, parents in my panel express low trust in government across multiple measures at the outset of the schoolyear. Like Americans in general, New Yorkers with preschoolers tend to believe government officials cannot be trusted to do right, and that the wealthy have exclusive influence insofar as citizens impact what government does. Self-referential measures of political efficacy are likewise low across preschool groups at baseline, and again mid-schoolyear (Section V).
Nonetheless, distrust and disillusionment function similarly to ideology and party identification in my analyses. These (repeatedly measured) citizen-level political attributes neither interfere with Universal Pre-K uptake nor impede the over-time accumulation of positive, policy-reinforcing, attitudinal feedbacks precipitated by experience with a child benefitting from New York State's valuable, visible (Arnold 1990), public preschool program.

The case of universal preschool establishes positive personal policy encounters can still recast individual preferences and expectations of government despite contemporary American politics. It also underscores why measurable new examples of policy-reinforcing political feedback are difficult to come by. Individual responsibility for social welfare and private risk are the predominant experience of democratic citizenship in the contemporary United States (Campbell 2014; Hacker 2006; Mettler 2018). Through this lens, universal preschool is among relatively few plausibly transformative social policies offered at any level of American government.

Large short-run effects follow substantively from the dual populations and evidence-based equity promoting purposes served by universal public preschool. Universal Pre-K frees parents – mothers in all but unusual circumstances that prove the

14 Overpoliced and routinely-surveilled race-class subjugated communities (Soss and Weaver 2017) present an important partial exception. Citizens living in such spaces are not unique through the lens of social provision, bearing individual responsibility for the material wellbeing and security of themselves and dependents, per my claim. But life in RCS communities is simultaneous marked by the repression of too much government in forms which rational individuals actively seek to avoid, like the police (Lerman and Weaver 2014; Goffman 2014), and Child Protective Services (Fong 2020).

15 So long as President Biden’s “American Families Plan” remains a proposal, and the enhanced Child Tax Credit included in the American Rescue Plan is not extended or made permanent.
rule – to work for pay, pursue higher education, upskill, or otherwise spend time on non-childcare activities. Whatever else mothers accomplish, fewer caretaking hours should help to chip away gendered parenting norms which persist despite decades of national experience wherein most women with children too young for public school also have had jobs. Reinforcing gender inequalities at home and work hold women back from realizing full career potential (Iversen et al. 2020), while undercutting financial security for American families, and macroeconomic vitality for the nation as a whole.

Extensive scholarship on the political economy of gender inequality, as well as recent findings from nearby Washington D.C., support a presumption that many New York mothers unfettered from childcare duties on schooldays will indeed pursue paid work in their freed-time. Career-disruptions associated with motherhood until children reach kindergarten-age in the United States, and not with fatherhood,16 have been linked to the United States’ stall around 2000, and 21st century decline in maternal workforce participation as rates in other rich democracies continued to climb (Blau and Kahn 2013). The same gendered parenting dynamics are responsible for persistent gendered gaps in wages (Goldin et al. 2017), lifetime earnings (Goldin and Mitchell 2017), and career advancement, even among the most elite MBAs (Bertrand et al. 2010). Close to home for my panel, mothers with preschoolers in the nation’s Capital have seen a 10 percentage point increase in labor force participation in the decade since D.C. launched free full-day preschool to three and four-year olds (Maleek 2018).

16 Fatherhood is associated with a boost in the labor market outcomes on which mothers take a hit.
Separate from the prospect of more time for paid work, although often coupled with it, high-quality universal preschool enables some families to save substantial sums otherwise spent on private arrangements (Section V). For all but the very-wealthy, the expense of lesser quality regular childcare arrangements, let alone comprehensive and educational private substitutes, ought to engender appreciation of government’s role. Alleviated of $13,000 per child in (pre-pandemic) average private preschool tuition, relatively well-off Universal Pre-K Parents experience a disposable income boost strikingly similar in magnitude to material gain through new earnings for non-wealthy Universal Pre-K Parents who could not dream of affording an arrangement comparable to NY’s public offering, but with the regular and reliable support of preschool, become situated to financially contribute for their families while the youngest learns.

Considering private preschool costs in terms of New York State’s minimum wage, a demographic disproportionately comprised of mothers – the lowest legal rate disproportionately paid to mothers and nonwhite mothers in particular – a fulltime minimum wage earner would need to work more than half of a year to afford average tuition for one preschool seat. Even so, if she was previously working to afford private center-based care at toddler or infant rates, half a year’s pay once her child reaches preschool will indeed feel like a bargain.

Along with this large and direct impact on a household’s potential disposable income – through more income and/or less private childcare spending – Universal Pre-K provides young children high-quality, reliable, and age-appropriate early education for

\footnote{According to analysis from the Economic Policy Institute, “a minimum wage worker in New York would need to work full time for 33 weeks, or from January to August, just to pay for child care for one infant (\textit{Child care costs in the United States} 2021).}
free. Offered without regard for family background, public preschool becomes a right of citizenship. Government childcare done this way approximates the noncontroversial public claim to K-12 education, rather than “welfare” steeped in the racialized stigma of mothers too deviant to care for their own children.

In short, universal preschool is unique among American social programs in delivering large, visible, and direct benefits which dramatically impact daily life for two generations of a family at once. For children, positive political feedback attributable to public preschool experiences is likely to require more than a decade to become measurable. Parents, on the other hand, register meaningful political change through public preschool exposure much sooner and more quickly than their children, as well as the “typical” social policy beneficiary in the contemporary United States.

My study thus contributes political science’s concerns for power, distribution, and democratic governance to the multidisciplinary research tradition establishing that public investment in preschool substantially improves wellbeing for American families. Supported by universal preschool, a parent is better situated to fulfill family roles of breadwinner and primary caretaker, as well as the political and civic duties of an empowered, engaged, meaningfully incorporated democratic citizen. Just a few months of life with a child in universal preschool changes parents’ understandings of government responsibility to children like their own, and even younger. On the caliber of universal public preschool, valuable and visible public benefits can have substantial political

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18 To the extent Universal Pre-K improves adult economic outcomes via success in school, greater economic resources should translate into higher participation among citizens who receive UPK as children. It is unclear whether and how preschool might have interpretive effects shaping participation in adulthood, but potential resource effects follow straightforwardly from UPK’s effects on educational attainment and economic success.
impact – altering basic beliefs on a matter so deeply-felt as childcare “choices” within months.

To be sure, my findings also highlight perverse unintended consequences, following from arbitrary features of New York’s implementation scheme despite the transformative benefits it delivers. Realizing substantial progress through public investment in their children, Universal Pre-K Parents come to know full well profound potential losses had random chance gone differently for their family. Experiences with preschool profoundly shape parents’ views about the proper allocation of private versus public responsibility for childcare. Yet, the deciding role of luck almost certainly distorts political lessons gleaned by winning parents in ways which undercut universal preschool’s substantial potential to enhance general faith in a government which needs it badly.\textsuperscript{19}

Universal Public Pre-K empowers parents with critical political resources (money; time; greater leeway for a career over hourly work). More importantly, public preschools provide literal venues for democratic engagement, institutionalizing opportunities to practice collective governance and constructive partnership with public officials through PTAs and parent-teacher conferences, for example. Considered through an organizational lens inapplicable to many social benefits,\textsuperscript{20} public preschool boasts

\textsuperscript{19} At this point in mass polarization government assessment questions largely proxy for partisan identification with or against whomever occupies the White House. Nonetheless, a secular decline in trust of government is clear in American opinion data over recent decades. See Pew, (PEW 2021)

\textsuperscript{20} The lack of constituency-generating potential when benefits are hidden in the tax-code (employer sponsored health insurance; home mortgage interest deduction), delivered by private 3rd parties (ACA exchange plans), or simply deposited into the bank accounts of discrete citizens (SSDI, advanced CTC) is a recurrent theme in policy feedback research.
potential to coax political community among claimants embedded within pre-organized social networks defined by mutual interest in the wellbeing of a particular school, and generally the surrounding neighborhood. Endowed with resources, skills, and social infrastructure long recognized as essential ingredients of upper-class bias in political participation (Brady et al. 1995), some Universal Pre-K Parents may also become too attuned to arbitrary and partial public investment in early opportunities critical for lifetime success to reasonably consider more deeply engaging American democracy with potentially substantial personal political capacities acquired thanks to impactful experience of government in preschool.

In concluding, I situate supply-constrained “universal” preschool within context of a much larger cross-section of decentralized and capricious contemporary American social policy which characteristically leaves government susceptible to delegitimizing unreliability (Jacobs and Mettler 2018; Mettler 2018; Lerman 2018). Beneficiaries may come to deeply appreciate some personal instance of public support, and from it conclude they cannot generally rely on similar. Who expects to win the lottery twice?

Above all, my research suggests acute need will not suffice to inspire mass demand for policies that support overstretched and struggling families in the United States. Instead, the evidence of political adaptation presented here suggest American families without access to public childcare options will continue to pursue class-stratified private strategies. Absent fundamental reforms aligning American social policy and labor markets with family sustaining norms embraced amongst even the next-most-liberal rich
democracies, Great Britain and Canada, private childcare responsibilities will perpetuate deeply disparate gendered, raced, and classed citizenship experiences in the now tenuously-democratic United States.

Overview

Section II provides background through a policy feedback lens on the nature of partial public investment in preschool entitlements throughout New York State and thus the scope of my panel study designed to measure and understand the program’s political effects. I emphasize design features which make this supply-constrained universal program an ideal test for competing theorized links between citizen preferences and policy encounters. For further nuance on program characteristics integral to political impacts, I contrast the state preschool initiative with President Obama’s signature healthcare package along key political and policy dimensions. Contextual and design differences between decentralized federal healthcare reform and state provision of preschool alongside K-12 in select public schools helpfully ground predictions which differentiate preschool politics in New York State from much more recent policy feedback research. Building on this scholarship, the policy level overview capitalizes on hindsight, “anticipating” weakly positive and more-immediately measurable negative

21 Notably, Asian rich democracies, Japan and South Korea, have embraced universal preschool and comprehensive family leave policies in the last 5-10 years, see (OECD 2021).
shifts in observed ACA support, toward motivating my claims about a social benefit politically transformative in the short run.

Universal preschool demonstrates to citizens through their children that government cares. For most Americans this lesson is at least somewhat novel. Drilling down beyond policy characteristics, Section III focuses on citizen-level mechanisms which might link parent preferences to children’s preschool arrangements. By situating lofty expectations about Universal Pre-K’s potential to transform childcare preferences and reorient relationships to government in foundational feedback studies linking differently designed programs of social insurance and assistance to the cultivation (Campbell 2003; Mettler 2005), and conditional enrichment or degradation (Soss 2000) of democratic citizenship, I also address emergent concern that modern American forces like political cynicism and polarization pose insurmountable interference.22

Focusing on childcare as an illustrative case, I develop a tractable theory of policy-adaptive political preferences alongside two prominent and plausible alternatives. Each account expects childcare attitudes to covary with preschool arrangements among parents, but they differ fundamentally with respect to cause, direction, and mechanisms. After grounding all three hypotheses in prior policy feedback research, I delineate testable predictions about variation in parent preferences according to preschool arrangement, pinpointing when, how, and for whom observable implications conflict. Compared to alternatives of self-selection and maximization, preference adaptation through policy experience yields weaker intuitions about between group differences in

22 Interference in the sense of conditioning or blocking policy’s benefits from lending to reinforcing politics.
September views on the nature and degree of government’s childcare responsibilities. Instead, my later onset and cumulative mechanism – adaptation through direct personal encounters with policy – emphasizes cross-cutting change over months, along separate practical and financial dimensions of public responsibility, with a specific exception of stasis in both attitudes for the group lacking preschool in any form.

Section IV and related appendices detail my two-part difference-in-differences design and the online panel of program-eligible parents I recruited to independently measure a large-scale natural experiment – supply-constrained preschool entitlements allocated by local lotteries throughout New York State. Capitalizing upon the rare randomized social policy treatment without risking broader substantive relevance (Dunning 2012), I pair complimentary natural and quasi-experimental analyses testing the same trio of competing hypotheses. Indeed, one implication of theorizing personal, impactful, experience as the cause of covariation in parent preferences and children’s’ care arrangements is that any evidence of differential between-group attitude change detected with the randomly assigned subsample of my panel, “Applicants” (N=79) should be evident in broadened latter analysis with “Constituents” (N=172), program-eligible New Yorkers screened for learning needs compatibility and to rule out aversion likely to thwart uptake of a Universal Pre-K offer. This section also overviews the

\footnote{About a dozen parents in my panel whose preschoolers benefit from Individualized Educational Plans (IEP) are excluded from the broadest population of interest in this study, UPK “Constituents.” Although reasons for violation of necessary assumptions are different, special needs children do not reasonably have potential outcomes in the treatment condition in the same sense as those excluded from quasi-experimental sample due to a parent acknowledging an aversion to the program. Sidestepping a thornier call about well-defined potential outcomes, Head Start parents are dropped from Constituents because I recruited too few into the study.}
microtargeted social media campaign with which I recruited UPK-eligible New Yorkers into my online parenting study; appendices IV A-C cover further data and met details.  

With parents grouped by preschool arrangement — universal public ($n=63$), private ($n=49$, including 7 Lottery Losers), and none ($n=60$, including 9 Lottery Losers) — the fifth section presents select stylized facts about parenting experiences and childcare attitudes in aggregate. Guided by decades of attention to policy’s resource and interpretive effects (Pierson 2000), descriptive evidence in this section considers salient material ways in which preschool does or does not differentiate families. Before proceeding to main results, I introduce the study’s two primary attitudinal outcomes — preferences for public over private childcare provision ($\text{Provide}_{it} = 1$), and childcare financing ($\text{Pay}_{it} = 1$). Group averages for each outcome are shown without adjustment, measured in September 2019 and January 2020, the pre ($t_0$) and post-treatment periods ($t_1$) used to test Universal Pre-K’s one-semester effects on change in beliefs about government childcare responsibility.

Section VI presents empirical findings from a pair of complimentary difference-in-differences analyses. With panel data collected for the purpose, I test my hypothesis of policy-adaptive political preferences against prominent alternatives privileging fundamental traits to explain covariation between parent attitudes and preschool arrangements: self-selection, and maximization. I begin with the most straightforward results, restricting focus in Part A to the subsample of my panel for whom UPK

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24 Extra materials include: microtargeted social media campaign with which I recruited eligible New Yorkers for paid online surveys on parents’ experiences and perspectives. Sample advertisements and information about pre-screening as well as further detail about natural and quasi-experimental comparison groups used to estimate UPK’s effects.
constitutes a fully randomized natural experiment, Applicants (N=79) comprised of Universal Pre-K Parents and Lottery Losers (n=16). Although my data is observational, I know in this case that treatment is randomly assigned among parents who participated in local lotteries for a seat. I begin with an informal check for balance on pretreatment covariates to assess my independent measurement of random assignment among lottery participants (Dunning 2012). Next, I employ difference-in-differences estimation to test for significant adaptation in preferences attributable to one semester with Universal Pre-K. Notwithstanding a small sample skewed toward treatment, results offer compelling causal evidence that life with preschool transforms parents’ basic beliefs about early arrangements necessary for the youngest to thrive, along with the scope and nature of government’s responsibility to ensure the availability of such opportunities.

To directly assess the relevance of clean causal insights from the potentially idiosyncratic bunch among UPK-eligible parents who try their luck for a free seat, Part B presents findings from a complimentary quasi-experimental analysis. Again using difference-in-differences estimation, I test the same three competing hypotheses with a broader nonrandom subsample of Universal Pre-K Constituents (N=172), not merely eligible but verified-potential applicants. In this way, the 2nd half of my analysis is designed with the aim of replicating, then extending, strongly identified one-semester treatment effects of Universal Pre-K among Applicants (N=79) in tests able to

25 Practically speaking, I estimate two-way fixed effects OLS regression models without added covariates. Along with individual and period-level (“two-way”) fixed effects, an interaction between binary indicators for treatment status and post-treatment period estimates differential change, the quantity of interest. Heteroskedasticity robust standard errors are reported with all regression results to account for the panel structure of my data (Aronow and Samii 2016; Samii and Aronow 2012).
simultaneously estimate adaptation in childcare preferences attributable to a semester spent spending for private preschool.

The discussion section (VII) unpacks my findings, causal and generalizable evidence that public investment in universal preschool can be politically transformative for parents within months. My results add benefits for American democracy to public preschool’s economic and educational impacts, long recognized as extraordinary at both micro (Heckman and Mosso 2014; Heckman 2006; Heckman and Karapakula 2019b; Gray-Lobe et al. 2021) and macro-levels (Sprung-Keyser and Hendren 2020; Heckman 2006; Heckman and Karapakula 2019a). To provide substantive context for the attitudinal impacts of preschool presented in Section VI without covariate adjustment, I plot unadjusted and adjusted estimates of Universal Pre-K’s one-semester attitudinal effects alongside coefficients for theoretically important traits including relatively high household income (above $100,000 before taxes in 201926), Republican identification, single motherhood, and race, in addition to potential over-time confounders suggested by prior research.

In closing I elaborate upon dual unforeseen threats to democratic legitimacy in the apparently fair practice of managing public preschool supply-constraints with community-level lotteries, as well as expected remedies grounded in lessons from studying the partway-funded universal preschool status quo in New York State. More important to precipitating self-undermining political feedback than personal traits like conservative ideology, distrust of government, or relatively high income, New York

26 My household income measure is an ordered factor, grouping self-reported 2019 household income before taxes in 5 bins: . The analysis in Section VII omits the first four income categories from presentation, focusing on the highest earning subset with incomes exceeding $100k.
State’s partial funding and arbitrary allocation rule generate structural incentives corrosive to mass support for universal preschool. Implementation inadvertently turns off the program’s best resourced natural proponents. Insofar as early education is not part of this political problem, negative feedbacks among potential Universal Pre-K claimants without benefit access, as well as lottery-induced distortion to the political lessons Universal Pre-K Parents glean about government in general, can be remedied with tweaks to New York State’s current approach. The theory of political adaptation through policy experience developed and tested here lend several specific redesign suggestions.
II. (KINDA-SORTA) UNIVERSAL PUBLIC PRESCHOOL IN NEW YORK STATE

By design, my panel tracks lived experience and political change among New Yorkers with universal preschool-eligible children, not just the fortunate who win seats. Informed by emergent policy system-level perspectives on offsetting (Hobbs and Hopkins 2021b) geographically disparate (Michener 2017), racially disproportionate (Michener 2019) and otherwise heterogenous political effects when citizens situated-dissimilarly experience government in a policy, I extend focus beyond beneficiary parents to a broader set of citizens likely to have distinctive and important political reactions to limited universal public preschool – the majority of New Yorkers whose eligible preschoolers cannot have a seat. Extra-burdened citizens of this kind ("burdenficiaries" Michener 2019) are importantly different from general outside publics theorized apart from target populations of policy in some earlier feedback research (Mettler and Soss 2004; Soss and Schram 2007; Schneider and Ingram 1993). Indeed, research to date on healthcare reform’s political consequences has most reliably recovered ACA-undermining negative shifts in support concentrated among aggrieved Americans who lost (Hobbs and Hopkins 2021a), or continuously lacked (Jacobs and Mettler 2018), affordable care after implementation.

Because feedback effects result “not only from policy benefits but also from policy burdens” (Jacobs and Mettler 2018), potential applicants warrant attention alongside Lottery Losers and Universal Pre-K Parents within each school district as core constituents of New York’s Universal Pre-K program. Excluding the tiny minority among my panel who express aversion to universal preschool (less than 4%), three who refrain
from ruling it out, and families with individualized learning needs, UPK-eligible New Yorkers are marked by common entitlement to the valued education benefit as a discrete political class with outsized policy stakes.

Public preschool coverage is partial, but parents’ words leave little doubt about well recognized community-wide consequences for childcare strategies, maternal workforce participation, and family wellbeing. After attesting to knowingly not having applied for a Universal Pre-K spot and sustaining support for their state’s preschool program, potential applicants included in my nonrandom Constituents (N=172) sample overwhelmingly opted to explain themselves in open-response space. Representative of several appeals to the unworkable uncertainty of lotteries a few months ahead of preschoolers’ first day, one Private Pre-K mother put it plainly: “our district doesn’t have enough spots for every child, only a small fraction. So (we) sought other preschool options.” Others took effort to clearly convey actual intent to apply, citing uncertainty about lottery processes as the problem alongside the arbitrary decision rule and undersupply; “our district is limited on spots and draws from a hat...and I missed the draw because no one said when it was.” Such perspectives disabuse the notion that Universal Preschool’s profound impacts are limited to the share who try their luck for a seat, much less win. Altogether, volunteered qualitative insights underscore reliable childcare as the linchpin to family survival, let alone “balance,” with dual breadwinning

As I do, in defining the quasi-experimental “Constituents” (N=172) subsample of from my panel, analyzed in the latter half of the results section (VI). Just seven of one hundred and eighty-two parents who completed the first two waves of the “New York Parenting” survey express any aversion to UPK. Along with three more parents who chose “other” without writing anything in, ten out of 182 parents are excluded from analysis. Section IV and appendix IV B review data processing and membership rules for the parent subsamples analyzed in this study.

With application, lottery, and announcement timeline specifics separately set by each public school district.
and caretaking responsibilities unlike anything borne privately in the rest of the rich
democratic world. Parents with the privilege often value a sure bet – be it purchased, or
personally provided. Both undercut universal preschool’s natural coalition, but they are
not equal. I argue that the resort to market solutions has distinctive political
consequences, onset with the first months of private preschool.

Whether parents pay a premium for assured preschool, opt for private programs
to facilitate a full workday, seek costly alternatives after proving unlucky in their local
lottery – or missing their chance for a chance due to purportedly-poor communication
between public schools and qualified families, my hypothesis of preference adaptation
through lived policy experience expects that *Private Pre-K Parents* will be likely to
develop new habits and loyalties once life takes that course. Months invested in private
substitutes give rise to new beliefs matching new behavior, alongside opportunities for
new social ties amongst likeminded parents in this particular context, school for very
young children. Precipitating reasons for private enrollment become trivial once new
routines reshape basic assumptions about the desirability of one’s own pricey approach,
and in turn, the relative appropriateness of government provided early education. This
lock-in dynamic whereby chance becomes enormously consequential through
increasing-returns processes and successively stickier politics is the hallmark of
foundational theorizing about feedback in the political world (*Pierson 2000*).

Following from developmental rather than behavioral premises about citizen
preferences, my expectation that mutual experience generates the same specific pattern
of attitude adaptation over months among *Private Pre-K Parents*— regardless of prior
lottery participation—is also a critical point of conflict in observable implications
between my mechanism, attitude adaptation driven by divergent day-to-day experiences
of proper parenting over months, and a “flip-the-switch” (*Campbell 2020a*)
maximization mechanism forwarded with more recent findings of negative feedbacks among burdened subgroups alongside, or instead of, expected gains in political support among citizens who most benefit.

Discussed below under policy design, and at the parent-level next in Section III, the income-unconditional “universal” nature of eligibility for New York’s public preschool program is widely viewed as critically important in American politics, whether mass attitudes are principally understood as a function of inherent ideology, material interest, or personal policy experience. Of forty-four states offering public preschool in some form as of fall 2019,29 New York is among eleven which do not impose low-income rules with intrusive burdens of proof for parents sufficiently low-income for a claim to public preschool. Along with the status affirming universality long recognized in scholarship and conventional political wisdom as essential to the deflection of “welfare stigma” by design in American social policy (Schneider and Ingram 1993), acute supply constraints and random allocation make New York’s program methodologically ideal to test citizen-level propositions about mechanisms which link policy attitudes and uptake.

Specifically, non-universal coverage supports the comparison group methodologically required for difference-in-differences estimation, and if the parallel trends assumption is credible, causal interpretation of universal preschool’s estimated feedback effects without random assignment. To the extent actual lotteries make statistical approximation redundant in analysis with my natural experimental sample, a conservative empirical approach in estimating Universal Pre-K’s effects upon preference

29The National Institute for Early Education Research (NIEER) at Rutgers University compiles an excellent series of annual reports on State Preschool Programs. For the 2019 report, see “The State of Preschool 2019”.

27
change among Applicants (N=79) provides unusual confidence for presuming “as if random” assignment among Constituents (N=172) in the second quasi-experimental set of hypotheses tests.

The partial nature of public investment responsible for comparable untreated parents is also important to the tractability of theoretical insights. As universal public preschool uptake approaches 100% for age-eligible children in places like Washington D.C., the loss of cross-sectional variation in access to valued social benefits among qualified claimants would simultaneously erode a great deal of generalizability to citizen-level political feedbacks affecting different places, policy spaces, and citizen subpopulations within the United States. Near-universal participation in public preschool should give rise to very different politics within a jurisdiction, which may offer an aspirational example but severely limits potential to glean broader lessons about ways in which characteristically inadequate and arbitrary social provision is implicated in modern American challenges of fractious citizenship and imperiled democracy.

By extension and methodologically most-important, such an inquiry would preclude chances to observe political transformation in progress, as parents experience free high-quality early education in context where due claims to public preschool cannot be taken for granted. Where public preschool access is truly universal, parent expectations and broader public opinion should catch up to policy on the ground; once truly-universal preschool is established within a community, my theory of policy-adaptive political preferences implies generalized local expectation of government

30 A of 2019, NIEER reports 87% of all 4-year-olds and more than 70% of 3-year-olds residing in the nation’s Capital participate in the City’s Universal Public Preschool program.
responsibility. On an at-least intuitive level, citizens’ should come to understand that early learning essential to success in school, and life, begins years before kindergarten, once PK-12 public education is routine in their locality.

Reaching just over half of the Empire State’s 4-year-olds and a small share of 3-year-olds, New York’s preschool program covers a very large absolute number while leaving most eligible children dependent on private alternatives to their common public claim. Very large potential and actual beneficiary populations distinguish New York State’s program from similarly non-means tested initiatives in large American cities and smaller states. With elbow grease and some ingenuity, rare program size is the final critical program feature which permitted me to recruit Universal Pre-K eligible parents and independently measure over-time political change, if any, attributable to the largescale randomized preschool experiment without reliance on sensitive application and lottery data from the state or collaborative preschool program administrators with uncanny time to spare.31

Before theorizing parent-level adaptation in childcare preferences according to preschool access and differentiating my theory of political adaptation from plausible behavioral explanations for covaried attitudes and arrangements, I underscore essential dimensions of political context and policy design along which universal preschool in NY

31To be sure, I welcome collaboration with any interested state or local public preschool program! Initial efforts to pursue this study through partnership with a public preschool program were unsuccessful. Combining IRB-granted levels of access to individualized applications and administrative data with permissible new research activities in school districts with robust universal preschool programs also proved insufficient for recruiting the panel necessary for this study.
State differs importantly from the Affordable Care Act, the primary focus of much recent feedback research for good reasons, mostly-unrelated to theory.

As the Affordable Care Act (“ACA”) bestowed millions of Americans with affordable high-quality healthcare, one way or another, the complex of reforms reliant on private sector and subnational implementation granted political scientists a plethora of empirical opportunities to more rigorously assess whether new policy in fact creates new politics (Schattschneider 1935). Yet, compared to civic transformation through experiences with Social Security (Campbell 2003), GI benefits (Mettler 2005), or cash entitlements conditional on Head Start (Soss 2000), the evidence of Affordable Care Act-reinforcing political gains among Americans helped by reform is modest over the decade since passage.

Key differences in politicization and design fundamentals between the ACA and NY State’s universal preschool program matter a great deal according to the experiential, adaptive view of policy feedback offered here. These differences help account for small and null effects in several studies of healthcare reform’s individual-level political effects and provide the final background before moving to parent-level theory, mechanisms, and competing hypotheses in Section III. Insofar as scholarship suggests policy design and political circumstance can engender or inhibit self-reinforcing citizen-level political

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32 Soss (Soss 2000) finds that AFDC cash welfare entitlements (eliminated in 1996) increased mothers’ internal political efficacy, but demeaning program treatment diminished assessments of government and discouraged political participation. Mothers with children also enrolled in Head Start were a critical exception, internalizing the program priority of community governance and coming to view politics as “open and democratic.” Michener makes a related about political (non)incentives when cut benefits are perceived as government incapacity (2018).
feedbacks measurable in the short-to-medium term, Universal Pre-K in New York State and the Affordable Care Act reasonably approximate best and worst cases.

**Political Context**

Passed without support from a single Republican after GOP elders had protracted Senate consideration in bad faith, symbolically “repealed” ceaselessly in the lower chamber until Democrats regained control, saved once with the silent thumbs-up of a cancer-stricken GOP Statesman, and before that by a centrist Chief Justice whose swing-vote is likely now insufficient to preserve civil rights on which Americans rely. Unimplemented still today for millions of working poor Americans, disproportionately Black and residing in the southeast, because about a dozen Republican Governors refuse stunning federal sums available for extending Medicaid to citizens under their jurisdiction in most matters pertaining to the quality and accessibility of public goods, even nominally-federal ones. Debate, passage, and implementation of the Affordable Care Act offer a study in racialized and spatialized partisan polarization’s detriments for American governance and democracy today.

Unsurprisingly (Druckman et al. 2013), mass spillovers of bitter elite polarization have been evident in public opinion about the Affordable Care Act from well before the

33 As Jonathan Cohn reports, GOP Senator Grassley (R-IA) eventually admitted to President Obama that, in principle, no set of amendments could secure GOP support for legislation championed by his White House (Cohn 2010).

bill became law. Prior to passage in 2010, never mind chances for personal experience with the most substantial reforms since 2014 with launch of the individual marketplaces and expanded Medicaid in select states, scholars find strong evidence that party identification split American views (Kriner and Reeves 2014). Comparison to opinions of healthcare reform instead championed by President Clinton highlight the distinct work of racial animus in shaping survey opinions. Importantly, Tesler shows American healthcare policy views were thoroughly racialized via association with President Obama already by 2009 (2012).

Further evidence that elite party cues dominate most standard survey measures of ACA support comes in familiar findings of split views on the healthcare law referenced specifically, alongside wide endorsement of most provisions and goals, especially but not exclusively when unidentified as such. Moreover, such findings point to the possibility of disjuncture between citizens’ party-aligned survey opinions about the Affordable Care Act globally, and seemingly more substantive views about particular policy ideas contained therein; at different points in healthcare reform’s political saga, such a break could reflect legislative particulars yet to polarize the masses by way of partisan elites; after 2010 and especially 2014, it may be helpful healthcare reform provisions depolarized by personal experience despite strong party cues. In some cases, perhaps specific policy ideas are too appealing to expressesly reject in blinkered one-by-one presentation of discrete ACA provisions, designed in some sense to elicit the inconsistencies apparent. The last prospect raises the greatest interpretability challenges,

35 This dynamic is clear in Kaiser Family Foundation panel data preceding passage of the Affordable Care Act. For example, see pages 9-10 in the “Chartbook” summarizing Nov. 2009 poll results ("Chartpack: Kaiser Health Tracking Poll — November 2009" 2009)
and concern insofar as studies of the ACA’s attitudinal feedbacks invariably use survey measures of affect toward the Affordable Care Act explicitly and in total, as opposed to items tapping more basic views about whether, and to what extent, providing or financing healthcare is a government responsibility.

Effectively absent from the federal agenda since Nixon surprised his own Cabinet negotiator with a vitriolic veto of comprehensive bipartisan legislation in the final days of his first term (Jacobson forthcoming) universal preschool is not now subject to the fraught partisanship typical of nationalized social policy politics. This rarity, under-polarized policy issue space, importantly implies much greater latitude to sway citizen beliefs with impactful governance.

Experiences with government meaningfully constitute, constrain, and condition citizens’ expectations of the public sphere. This follows from routinization of previous political settlements in policy and also the basic nature of human cognition; summarizing for political scientists used to assuming rationality, Druckman and Lupia emphasize a fundamental upshot from cognitive psychology “personal experience affects the set of objects over which people have preferences” (2000, 4). In this light, early-childhood policy is a matter on which most Americans should have very little pertinent information, let alone affective partisan reactions.

Flexibly measured to allow for parent preferences which vary between infants, toddlers, and preschoolers on the question of most appropriate care providers (Figure 5.6),36 group average preferences for public childcare, over private alternatives, illustrate

36 Attitudinal outcomes are graphed by preschool-group and discussed descriptively in the final, outcome-focused, subsection of Section V. See, Figure V.3.1.
one such manifestation of status quo policy in measured “public opinion.” Supremajoritarian endorsement by roughly seven in ten of public kindergarten over purchased and family-provided private options combined. When it comes to four-year-olds, support for the same closely tracks the age group’s actual UPK incidence. Average preferences for public childcare arrangements drop precipitously when children younger than these norms are considered. All else equal, little-to-no polarized prior information and low floors for the youngest children grant wide room to change American minds through direct experience with Universal Pre-K.

Assessing the ACA through the same mass partisanship lens, first experiences following implementation faced something closer to ceilings in support and opposition from Democrats and Republicans, respectively. After years of elite partisan warfare, it is not surprising that Obamacare had to literally fix hearts to change American minds. Having endured more than a decade of multiply-situated Conservative attack, in recent years the accumulation of personal, familial, and community benefits from healthcare reform at last appear to engender increased support for the Affordable Care Act; American public opinion now seems to reflect national experience with 30 million fellow citizens acquiring affordable care. It is notable that many quantitative ACA feedback studies allow little-to-no time for actual experiences with the legislation’s most substantial provisions implemented since 2014 before assessing positive policy feedback prospects to be meager, a point revisited in later sections.

There are also fundamental reasons of existing policy infrastructure for measured optimism about universal public preschool’s prospects for mass political acceptance, relative to the polarizing nationalized experience before the Affordable Care Act showed any hint of broad acceptance. To the extent universal preschool programs are administered by public school districts, as in New York State, this transformative social
policy may also sidestep potential to provoke the racialized public scorn characteristic of welfare politics in the modern United States (Gilens 2009; Soss and Schram 2007; Mettler 2018). Pre-K funding comes from all levels of government, in place-specific mixes. Hyper-local administration in this policy space means that the overall adequacy of public funding should matter a great deal, while the relative shares of local, state, and federal contributions makes less difference.

To be sure, American public education is no less racialized or spatialized than healthcare. But administrative differences in kind are important in this policy comparison. The profound sense in which decentralization breeds inequity is plain in public education, as Michener (2019) notes, typologizing education among the social policy domains most prone to racialized feedback dynamics. Deeply racist and intergenerationally disparate ways in which locally financed and governed public education has institutionalized in segregated residential space suggest that party-line mass polarization of opinion should be exceptionally less likely than proven in the superficial racialized national climate in which political preconceptions hardened years before Americans had chances for direct personal experience with reform’s health and economic benefits. Considered against decades of Democratic Presidential failure culminating in Obama’s expansion of federal involvement in entrenched, for-profit, private systems of healthcare delivery and insurance, the hyper-devolved governance and routine local politics of American public education raise doubt that adding an earlier year or two to school risks a more-polarized polity.

Given highly-local administration within space pervasively sorted by race, class, and now politics, universal preschool broadly experienced as an extension of familiar public education entitlements for slightly older children bears promise for bipartisan appeal exceedingly rare in modern American politics – albeit mostly within separate
residential enclaves. Comparing recent trends in views of federal responsibility, PEW notes two matters of broad bipartisan agreement on which Democrats and Republicans agree national government should play the leading role – (1) clean air/water; and (2) high quality K-12 education. For the other five measures studied, “white adults, those ages 65 and older, and people with higher incomes are far less likely than those in other groups to say the government has a responsibility to provide services” (PEW 2021).

In what seems the worst and most-likely case for mass politics, universal preschool should map onto race and place-based disparities institutionalized in American public education. All evidence suggests that universal public education started earlier will profoundly improve chances that children born with the least have early opportunities critical to keeping up with more privileged children through and beyond K-12; thoroughly studied income-targeted preschool pilots now decades old make it clear that earlier public education has profound benefits even when preschool begins under the same segregation and inequality endemic to the United States’ public schools.

More ambitiously, any number of adjustments could reasonably advance long frustrated integration goals in public education. Extension of public education to younger grades provides unique chances to rebuild relatively less racist and unequal public education system from the ground up. Imagining the particulars is beyond my present scope, but at minimum findings from this study suggest integrated preschool assignments would likely be sticky for many families otherwise inclined to enroll in public kindergartens funded and populated within-enclave. Decentralized public

37 The five other responsibilities asked about were: health insurance, adequate retirement income, an adequate standard of living, access to high-speed internet, and college education. See, PEW 2021.
education devolves control to relatively homogenous communities, self-selected in some measure, and should therefore engender mass politics quite unlike the affectively polarized national environment in which negative feedbacks of the Affordable Care Act have been more readily observed than positive shifts in political support, considering the runup to passage up through short-to-medium term aftermath of key provisions implemented in 2014.

Policy Design

Quick consideration of the Affordable Care Act’s design underscores characteristics common in contemporary American social policy that scholars find to minimize, delay, and perhaps thwart feedback processes, which again render recent federal healthcare reform importantly unlike New York’s State’s supply-constrained universal preschool program insofar as observable short-term political effects are likely.

Decades of American social policy scholarship establishes generous entitlement-based benefits provided by government in reliable and roughly uniform fashion as ideal, insofar as social policy can be crafted to foster self-reinforcing (“positive”) political dynamics among beneficiaries and broader publics. More aptly a bundle of modest federal interventions with collective systemic impact than a federal public healthcare program, the Affordable Care Act raises red flags along each key policy design dimension deemed important by prior feedback research. Reliant on existing private systems of healthcare provision and insurance, plus state governments for public assistance-based

38 In the healthcare realm, the UK’s beloved National Health Service comes close.
fixes for the stigmatized poor, the ACA’s variety of subsidies, taxes, and rules aim to address diverse access and affordability issues faced by differently-situated Americans.

For select citizens, direct material gains under the Affordable Care Act are substantial. But terms of coverage and cost savings are complexly conditional on factors including age, income, employment, and family arrangement, interacted with place of residence (Michener 2018). In the most unjust instance, millions of disproportionately Black and Brown working poor Americans with the 21st century misfortune of residing below the Mason-Dixon line, roughly speaking, remain stranded in the same coverage gap where they started, more than a decade ago. This stands out against much greater success bridging pre-ACA gulfs faced by sympathetic target populations (Schneider and Ingram 1993), including young adult dependents with relatively well-employed parents, or Americans with once uninsurable “preexisting” medical conditions, in ways which begin to paint the challenges of coaxing a constituency from legislation with such disparate citizen-level solutions, and real world downstream implications.

Far from an overhaul of the expensive American system in which business-tax deductible employer sponsored health insurance has long been cherished but eroding bedrock, the Affordable Care Act all-but-inevitably layered atop the United States’ divided public-private healthcare infrastructure (Hacker 2002). By addressing major

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39 For example, recent high school and college graduates entering the workforce and now able to stay on parents’ insurance until the age of 26. Those with once unaffordable conditions similarly stand out.

40 As Democrats in Congress work toward two-track hard and human infrastructure objectives in the summer of 2021, powerful Black Democrats from southern non-expansion states including GA Senator Raphael Warnock and House majority whip James Clyburn have begun making this point to argue for a new Medicaid like stopgap (Roubein and Ollstein 2021).
failures and shortcomings of the United States’ systems for healthcare provision and insurance President Obama’s signature legislative achievement further entrenched prior arrangements in important respects (Hacker and Pierson 2018).

Under the guise of upper-tier job benefits that cover a declining majority of Americans, taxpayer funded employment based private health insurance epitomizes one form of decentralization prevalent in contemporary American governance — attempted devolution of public goods and services provision to private sectors and for-profit third parties. Tax incentives are primary and the best studied instrument. In sectors including healthcare and childcare, fixed-rate federal payments directly to private practitioners also play an important role. Notwithstanding budgetary equivalence between spending and forgone tax revenue, political scientists find large political differences between conventional publicly-provided benefits or in-kind services and tax expenditures, in terms of policy effects as well as origins (Mettler 2011; Howard 1997; Hacker 2002). As a free market alternative to public health and childcare services, government subsidization of private care providers may similarly distort citizen perceptions of federal sponsorship.

Disparate solutions contingent on position within the lifecycle, income distribution, and workforce are complicated fifty-fold by state-level implementation of the Affordable Care Act. Red state refusal to expand Medicaid at a meant-to-be-irresistible rate is only the most glaring geographic inequity due to subnational differences in implementation of key ACA provisions. American experiences with the law’s individual marketplaces also differed tremendously across states after 2014 in ways likely to undermine the healthcare package’s short-term capacity to increase survey-measured support among exchange plan purchasers. My intuition about the legitimacy threat posed by unreliable benefits of varied quality appears consistent with ACA feedbacks findings to date. In a rare study considering the individual exchanges as a
potential source for citizen-level political feedbacks, Hobbs and Hopkins find that localities subject to soaring exchange premiums also see ACA attitudes sour among price-affected citizens (2021a).

Free high-quality public preschool, on the other hand, is theorized to have systematically large and quickly tangible effects upon daily life for family members in two-generations at once. Lifelong harms of material deprivation in the first years of development are difficult to overstate – as is the function of universal preschool toward empowering families to avoid poverty and enjoy some semblance of security while raising the next generation in all rich democracies today, bar this one. 41

From the perspective of children born outside of privilege as well as the public purse, free high quality preschool’s lifelong learning and economic benefits are concrete (Gray-Lobe et al. 2021; Heckman and Karapakula 2019b; Sprung-Keyser and Hendren 2020; Heckman 2006). Since LBJ’s Great Society, an educational head start has been increasingly well understood to uplift life chances for poor children, raising high school graduation rates and earnings, among other proxies for adult “success.” Rising income inequality and deepening insecurity among American families in intervening decades lead to the contemporary conclusion from Boston’s universal program: effectively all children benefit a great deal. Assessing educational attainment through college among children randomly assigned to public preschool between 1997-2001, economists no longer find income or race differences in the degree to which participation in high quality early childhood education significantly benefits American citizens (Gray-Lobe, Pathak, 41

41 Among a range of family policies and parent-friendly workforce regulations standard in much of the OECD, from paid maternal and parental leaves to child/family allowances/benefits.
Received wisdom about educational attainment and political participation suggest that Universal Pre-K-treated Bostonians now young adults are likely more engaged democratic citizens than peers less lucky, about twenty years ago, in a preschool lottery now known to have been fateful.

Decades of preschool evaluation research in a psychological vein underscore that efforts to promote upward mobility through public education rely crucially upon equalizing school readiness on entry to kindergarten; the extremely early, cumulative nature of human development makes this so (Zigler and Muenchow 1992; Raver and Zigler 1997). Insofar as citizens readily recognize this rapid progress, parents are plausibly more prone to swift political adaptation watching children benefit than would be true if themselves targets to similar public investment. Even permitting that something may be special about policy feedbacks when one’s child is primary beneficiary, transformative personal policy experiences are not in principle limited to Universal Pre-K. Considered at the citizen-level in further detail next, in Section III, comparably life-changing government benefits are rare before retirement in the United States.

As it is done in New York State, Universal Pre-K is unmistakably government. The preschool program is administered by hyperlocal independent school districts, with most preschool classes housed in public elementary schools. Focusing on direct implementation rather than the nature of preschool benefits per se, unambiguously public and routine provision of (P)K-12 education stands out from American social policy norms, typified by the Affordable Care Act in respects including decentralized, largely-private administration as well as racially inflected and typically rancorous national politics.
By this reasoning publicly subsidized private provision of childcare under existing income targeted schemes, as well as proposed more-nearly universal ones, should not have the same profound political feedbacks of publicly-provided early care and education programs like the one studied here.42 Rendering government invisible from the ground, devolution such that private intermediaries deliver government perks biases public appreciation, plausibly leading citizens to habitually misallocate reputational rewards to private points of interface, due credit American government could badly use with almost everyone in the United States today.

The third criteria, reliability, is where raffling of short-supplied preschool entitlements throughout New York State obviously falls short. From state differences in federal benefit eligibility and generosity and hyper-locally funded public education to impossibly long waitlists for housing assistance and inaccessible by design state-managed insurance for the unemployed, characteristically unreliable support from government erodes democratic legitimacy in the United States. As noted above, and further discussed in the concluding section (VIII), partial public investment and random allocation under New York State’s current implementation of universal preschool combine with unforeseen perverse political consequences of at least two kinds. Forced resort to private early education alternatives constricts the natural base of core policy support for public preschool. At the same time, randomized access very likely distorts general lessons about American government, and the value of engagement, which

42 The means-tested federal childcare vouchers programs administered by each state offers a helpful example for considering potential inadequacies of public subsidies to prevent childcare market failures, especially in rural and lower-income communities. Reaching about 7% of children poor enough to qualify, the CCDF program is notorious for maintaining and freezing waitlists, as well as too-low provider reimbursements which leave marginalized citizens even fewer childcare options.
winning parents glean because they come to acutely appreciate Universal Pre-K’s benefits as they live them, more and more over months.
III. DIVERGENT PRE-K EXPERIENCES & POLITICAL ADAPTATION: theorizing patterned parent preferences & preschool arrangements

In childcare, like most spheres, Americans rhetorically relish ‘choice.’ But the private responsibilities which frame these choices strongly induce one among few set strategies for fulfilling childcare needs (Pierson 1993). Mothers’ (non)policy constrained decisions to intermittently deprioritize paid work and personally provide care before kindergarten, or alternatively, with a partner to jointly toil toward affording early care and education at market rates, powerfully reshape future beliefs, behavior, and propensity for political engagement.  

Two primary maternal workforce strategies for privately meeting childcare needs, workforce withdrawal and full-time dual breadwinning, are not typically available to all American families, let alone in equal measure. Yet once one or another approach within the two extremes is adopted, self-identity often becomes sincerely tied to daily work-life circumstances, importantly including class-stratified childcare practices. Material differences in mothers’ likelihood and gains to future work mean that economic impacts of episodic stepping away versus continued career building through early motherhood reinforce work and marriage-based identity divisions.

More than removing childcare from politics, private responsibilities gender and personalize the high stakes; American mothers blame themselves (Collins 2019), and shame each other, for predictable consequences of little-to-no government investment in the critical first few years. Amid gendered and racialized economic disparity, widespread

43 Mothers are default primary caregivers, and when coupled and working, almost always the second earner. Although gender neutrally targeted to recruit Facebook-identified parents with preschoolers my ads overwhelmingly reached mothers (165 of 172 in quasi-experimental sample, as detailed in the 4th section).
insecurity at the family-level thwarts broad-based support among mothers themselves for gender equality in principle, as well as policies intended toward achieving it (Iversen and Rosenbluth 2010; Kuziemko et al. 2018). For straight mothers with (white) working partners, (white) male privilege is generally essential to the family’s bottom line. This codependence entrenches relative advantage for some family-types within a distinctly-American childcare status quo, suboptimal from any perspective except the effortless transmission of status intergenerationally within families.

Parents with any advantage face compelling incentives to hoard in children’s interest and invest what they can to secure future success for their own (Grusky et al. 2019), buying into better neighborhoods, or elite universities in the limit. Free, high quality, universal public preschool promises a partial antidote to zero-sum parenting induced by the historic paucity of public investment in American early childhood.

By way of much more nearly uniform, educational, and affordable early arrangements, universal preschool generates mutual recognition among politically and demographically diverse parents of pronounced new interests in government playing a substantial childcare role. Residential segregation patterns suggest many undergo belief change physically proximate to parents who mostly look, live, and vote similarly. Nonetheless, within their respective communities across New York State, Universal Pre-K Parents’ attitudes should adapt in substantively similar fashion—becoming much more inclined after one semester to consider childcare a government responsibility in every sense.

Among unretired Americans unaccustomed to direct, dignified, meaningful, and unabashedly-public social benefits, universal preschool is also a prime opportunity to
restore, or build anew, appreciation for government’s potential as a force for good. How better than literally to establish government cares about families?44

As a valued and visible most likely case for short-term positive and negative feedbacks, New York State’s universal preschool program also offers an unmatched chance to rigorously assess whether policy changes politics at the citizen-level, and if so, how.

Situating partway-funded Universal Pre-K within generations of scholarship on citizen-level political feedback in American politics, Section III develops a tractable theory of policy-adaptive political preferences against two emergent alternatives privileging personal traits to account for modern feedbacks. I argue that direct and generous benefits can profoundly change individual policy preferences and enhance citizens’ subjective sense of democratic incorporation in contemporary American political context. By the same mechanism, experiential preference formation, I theorize government inaction as the cause of specifically patterned parent preferences for private childcare responsibilities too. Nonpolicy makes politics.45

The third portable insight gleaned from treating attitudes as adaptive to impactful government encounters concerns benefit uncertainty, and ways in which the reliability of social provision affects potential for changed policy views to translate into

44 This is essentially the calculus behind the Biden Administration’s American Families Plan, included within the so-called “soft infrastructure” package Democrats hope to pass through budget reconciliation in fall 2021.

45 My claim is deeply influenced but distinct from, arguments about policy drift, purposeful non-updating of extant policy when changed conditions demand, as well as claims linking private social benefits like tax expenditures to distinctive feedbacks. Here, nonpolicy in the form of partial-funding necessitates politically important private strategies for New Yorkers without access to a public pre-k seat.
more politically efficacious beneficiaries with some reasonable motivation for deeper
democratic engagement in the future. In cases like New York State’s (kinda-sorta)
Universal Pre-K program, benefits may transform beliefs about the desirability of
government intervention and substantially enhance perceived civic worth during a
demanding but unsupported lifecycle moment, at the same time the policy’s
implementation strongly conveys to citizens—charged in this instance with raising our
next generation—that American government cannot generally be counted on. At least as
important, uncertainty strips parents of political agency when successful in claiming the
life changing benefits increasingly understood as vital to children’s wellbeing and future
success (Soss 2000).

**Direct Policy Experience versus Personal Traits**

Flipping the expectation that citizen preferences drive democratic policymaking,
I argue that impactful personal experiences with current policy beget individual desires
about the role of government. Specifically, I leverage New York State’s short-supplied
universal preschool program to develop and test the hypothesis of policy-adaptive
political preferences against two alternative mechanisms of attitude formation forwarded
in recent feedback studies for missing, mixed, or mostly-negative (policy-undermining)
observed political effects of social policy today. Beyond the substance of policy
experience, individual traits important in conventional behavioral and psychological
models of preference formation importantly underlie parents’ varied childcare
preferences under each competing claim.

Rather than political ideology, self-interest, or any parent-level factor(s), I
theorize divergent lived experience with a shared claim as the cause of universal
preschool beneficiaries’ distinctly public childcare preferences, as well as the more-private beliefs about appropriate early arrangements developed by parents with UPK-eligible children who do not have UPK seats. Class-specific private substitutes necessitated by nonaccess to due public preschool also give rise to arrangement-specific expectations of public involvement in early childhood, along distinct practical and financial dimensions of responsibility for care before school age.

Building upon classic accounts of policy feedback as developmental at the individual, group, and institutional-levels alike (Pierson 1993; Mettler and Soss 2004), I expect personal experience with valued and visible public preschool benefits to change parents’ childcare attitudes over months, splitting preferences among citizens with similar policy stake as families settle into distinct routines.

Substantial enough to reroute daily life and shape lifetime prospects for two-generations within each family with a seat, personal experience with Universal Pre-K should also, on average, transform parents’ childcare preferences, increase expectations of government responsibility, and improve perceptions of own civic worth within a semester. Similar political transformation is rare but not unheard of in American social policy (Campbell 2003; Mettler 2005), when public benefits are economically empowering and positively constructed (Soss 2000; Schneider and Ingram 1993).

Literally life changing magnitude and unambiguously-government, Universal Pre-K seats awarded without stigma evoke parallels to Andrea Campbell’s paradigmatic positive feedback case, “senior citizens” cultivated through enactment and expansion of the Social Security program (2003, 2002).
Bestowing substantial material resources (monthly checks), political legitimacy, and collective identity upon discrete Americans otherwise structurally prone to poverty unless wealthy, Social Security is unique in empowering a cohesive constituency across class lines, rooted in mutual interests following from Americans’ position within the lifecycle, not the income distribution or political spectrum. High levels of engagement among even low-income seniors in turn helped to entrench and grow Social Security over generations into the political third rail which retirees broadly and deeply depend upon in the United States today.

Social Security benefits provided $\frac{1}{2}$ of the average retiree’s income and $\frac{4}{5}$ of livelihood for the poorest 40% among elderly Americans around the time of Campbell’s study (Campbell 2002). It is difficult (likely impossible) to find another universal benefit which regularly doubles disposable income for non-poor families in the United States — except public education. While family budgets presume reasonable availability of free public options for K-12 education, extending the familiar to younger children promises an opportunity for substantial income gains to virtually all families who partake. Venturing into the vicinity of Social Security’s magnitude in a range of common scenarios, a free public preschool seat boosts potential family income through two non-exclusive channels: less private childcare spending and more maternal earnings. Recall, the pre-pandemic average price for a preschool seat in New York, $13,000 in 2019, was just more than half of the full-time annual earnings for minimum wage workers in the State — a population disproportionately comprised of mothers, especially mothers of color.

Even when mothers are not actually earning during the preschool week, freed time for non-childcare grants an important modicum of financial security in the possibility to pursue paid work if need be or desired. Relief from round the clock
childcare obligations may improve mothers’ life quality in ways independently conducive to enhanced personal agency, granting peace of mind with extra bandwidth, or more literally, extra joy in repurposing of weekly hours formerly devoted to childcare toward more favorite activities. In this sense, changed daily existence once a child begins public preschool raises potential for an association between benefits and heightened civic worth which need not coincide with improved general evaluations of government or estimations of what one’s engagement might accomplish.

In the Social Security case, seniors’ economic dependence on benefits prompted self-interested protective voting and eventually attracted organizational sponsorship essential to forging seniors into a formidable political force. But these feedback processes took decades on Campbell’s account, with distinct political efficacy among beneficiaries only arriving on the tail end of major developments (Campbell 2003, 2002). I expect transformative political change much more quickly among parents once personal experiences of Universal Pre-K diverge, on pace with Suzanne Mettler’s account of midcentury community participation precipitated by GI educational benefits for white soldiers returned from WWII (Mettler 2002).

Within a broader set of veterans’ benefits credibly culpable for the modern racial wealth gap (Katznelson 2005), Mettler links publicly funded higher education and vocational training to near-immediately heightened civic and political engagement among highly-educated and efficacious veterans in the postwar era, 1950-1964 (Mettler 2002). Importantly like Universal Pre-K and unlike monthly checks to the elderly, GI educational provisions promoted “citizen involvement in the day-to-day democracy” (Mettler 2002). Drawn into the public sphere by a preschool seat, parent engagement day-to-day should raise recognition of one’s community stakes with similar haste. For veterans, higher education courtesy of the grateful taxpayers functioned as a
participatory stimulus. By changing views about who government is by and for, the unexpected opportunity of college enhanced not only capacity but a predisposition for democratic engagement among white veterans who exercised claims. So too should public preschool generate rapid political change in parents, as routine experiences of democracy instill a fuller sense of incorporation and concretize the family’s interests in local affairs.

Patently lifelong impacts of public investment in very young children are also important to my expectation, substantial short run adaptation in parents’ childcare attitudes once preschool is underway. Here again, midcentury government investment in college for a (white male) generation returned from war is the most instructive classic case of positive feedback. No matter how deserved and deservingly constructed as an entitlement most Americans eventually intend to claim, living money for the elderly is not economically productive in the literal sense of public spending on extra-education, be it before or after the K-12 coverage already assumed as a right of American citizenship.

The productive nature of public investment in oneself importantly contributed to fast acting feedback effects in Mettler’s account of veterans. Political changes most pronounced among citizens afforded upward mobility by GI education benefits, veterans from less (but not least) privileged backgrounds, provide evidence for this claim (Mettler 2002, 2005), fulfilling a role similar to low-income highly engaged seniors in demonstrating Social Security’s feedbacks (Campbell 2002). College attainment, as well as the higher personal and household incomes which typically follow, are pervasively documented to increase participation in American politics (Brady et al. 1995).

Importantly augmenting these direct material gains in political capacity, Mettler emphasizes interpretive effects by which hard assets bestowed in the GI bill also carried
motivational political lessons. Perceptions that the government claim meaningfully improved life circumstances and lifetime chances were critical; college so greatly boosted veterans’ political engagement because it was publicly-financed (Mettler 2002).

To the extent citizens are plausibly more affected as witnesses to the blossoming of their babies than if they themselves received comparable public investment, the profoundly productive focus of policy should be even more important to politics produced by the government program at hand, Universal Public Pre-K partway-funded by New York State. Most parents cannot help but appreciate rapid developmental progress once a child begins preschool. Over months, mounting personal experience with a 3–4-year-old benefitting from free high-quality early education generates qualitatively new beliefs in parents about the types of arrangement very young children need to thrive, and in turn, greater expectations of government involvement, both practical and financial.

In an important sense, however, public spending on universal preschool does double as direct investment in parents who most need extra support. Viewed through the second lens of a workforce supplement, public preschool should be most meaningful for parents likeliest pushed out of paid work without it – mothers with low-to-moderate potential income and a partner who earns more. Among all Universal Pre-K beneficiaries, families likely to coordinate paid work and private childcare responsibilities with fewer than two full-time breadwinners because purchased options are cost-prohibitive stand to gain the most from more gender-equal opportunity to pursue paid work. Stark discontinuity in workforce participation among mothers, but not fathers, before and after youngest children reach kindergarten in the pre-pandemic United States offers strong evidence that partial but regular daytime coverage, reliably Monday through Friday barring set breaks and summers, should support much more
maternal work. Empowering more mothers to maintain and invest in careers, job aspirations attributable to earlier public coverage may further enhance agency through improved notions of own standing within the American polity, society, and workforce.

Finally, preschools themselves anchor my expectation of large and relatively-rapid adaptation in beliefs about public childcare responsibilities among parents fortunate to have a Universal Pre-K seat, and conversely, peers who must pay dearly for formal private substitutes. Alongside the provision of education and enrichment for children, preschools fundamentally serve as sites for parent political learning and socialization within context of New York State’s short-supplied entitlement program. Institutional and organizational attributes of universal preschool profoundly contribute to this policy’s potential to empower citizens and enhance democracy. Within expensive private settings, these same features of preschool instead preserve advantage and promote “meritocracy” for purchase.

Universal Pre-K is different from benefits paid directly to citizens insofar as claims (in the pre-pandemic period considered) focus policy-relevant activity, information, and interests in one location. As a practical matter, public provision physically concentrated at a particular site ought to increase citizen interface in ways which substantially raise the chances beneficiaries become a mutually-recognized constituency with means and realized incentives to collectively pursue politics. Even in the case of Social Security, the quintessential direct benefit, Campbell underscores the importance of focal places in helping seniors with fewest private resources overcome political participation barriers (Campbell 2002). Community-based senior centers served informational and organizational purposes vital to stoking political interest and engagement opportunities despite lower socioeconomic status.
More than a neighborhood hub for public services and community interaction, public elementary schools normally institutionalize chances for citizens to participate in collective governance. Parent-Teacher Associations (PTA) and similar open membership groups exist to coordinate and channel parent voice into policies, programs, and procedures within the schools entrusted to educate their children. Well short of formally joining organized parent stakeholders, informal neighborly encounters within context of children’s school should at least facilitate build-up of “social capital” linked to individual political feedbacks in comparative research (Kumlin and Rothstein 2005), and American democratic vitality in influential work by Robert Putnam (Putnam 2000). Rarer yet, public schools offer routinized and frequently productive interaction with government officials, children’s teachers and school administrators.

Indeed, the particular promise of government-provided preschool as a venue for parents’ democratic learning and practice is underscored in Joe Soss’ archetypal study of citizen-level feedback across tiers and programs of American social provision (Soss 2000). Complicating neat dichotomy in the effects attributable to universal and means-tested public programs, Soss links simultaneous experience with a child in income targeted Head Start to significantly improved assessments of the American political system among poor single mother claimants of cash aid. Invasive and demeaning personal encounters with government in claiming cash benefits conveyed strongly negative, demobilizing lessons about how and for whom government works, but the subset of mothers who also received free public preschool were unique. Simultaneous

46 Mothers in Soss’ study assessed cash entitlements through the Aid to Families with Dependent Children (AFDC) program replaced in 1996 with time-limited non-entitlement cash benefits under the Temporary Aid for Needy Families (TANF) program.
with cash aid, means-tested preschool made poor single mothers substantially more likely to view American politics as “open and democratic” (Soss 2000).

Program design premised in the Great Society principle of “maximum feasible participation” encouraged among Head Start mothers a distinctive willingness to speak up within context of program participation, with spillovers to more positive beliefs about governance and democracy conducive to greater engagement in general (Soss 2000; Bruch et al. 2010). Focused upon the practice of collective decision making and setting policy within children’s preschools, activities and objectives essential to incorporating parents through Head Start align in substance with the aforementioned Parent-Teacher Associations.

In short, my expectation that personal experience with Universal Pre-K will transform childcare preferences, generating in parents desire for a substantially larger government role in early childhood within months, is grounded in foundational policy feedback insights regarding the magnitude of claims, degree to which they draw citizens into daily democratic life, clearly lifelong impacts of public investment, and localized provision at sites situated to double as community venues for parent democratic learning.

But uncertainty induced by New York State’s partial funding and random assignment undercuts some of these feedback-inducing attributes in ways likely to distort the transformative program’s political effects upon parents lucky to have Universal Pre-K seats because they are appreciative, and increasingly so. Again, I expect large and swift attitudinal feedbacks. However, changed views about what government can and should do, based in impactful policy experienced by chance, may subsist with
skepticism about American democracy in general and the utility of personal engagement in particular. What the United States will do for its citizens.47

Soss’ classic comparative case study of welfare claiming is illuminating here as well. The very same poor single mothers discouraged from voting by experience with a hostile welfare system derived consequential agency in the political exercise of unwanted claims (Soss 2000).

By enabling poor mothers to meet material obligations to children and maintain some independence from exploitative private relationships (domestic; employment; housing; etc.), resource gains through program participation also facilitated the self-respect and social dignity necessary to fully exercise political rights within their communities. More-than-material benefits built up personal political confidence.

Fundamental to Aid for Families with Dependent Children (AFDC)’s potential to empower citizens with heightened political capacity at the same time that program experiences depressed electoral participation, the entitlement-basis of cash assistance for poor families before 1996 created expectations necessary to motivate mothers’ demands on an American safety net eager to shirk. Mothers’ expectations were in turn essential to framing opportunities for enhanced political agency in successful struggles with the state to wrest rightful support. Direct access to public resources – the “signature

47 The COVID-19 pandemic confounds measurement of meaningful change in actual political participation. NYS shut down about two months after my January 2020 wave. For example, the first election following preschool-treatment was the state’s democratic presidential primary. Originally set for April 28, 2020, the primary was postponed until June 23, 2020 due to the pandemic. More generally, in-person and online forms of political participation changed profoundly after the second wave of my survey. Similar issues of interpretation with internal and external measures of political efficacy after January of 2020 inform present focus on Universal Pre-K’s feedback effects over one semester only, pre-pandemic.
feature” of benefit claiming as political behavior – was the linchpin to gains in perceived political capacity on Soss’ account (2000), even when program participation did not also yield views of government and politics as systems susceptible to beneficiary influence, but rather the opposite for welfare mothers who did not also have public preschool through Head Start.

As a particularly blunt instance of the benefit uncertainty characteristic to contemporary American social provision, lottery assignment of limited Universal Public Pre-K seats likely devastates the empowering citizen-level link between securing a valuable public benefit and improved assessment of personal political capacity. Random access robs parents of political agency in claims to early education, understood, more and more, as essential to preschoolers’ palpable progress, while simultaneously raising chances that government shortcomings will come to be understood as a reflection of too little will and not a lack of way.

Transformative yet supply-limited preschool benefits establish that government has tools to better equalize opportunity and improve life for all families in ways that might, under certain circumstances, exacerbate the likelihood among parents with seats of inferring the worst about government’s chief concerns and motivations from the matter of partial public funding. Luck of the draw also erases parents’ direct contributions to children’s developmental success in ways which could make extra-salient for some citizens a lack of personal agency in seeing to the success of their children.

Alongside perverse interpretive effects of lotteries – affecting external and internal assessments of political confidence because beneficiaries appreciate preschool – material effects of the public program are also undermined by the unreliability of seats
awarded a few weeks-to-months before preschool starts. Non-universal access to the entitlement should constrain the second channel for increased disposable family income with receipt of a Universal Pre-K seat – more maternal work, and particularly, more gender-equal career investments while children are young. Late timing and unpredictable allocation considerably exacerbate the maternal career consequences of public underfunding; insofar as increased workforce participation is the pathway for potential gains most available for low-to-average income families, feedback-diminishing consequences of unpredictable early education access should be class-biased. Most common among couples already full-time dual breadwinning, boosts due to saving about $13k annually on private tuition are not time sensitive in this same corrosive fashion.

Whether purchased or family-provided, private childcare options have the inverse feedback implications following from relative certainty. This dynamic is not essential to the politics of preschool or the nature of public benefits per se, but rather a consequence of non-universal access to early education entitlements in New York State, aggravated by late and random allocation. Within context of local Universal Pre-K lotteries, private arrangements afford reliability and security in greater ability to plan. Paradoxical perceptions of choice in class-stratified adoption of private solutions should also permit substantial potential for enhanced personal efficacy in whichever non-government childcare strategy is pursued. This stands in stark contrast to the relatively depersonalizing experience of witnessing one’s preschooler flourish because of a transformational lottery win.

Growing accustomed over months to life with a child’s expensive early education, *Private Pre-K Parents* with policy-adaptive preferences, as I theorize, will on average update September assumptions about the appropriate allocation of childcare responsibilities. Not elite tastes, conservative ideology, or economic self-interest, but
mutual and mounting experience of unrealized Universal Pre-K claims at exclusive substitute sites causes preschool purchasers to become more opposed to public provision of childcare over months, as new beliefs and desires emerge from new routines. Along the second, financial dimension of care before school age, lived experience should have the opposite impact on Private Pre-K Parents’ views of government responsibility. Time spent with costly and educational early arrangements generates in parents new appreciation for the profoundly resource-intensive needs of very young children. In turn, a semester should increase demand for public financing among private preschool users much like their peers with free public seats.

Short-supplied Universal Public Pre-K stunts support for government responsibility in the childcare realm among No Pre-K Parents too. But this subset defined by nonaccess to formal early education undergoes neither a common shift in daily parenting nor, in expectation, systematic swings in childcare beliefs coincident with the schoolyear.

➢ **Hypothesis: Policy-adaptive political preferences**

*Life with a child in universal public (purchased private) preschool gives rise to new beliefs about the appropriate allocation of public versus private responsibility for providing childcare before school age, and as a separate matter, paying for it.*

*Among parents without formal early education, views should be stable, on average remaining near baseline low support for government provision and financing without regard for the preschool calendar.*

Measured against the empowerment of senior citizens (Campbell 2003), appreciative and engaged college-educated white GIs (Mettler 2005), and my prediction of more deeply incorporated citizens with transformed desires for government
involvement within months of a child benefitting from universal preschool, a new
generation of feedback research focused largely on implementation of the Affordable
Care Act yields meager evidence that the federal healthcare overhaul bolstered policy
support as it bestowed greater health security. Nonetheless, more robust negative
feedbacks observed in the same studies suggest differential policy exposure matters to
citizen-level political change. Section II made the case this has much to do with the
Affordable Care Act itself while Section IV, next, will extend these program-level claims
to considerations of research design.

Two broader interpretations advanced with mixed, missing, and mostly negative
modern feedback findings in recent research on and beyond the ACA lend the competing
parent-level hypotheses against which I test my theory of political adaptation in the case
of New York State’s kinda-sorta universal preschool program. Like political adaptation,
alternative plausible mechanisms of variation in parent attitudes – A: self-selection, and
B: maximization/retaliation – predict patterned childcare beliefs and preschool uptake
among the program eligible New Yorkers studied here. However, the three theoretical
accounts differ fundamentally with respect to cause, direction, and observable
implications delineated below, in the next subsection. Rather than divergent daily
experience with a common claim, personal traits underlie parent preferences for public
versus private assumption of childcare roles – political ideology/affect and self-interest
under Alternatives A and B, respectively.

The first set of arguments espouse a “conditional policy feedback theory,” with
restrictive demographic scope conditions imposed upon the citizen-level political effects
attributed to direct policy experience in classic accounts, and my own. Incorporating the
most prominent behavioral explanation for divided American policy attitudes into the
institutional phenomenon of feedback, political ideology and partisanship are theorized
to delimit particular subsets among citizens for whom policy uptake (Lerman et al. 2017) and experiential opinion change (McCabe 2016b; Lerman and McCabe 2017) are most (least) possible. Along with political identification, levels of trust in government and “political knowledge” further condition individual prospects for feedback in several accounts within this vein (Jacobs and Mettler 2018; Mettler 2018; Lerman 2018). For example, Lerman and McCabe (2017) argue Republicans, Independents, and the disengaged among 65 year olds are uniquely prone to update views upon Medicare experience because they otherwise resist positive elite messaging on public healthcare.

Among ACA beneficiaries, Republican identification, government distrust, and their combination are alternatively understood to dominate (Jacobs and Mettler 2018) or disrupt attribution (McCabe 2016b) of personal gains to federal reform. More fundamentally though, these prior political traits dictate individual likelihood of participation in expanded Medicaid and self-insurance marketplaces established under the Affordable Care Act (Lerman et al. 2017). Thus, in contexts like the preschool program investigated here (and unlike Medicare), where access is not nearly universal, one upshot of conditionality as theorized in addendum to conventional feedback theory (Lerman and McCabe 2017) is that citizens most susceptible to positive change through personal experience are least likely to encounter belief-changing policies due to the same traits. And vice versa, insofar as supportive beneficiaries self-select.

➢ Alternative Hypothesis A: Ideological and/or Affective Self-Selection

Liberals, Democrats, and/or parents highly trusting of government will select into Universal Pre-K lotteries because of predisposition to believe childcare is a public responsibility. (Among applicants, winners & losers randomly assigned).
Republicans, conservatives, and distrusting parents adverse to government-involvement in childcare will avoid UPK notwithstanding eligibility and split along class-lines into purchased or family-provided private alternatives.

Rather than differential response to the same policy opportunities or experiences on account of party or degree of trust in government, a second emergent class of claims instead emphasize self-interested and heterogenous feedbacks across citizens differently benefitted and burdened by policy (Hopkins and Parish 2019; Hobbs and Hopkins 2021a; Jacobs and Mettler 2018). A sometimes-implicit maximization mechanism understands promptly observed negative feedbacks as retaliation by Americans saddled with new costs or rules and expects (but less often finds) inverse support near instantaneously once other citizens realize beneficiary status.

Similar to my theory of political adaptation, recent explanations of policy feedback in this spirit focus upon ways in which new policy distinguishes winners from losers by distributing valuable benefits, as well as outsized-burdens (Jacobs and Mettler 2018). Unlike Alternative A (self-selection), causality under the maximization/retaliation hypothesis runs in the same policy-to-citizen direction (i.e., feedback) as my own experiential explanation; however, much faster “flip-of-the-switch” (Campbell 2020a) style effects are rooted in interests inherent to (non)beneficiary status rather than the substance of divergent personal policy experience.

Studying political effects of the ACA’s self-insurance marketplaces, Hobbs and Hopkins’ emphasis on insurance-status based discontinuity in ACA opinions “at precisely the moment when the exchanges opened” (2021a, 2) epitomizes the second alternative, self-interested maximization among beneficiaries offset by retaliatory shifts
from the extra-burdened. Motivating their study with descriptive analysis of inversion in group-average approval of the legislation among uninsured and self-insured Americans “almost immediately after implementation” (Hobbs and Hopkins 2021a, 7), the authors reveal a great deal about dynamics presumed responsible for individual policy attitudes through the specification of extremely-short time horizons for effects to take hold. In line with a vast political economy literature on individual demand for redistribution, self-interest follows axiomatically from classification under the reform package rather than adaptively from substantive ways in which new federal policy changed healthcare, and in turn, Americans’ expectations of government’s role therein.

➢ **Alternative Hypothesis B: Beneficiary Maximization & Offsetting Burdenficiary Retaliation**

*Upon becoming beneficiaries (burdenficiaries), applicant parents receiving (not receiving) Universal Pre-K immediately become avid supporters (opponents) of public childcare responsibilities in response.*

*Non-applicant private preschool purchasers & those who forgo formal early education should begin & remain opposed to public responsibilities.*

**Patterned Parent Preferences over Childcare Provision & Financing**

The three hypotheses which plausibly link parent preferences to young children’s care arrangements yield contradictory expectations about the timing and pace of processes responsible for varied childcare views, as well as essential qualities of the patterning produced in desires for government involvement among citizens with the same stake but disparate access to universal public preschool in New York State.
Only onset once preschool starts but cumulative, I expect substantial and split adaptation in average support for government-provided early childhood options in a few months’ time. Although rapid within the realm of positive feedbacks documented to reshape mass politics through American social policy, my anticipation of arrangement-based patterning in childcare attitudes after one semester lags by months and years the fragmentation in parent views under competing mechanisms of maximization and self-selection, respectively.

Simply put, this renders the mid-to-late September baseline post-treatment under either alternative, but well before attitude change on my account of adaptation through policy experience. As detailed within the context of research design in Section IV, next, my baseline survey and first follow-up fielded a few weeks into the fall and spring semesters of schoolyear 2019-2020 afford maximum leverage for distinguishing any impact attributable to parents’ divergent direct experiences with universal preschool from self-interest acquired with (non)beneficiary status, let alone political predispositions.48

Beyond these essential and testable (by design) temporal differences in each mechanism’s observables, my expectations of policy-adaptive preferences conflict fundamentally with predictions under the competing hypotheses regarding attitudinal import of actual rather than potential lottery participation, as well as the distinction purported thus far between practical and financial dimensions of parent desires for

48 Random allocation of limited public preschool seats and my within-parent estimation approach redundantly preclude any chance that political predispositions play a primary role in changes I attribute to parents’ divergent public and private preschool experiences. Section IV fully elaborates this point, although Figure 3.1 should make it pretty clear sooner.
government responsibility in childcare. Differences in the patterning of theoretical expectations between views on responsibility for provision, and separately, costs, as well as natural versus quasi-experimental subsamples of program-eligible parents are summarized with Table 3.2 in closing this section.

But first, Figure 3.1 grounds differences between adaptation, self-selection, and maximization as processes potentially responsible for parents’ varied points of view by plotting competing predictions about parent preferences for government-provided care among public and private preschool users over a full year, beginning the spring prior to preschool enrollment. Deeper-blue bands within the shaded regions marking fall and spring semesters of the 2019-2020 preschool year denote the dates of first and second round surveys in which parents’ childcare attitudes were measured. The UPK registration period and lottery dates depicted come from the timeline set by Buffalo CSD, selected as a large, urban, and non-NYC district to be “representative” of preschool application cycles scheduled and managed independently by officials charged with administering local public schools in the State’s eight hundred school districts.

Beliefs about Public Provision (Figure 3.1)

Figure 3.1 visualizes predicted trajectories in the first view, preference for publicly provided childcare over purchased and family-based private arrangements, under Alternative Hypotheses A and B juxtaposed with my own, crosscutting adaptation over months of divergent daily experience with transformative public preschool benefits.
Figure 3.1 Competing Mechanisms of Attitude Formation: Predicted Trajectories in Parent Preferences for Publicly-Provided Care by Pre-K Group

Notes: Because preschool application cycles are scheduled and managed independently by eight hundred school districts across New York State, the UPK registration period and lottery dates depicted are based on Buffalo CSD's timeline.
Within each panel of the figure, theorized average support for public provision among *Universal Pre-K Parents* is tracked in green over the twelve months beginning in April 2019, preceding the September start of universal public preschool; expectations about *Private Pre-K Parents*’ level of support for publicly provided options is shown in gray over the same year per each theory.

Starting from the top, the self-selection hypothesis inspired by theories of conditional policy feedback notably does not entail policy feedback in programs like the partway-funded preschool entitlement studied here. Undersupply increases chances that the program’s strongest proponents opt-in already nearing the support ceiling whereas citizens potentially susceptible to profound opinion change are least likely to apply for a chance at one of few free public preschool seats.

This is reflected in the top panel with three perfectly flat theorized trajectories in support for publicly-provided childcare. At the top of the top panel, *Universal Pre-K Parents* and *Lottery-Losers-turned-Private-Pre-K-Parents* depicted in dashed gray maintain comparably strong support, starting well before the period shown and presumably continuing long past.

Also in gray but at the bottom of the upper panel portraying self-selection, hypothesized preferences among *Private Pre-K Parents* who do not first try their luck for public seats sustain similarly-steady but low support for government childcare. In practice, this suggests null results in my natural experimental analysis, Part A of the Results Section (VI), while quasi-experimental comparisons between *Universal Pre-K Parents* and program-eligible nonapplicants without formal preschool will not turn up significant changes but should see profound group-level baseline differences.
Moving to the middle panel, the maximization hypothesis (Alternative B) roots split attitudes among lottery participants in the random drawings independently scheduled by each school district sometime during the spring or summer before free full-day preschool starts for lucky families who win. Like adaptation depicted beneath it, maximization is also an account of feedback which flips standard behavioral interpretations to explain opinion as a function of public policy.

In line with microeconomic assumptions about citizen-level politics following from workhorse political economy models of welfare politics (Meltzer and Richard 1981) and materialist theorizing of preferences, maximization depicted in the middle panel shows a sharp discontinuity in parent preferences “at precisely the moment” (Hobbs and Hopkins 2021a, 2) local random drawings differentiate unlucky Lottery Losers from Universal Pre-K Parents. Support for publicly provided care drops precipitously “almost immediately” (Hobbs and Hopkins 2021a, 7) among losing parents in dashed grey, while winning parents portrayed in green become strongly supportive just as fast.

At the bottom of the middle panel, non-applicant Private Pre-K Parents in solid gray sustain consistently low-support for publicly provided care arrangements beginning sometime prior to joining my panel and extending without much change through the end of the school year. Notably, for this quasi-experimental subgroup of well-off parents, over time predictions under maximization are observationally equivalent to self-selection. Table 3.2 recaps this difference among other conflicts in patterning between mechanisms in expectations contingent on lottery participation, and predictions along practical versus financial attitudinal dimensions.

Finally, the bottom panel of Figure 3.1 shows my hypothesis of split adaptation in preferences over government provision of childcare. Impactful and cumulative personal
experiences with universal public (purchased private) preschool drives adaptation in parent beliefs about children’s needs and government’s practical role in fulfilling them. On my account, winning parents do not become strong proponents upon winning draws or all at once with the start of school; by the same reasoning with respect to private preschool purchasers, lottery participation becomes irrelevant once preschool routines reroute parent perspectives about the desirability of public provision.

*Lottery Losers* who go on to purchase early educational substitutes are simply *Private Pre-K Parents* under my experiential theory of political adaptation due to policy; if mutual daily experience with pricey substitutes to short-supplied universal preschool truly causes parents’ basic beliefs about childcare to change, there is no need to differentiate with dashes parents who hoped for a free seat before investing in plan B.

If parent preferences are adaptive as I argue, any September differences in support between *Private Pre-K Parents* who do and do not participate in universal preschool lotteries will be swamped in substantive importance by similarly sharp downslopes in support for publicly provided options after the start of school as *Private Pre-K Parents* grow accustomed and appreciative of their early education investments.

**Beliefs about Public Financing**

At the same time as divergence in attitudes about public provision of care between public and private preschool users, my theory of policy-adaptive preferences predicts convergence in increased support for public responsibility along the second, financial dimension of childcare policy opinions. As before, my hypothesis of political adaptation yields weak expectations about baseline views in September and very strong
presumptions of change over time as parents live with a formal preschool arrangement, public or private in this case. Here, experience with the most-costly and school-like type of early childhood arrangements generates an expectation that government should foot the bill.

Taken together with predictions along the first attitude shown in the bottom panel of Figure 3.1, I expect crosscutting attitude change among Private Pre-K Parents, with average support for public provision of childcare declining over the same months that parents come to increasingly demand public subsidies.

By contrast, under the maximization alternative in the middle panel, self-interested preferences determined by (non)beneficiary status renders meaningless any distinction between public involvement in providing childcare as opposed to merely paying for it. If understood in partisan terms, the same is true for Alternative A, self-selection. However, an ideological reading of selection into preferred arrangements

Table 3.2  Competing Mechanisms of Attitude Formation: Empirical Expectations across Attitudinal Outcomes & Parent Samples

<table>
<thead>
<tr>
<th>Hypothesis</th>
<th>Preferences for Public Provision (I) &amp; Public Financing (II)</th>
<th>Findings in Natural Experiment vs. Quasi-experiment</th>
</tr>
</thead>
<tbody>
<tr>
<td>Alternative A: Ideological/Affective Self-Selection</td>
<td>Parallel if partisan; if ideological, social conservatives may prefer public subsidies for family-provided care (Romney’s FAP)</td>
<td>Different expectations (null results in natural experiment)</td>
</tr>
<tr>
<td>Alternative B: Beneficiary Maximization</td>
<td>Perfectly parallel</td>
<td>Some expectation for Universal Pre-K Parents; *different expectations for Private Pre-K Parents (*conditional upon lottery participation in theory, but not observed in timeframe studied)</td>
</tr>
<tr>
<td>Policy-adaptive Political Preferences (Proposed Hypothesis)</td>
<td>Distinct attitudes</td>
<td>Same expectations</td>
</tr>
</tbody>
</table>
leaves open some chance that social conservatives opposed to government childcare might endorse direct subsidies to offset family-based arrangements.

Difference-in-differences designs garner most attention for methodological properties, mimicking an experiment with nonrandom panel data which meet requisites. As the next section reviews, the usual desired statistical properties are important to this study, especially the latter half of my empirical analysis with broadened focus on a nonrandom sample of program-eligible UPK Constituents (N =172).

Importantly, before moving to research design in the fourth section, it bears note that the difference-in-differences estimator is also theoretically appropriate for testing hypotheses about policy feedback, a phenomenon I have theorized over time at the citizen-level. Alternative hypothesized links between preschool access and parent attitudes tested in this study –self-selection (A), and maximization (B) – generate competing expectations about between-group differences whereby arrangements cause, or reflect in the first instance, variation in parent preferences.

Combined with the crucial timing differences in the three theories depicted in Figure 3.1 and elaborated upon in the next section, support for my claims will be found in highly significant interaction terms capturing differential change by preschool group, whereas evidence in line with maximization or self-selection will be evident in the coefficients capturing September baseline views I deem to be effectively pre-treatment insofar as personal and cumulative impactful policy experience will generate new beliefs.
IV. RESEARCH DESIGN & DATA

A mismatch between originally theorized processes of citizen-level change over time and available evidence has left precise dynamics by which policy makes politics underspecified in much recent quantitative policy feedback scholarship. This may be most true where scholars have exploited strong research design opportunities and high quality secondary data\(^49\) afforded by uneven implementation of the Affordable Care Act to recover surprisingly weak signs of expected positive effects (Hopkins and Parish 2019), more robust and immediately measurable negative feedbacks (Hobbs and Hopkins 2021a; Jacobs and Mettler 2018), or demographically-contingent and conditional feedbacks which require extra theory and narrowed empirical focus to detect among some beneficiary subgroups (Lerman and McCabe 2017; McCabe 2016a).

Discussed above in context of the maximization/retaliation interpretation of mostly negative modern feedbacks and heterogeneity in policy’s (Alternative B) and partisan or affective self-selection built into some contemporary theorizing of fundamentally diminished positive feedback prospects amongst polarized and distrustful Americans (Alternative A), empirical ambiguity seems to tempt recourse to behavioral explanations, self-interest and ideology; both traits are theorized as essential to political preferences by different camps within political science, but not traditionally the policy feedback one. A break from conventionally developmental thinking about feedback

\(^{49}\) Mettler and Jacobs are the important exception with a noteworthy original panel (“the US Public Policy Study,” see supplemental appendix for (2018) tracking a nationally representative panel of Americans and their opinions as they experience the ACA. The results cited here consider data from 2010, 2012, and 2014 – such that there is very little coverage of the post-implementation period for key provisions like expanded Medicaid and state marketplaces for self-insurance. In more recent findings extended through 2018, cited elsewhere in this study, Mettler and Jacobs find much greater positive feedback once Americans meaningfully had a chance to see the ACA’s benefits for American society, and experience the new reforms, personally or through family.
contributes at the research design phase to subsequent findings which depart so starkly from classic insights about an experience-driven, adaptive, political process – democracy as citizens live it, substantively speaking.

Answering calls for stronger research design in the policy feedback domain (Larsen 2018; Campbell 2012), and more conscientious theorizing of time (Campbell 2020a; Hacker 2002; Pierson 2011), I combine careful case selection with original panel data to isolate and generalize the attitudinal effects of divergent experience with life changing but supply-limited universal preschool benefits. Qualities of Universal Public Pre-K juxtaposed with typical American social benefits and provisions in the paradigmatic Affordable Care Act, as well as system-level features of New York State’s randomized program inform my focus on this new social benefit to develop tractable theory regarding citizen-level ways in which impactful policy encounters beget desires regarding government’s role. My original panel study leverages random allocation of about ¼ as many State-funded Universal Public Preschool (“UPK”) seats as age-eligible children in New York for empirical tests suited to policy feedback theorized to reshape citizen political beliefs and behavior through direct personal experience over time.

To recap case selection essentials covered thus far, free full-day public preschool is unmistakably government and invariably high-value.\(^5\) Importantly unlike social insurance or direct transfer benefits, preschools pre-organize for parents a community with shared interests, facilitate participation in collective decision making through PTA’s and similar, increase chances for outside mobilization by virtue of group association, and

\(^5\) The most recent pre-pandemic state average tuition for private preschool equates to more than ½ of a year in full-time work at NY’s minimum wage ("Child care costs in the United States" 2021).
when public, promote distinctly less power-imbalanced interaction – ideally partnership – between citizens and street-level bureaucrats wearing the hats of parent and early childhood educator (Soss 2000; Soss and Weaver 2017; Lipsky 2010; Lerman and Weaver 2014; Weaver et al. 2019). At the program-level, Section II notes partial funding, random allocation, and universal eligibility in populous New York State. Emphasized then, and in the following pages, universal access precludes the between-group comparisons methodologically necessary in this instance, like all “natural” experiments, where data is observational even when treatment is fully random. I did not arbitrarily assign Universal Public Pre-K seats; I discovered and independently measured the real world political process (Dunning 2012).

Most critical to research design—specifically, the timing of repeat parent surveys designed to independently measure partway-funded universal preschool’s political feedbacks — experiences with benefits of the character just described should have substantial and direct effects that profoundly change day-to-day life, not to mention lifetime chances, for two-generations of each family at once. My cumulative mechanism does not onset until school starts, but relatively rapid adaptation in beliefs about government’s childcare responsibilities follows direct experience of universal preschool within months.

More than establishing causal direction, my panel isolates direct lived experience as the theorized cause of emergent change in childcare attitudes. Parent surveys a few

51 I follow Dunning (2012, 41) in defining “natural experiment” with emphasis on external control over assignment. Through this lens, New York’s lottery-allocated Universal Pre-K Program is a member of a special subset, randomized natural experiments; “The random or as-if-random assignment that characterizes natural experiments occurs as a feature of social and political processes – not in connection with a manipulation and carried out by an experimental researcher.”
weeks into the schoolyear \( t_o = September '19 \), and again after winter break \( t_1 = January '20 \), are timed to adjudicate between my preferred mechanism and alternatives which, as detailed in the previous section, yield very different expectations about the source and pace of processes causing childcare preferences to correlate with preschool uptake among parents with the same claim to public preschool. Recall: attitudes measured a few weeks into school are pre-treatment on my account, political adaptation through direct experience. If ideological self-selection or maximization better explain patterned parent beliefs by preschool arrangement, the baseline survey in September 2019 is post-treatment, as Figure 3.1 depicted.

The Affordable Care Act has proven impressively capable in recent years of generating political support by doing good for the American people. A rough rule of thumb places a tipping point in mass opinion around 2018, considering Republican and Democratic congressional campaign strategy vis-à-vis Obamacare. The monumental achievement in American social policy warrants extensive research by students of American politics. Still, at no time prior to passage nor in the wake of still-incomplete implementation has there been basis in policy feedback theory to expect self-reinforcing political gains, immediately measurable upon the law’s launch (Hacker and Pierson 2018; Campbell 2020a).

In the earliest instance, first chances for personal experience with the individual marketplace and expanded Medicaid followed five years of elite partisan warfare in D.C. Realistically, Americans overwhelmingly retained employer-sponsored or long-established public coverage on implementation day in 2014, and afterward. Given the nature and scope of specific reforms, it is not surprising, through an adaptive feedback lens, that it has taken years for personal, familial, and community experiences with a
more equitable, tax-subsidized, private healthcare system to amount to noticeably increased (and measured) public endorsement; Americans have gradually come to live amongst, rely on, and accept the ACA’s provisions as appropriate and desirable government intervention (Jacobs et al. 2019; Jacobs and Mettler 2020). Yet, several studies failing to find much increased support hardly cover the post-2014 period in which Americans have had chances to experience the Affordable Care Act’s most meaningful provisions (Jacobs and Mettler 2018; McCabe 2016b; Lerman and McCabe 2017). Focused on verifying the mechanism of preference adaptation through direct personal policy experience in a case where benefits likely feed back into political beliefs with exceptional haste, my design also aims to illuminate whether, and to what degree, some recent ACA research precludes finding robust positives with time horizons too-short to adequately observe the cause of citizen-level feedbacks, let alone to properly measure downstream political effects.52

Too Few Seats, Allocated Arbitrarily: Natural & Quasi-experimental Tests of Adaptation versus Alternatives

Partial public investment sufficient for about ¼ of age-eligible preschoolers statewide leads to a stipulation with Universal Pre-K grants extended by New York’s State Education Department to the vast majority of eight hundred school districts: lotteries. My two-part difference-in-differences design fully leverages this rare 52

52 Campbell makes a more damning related point about indirectness of ensuing political effects once benefits of health reform are experienced, “a phenomenon inherent to health insurance, not just the ACA” (2020a, 574); presuming health security to be the policy goal and conduit for political feedback, she underscores imperfect financial coverage and healthcare access as critical slippage points which lessen “feedback generating capacity” as the timeframe between policy and affected politics drags out.
opportunity in American social policy without leaving generalizability unaddressed (Dunning 2012). I pair complimentary natural and quasi-experimental tests of my hypothesis, policy adaptive political preferences, against prominent alternatives privileging ideological predisposition (Alternative A) and material interest (Alternative B) to explain patterned parent beliefs about the optimal allocation of childcare responsibilities.

The most straightforward results presented first in Section VI lay empirical foundations for further analysis. Restricting focus to the subset of my panel for whom Universal Pre-K constitutes a randomized natural experiment, Applicants (N=79), I test for significant over time changes in childcare attitudes among Universal Pre-K Parents using Lottery Losers as the “untreated” comparison group with whom common time trends are addressed. To assess my independent measurement of allocation known to be random, conditional on an actual attempt to claim a due seat for 2019-2020, I begin with a check for balance on pretreatment covariates as if I had controlled random assignment. Detailed in the next subsection, my difference-in-differences approach to estimating lottery-assigned preschool’s effects upon over-time attitude change goes considerably further to remove potential confounding in analysis where quasi-experimental techniques are not necessary, methodologically speaking,

Most important, difference-in-differences estimation of the same quantities with both samples, average one-semester belief changes by preschool arrangement, renders results from a setting with known random assignment unusually strong grounds for asserting “as if random” assignment in the second broader analysis – without invoking conditionality on additional measured covariates, as it turns out. Drawing support for inference from the narrower analysis despite untestable identification assumptions, quasi-experimental hypothesis tests with my Constituents (N=172) sample in return
assess the generalizability of insights gleaned from a lottery playing subset in a manner with potential to considerably augment the apparent relevance of causal estimates. To the extent quasi-experimental replication of first findings proves successful, my latter nonrandom analysis will also provide empirical basis for assessing concerns that the peculiarity of lottery-partaking parents accounts for large and rapidly accumulating adaptation in attitudes observed among the restricted randomly assigned subset.

Beyond lending context, the second quasi-experimental hypothesis tests build upon the first. My nonrandom Constituents (N=172) sample affords sufficient power to test the model most closely matching my expectations of divergent preschool experience and crosscutting feedback. Introduced in the following pages as the “Dual Treatment” difference-in-differences (DD.2) model, the extended specification (Equation 4.3) estimates Universal Pre-K’s effects simultaneous with adaptation in attitudes attributable to a semester spent spending for private preschool. Absent access to publicly provided options, private childcare strategies fundamentally divide families along socioeconomic status. Wealthier, whiter, more educated, and almost invariably married parents purchase private preschool; other families do not, and for the most part, cannot. This split is descriptively apparent but with too much uncertainty to formally test among the small set of Lottery Losers (n=16) against whom Universal Pre-K Parents are compared in the first hypothesis tests.

The natural- and quasi-experimental components of my research design are also complimentary in a theoretical sense distinct from the considerations of inference in studying this real-world political experiment which render my two-part design methodologically sounder than the sum of each part. As a matter of theory, evidence of cross-cutting adaptive attitudinal feedback observed among randomly assigned
Applicants (N=79) should be evident in subsequent analysis with Constituents (N=172). If, as I argue, divergent experience with a common Universal Pre-K entitlement drives patterned adaptation over months in beliefs about childcare responsibility along separate practical and financial dimensions, my mechanism implies similarity across samples, no matter that only some parents self-selected into lotteries in the months preceding schoolyear 2019-2020 and that even fewer won.

Considering Private Pre-K Parents (n=49) in this light, shared experience will be far more politically consequential by mid-schoolyear than any personal traits systematically associated with lottery participation prior to last minute purchase of a private seat. Because direct policy experience drives adaptation in attitudes and lottery losing is not theorized to do the work, my mechanism similarly lends theoretical basis to an expectation that No Pre-K Parents (n=60) comprised mostly of potential and not actual applicants provide appropriate “untreated” comparison parents for difference-in-differences estimation of UPK’s effects on attitude change in quasi-experimental tests among nonrandom Constituents (N=172), the methodologically-necessary expectation considered next.

**Difference-in-Differences Estimation**

Quite separate from suitability to testing between group differences in attitude change over one-semester as considered in closing the theory section (III), the difference-in-differences design employed in Section VI is known for desirable statistical
properties when longitudinal data satisfy requisites. This estimation strategy controls for systematic parent-level predictors of attitudes as well as common over-time variation in childcare preferences, in both natural and quasi-experimental tests of Universal Pre-K’s effects.

More generally, by isolating differential within unit change attributable to a treatment through over time comparison to untreated “control” units, the difference-in-differences estimator exploits repeat measurement to mimic experimental inference with observational data (Aronow and Miller 2019; Card and Krueger 1993), and sometimes to improve efficiency when randomization is present (Gerber and Green 2012, 97-98).

Considering parent preferences for public childcare responsibility along either dimension ($\text{Provide}_u = 1; \text{Pay}_u = 1$) in the classic two-period case we have here, the difference-in-differences estimate of Universal Pre-K’s average treatment effect adjusts observed change in public childcare preferences among “treated” Universal Pre-K Parents between September of 2019 and January 2020 (the “first-difference” estimate) by subtracting the average change in preferences observed over the same semester for untreated-but-arguably-comparable control parents.

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53 Requisites surprisingly seldom met with available large-N political surveys in frequent use by Americanists, as considered under the Data subsection.

54 The first difference is simply the time-series estimate of within subject over time change. It is equivalent to OLS Regression with “one-way” fixed effects, adjusting for unit-level confounding but not common time trends.
The foreshadowed common trends assumption requires that if Universal Pre-K Parents in my panel had instead not received free preschool seats, their counterfactual one-semester attitude change would be the same, on average, as the one-semester change observed for control parents. Substantively, the control group denoted in the left bracket of Equation 4.3 varies between the two sets of hypothesis tests in Section VI. In first tests with my natural experimental sample, Lottery Losers (n=16) provide a small but highly credible comparison group for eliminating time-trend confounding from estimates of the policy’s one semester effects upon winners, Universal Pre-K Parents, that is. The latter quasi-experimental analysis with my broader sample of Constituents (N=172) uses No Pre-K Parents (n=60) as the “untreated” comparison group.55

In the foundational analysis of lottery participants, random assignment more than meets the common trends assumption. Without additional pre-treatment measurements of childcare attitudes for my panel, the common trends assumption cannot be tested quantitatively in my study. Nonetheless, by grounding the second quasi-experimental set of tests in replication of first results, before extending them, the

$$\text{difference} - \text{in} - \text{differences} =$$

$$\frac{(Pref. \ Public_{UPK = 1}^{Jan.'20} - Pref. \ Public_{UPK = 0}^{Sept.'19})}{\text{Universal Pre-K Parents Average 1-Semester Change}} - \frac{(Pref. \ Public_{UPK = 0}^{Jan.'20} - Pref. \ Public_{UPK = 0}^{Sept.'19})}{\text{Control Parents Group Average 1-Semester Change}}$$

55 Recall, Constituents (N=172), of whom No Pre-K Parents (n=60) are a subset, are verified potential applicants. Parents who needs are not fulfilled by the program, and those expressing or not ruling out an aversion to the program as their reason for nonapplication are excluded from all analyses here.
narrower randomized component of my design provides unusual confidence for asserting assumptions about counterfactual attitude changes among *Universal Pre-K Parents*.

In practical terms, I estimate “two-way fixed effects” OLS regression models to test the trio of hypothesized links between parent attitudes and preschool arrangements. Although additional covariates are allowable with slight adjustment to identification assumptions and amply available because I collected them, my preferred regression specifications introduced below and presented in Section VI estimate the one-semester effects of *Universal Pre-K* upon attitude change without covariate adjustment. All models test for differential adaptation in parent preferences by preschool arrangement over the first semester of schoolyear 2019-2020. The two survey periods used for estimation correspond to a beginning of schoolyear baseline, $t_0 = September 2019$ (pre), and a follow-up a few weeks into second semester, $t_1 = January 2020$ (post).

For parent $i$ in period $t$, $Y_{it} = \{-1, 0, 1\}$ denotes one-semester change in a public-over-private childcare preference, taking one of three values for each attitudinal outcome — *less public* (i.e., more private), *stable*, or *more public*. For example, if parent $i$ starts the schoolyear preferring government provided care for children under school age ($Provide_{it_0} = 1$) but comes to embrace private alternatives after several months ($Provide_{it_1} = 0$), her one-semester change in preference for *public childcare*

\[ Y_{it} = \begin{cases} -1 & \text{for decrease in preference for public childcare} \\ 0 & \text{for no change in preference for public childcare} \\ 1 & \text{for increase in preference for public childcare} \end{cases} \]

\[ Y_{it} = \begin{cases} -1 & \text{for decrease in preference for public childcare} \\ 0 & \text{for no change in preference for public childcare} \\ 1 & \text{for increase in preference for public childcare} \end{cases} \]

\[ Y_{it} = \begin{cases} -1 & \text{for decrease in preference for public childcare} \\ 0 & \text{for no change in preference for public childcare} \\ 1 & \text{for increase in preference for public childcare} \end{cases} \]

\[ Y_{it} = \begin{cases} -1 & \text{for decrease in preference for public childcare} \\ 0 & \text{for no change in preference for public childcare} \\ 1 & \text{for increase in preference for public childcare} \end{cases} \]

\[ Y_{it} = \begin{cases} -1 & \text{for decrease in preference for public childcare} \\ 0 & \text{for no change in preference for public childcare} \\ 1 & \text{for increase in preference for public childcare} \end{cases} \]

\[ Y_{it} = \begin{cases} -1 & \text{for decrease in preference for public childcare} \\ 0 & \text{for no change in preference for public childcare} \\ 1 & \text{for increase in preference for public childcare} \end{cases} \]

\[ Y_{it} = \begin{cases} -1 & \text{for decrease in preference for public childcare} \\ 0 & \text{for no change in preference for public childcare} \\ 1 & \text{for increase in preference for public childcare} \end{cases} \]

For consistency with the results in Section VI, I exclude a permissible vector of individual-level controls, $X_{it}$, from presentation of my regression models. As noted in the results, I calculate and report heteroskedasticity robust standard errors clustered at the parent-level to account for the panel structure of my data.
provision would be negative, $\Delta \text{Provide}^i_t = Y_{it} = -1$, in the regression models below.

Each specification includes two fixed effects: $\alpha_i$ absorbing stable individual-level confounding for each parent $i$, and a time fixed effect, $\lambda_t$ accounting for any common over time trends in parents’ beliefs about childcare responsibility between the September 2019 baseline and post-treatment period $t$ (January ’20), a first follow-up fielded once families were back into the preschool routine after winter break, and still not for No Pre-K Parents.

The difference-in-differences is modeled with an interaction between binary indicators for lottery-assigned Universal Pre-K treatment ($Universal\ PreK_{it} = 0; 1$) and the post-treatment period ($t_1 = 1$), January of 2020. Maintaining the same notation, this reduces to $Universal\ PreK_{it}$ in each regression model (Equations 4.2 & 4.3). The quantity of interest, $\beta_1$, estimates the average within-parent attitude change attributable to one semester with Universal Pre-K. Equation 4.2 presents the core “Single Treatment” difference-in-differences (DD) model used to test hypotheses about childcare provision with my randomized sample, Applicants ($N = 79$).

**Equation 4.2  “Single Treatment” Difference-in-Differences (DD) OLS Regression Model**

$$Y_{it} = \alpha_i + \lambda_t + \beta_1 \text{UniversalPreK}_{it} + \epsilon_{it}$$

Testing for change with my randomized sample in the second attitude, preference for public childcare financing, $\Delta Pay_t = Y_{it} = \{-1, 0, 1\}$, and in all tests with the better-powered quasi-experimental Constituents ($N=172$) sample, I extend the base
model with one more interaction term for Private Pre-K “treatment,” coded just like the universal public one.\textsuperscript{57} The second quantity of interest in Equation 4.3, $\beta_2$, estimates average within-parent attitude change attributable to one semester with Private Pre-K along with $\beta_1$, the difference-in-differences estimate of Universal Pre-K upon preference adaptation. Baseline September views among \textit{No Pre-K Parents} ($n=60$) serve as the reference for simultaneous estimation of public and private preschool-driven adaptation in childcare preferences when this extended specification is used.

\textbf{Equation 4.3} \hspace{1cm} \textit{“Dual Treatment” Difference-in-Differences (DD.2) OLS Regression Model}

\[ Y_{it} = \alpha_i + \lambda_t + \beta_1 UniversalPreK_{it} + \beta_2 PrivatePreK_{it} + \epsilon_{it} \]

The “Dual Treatment” Difference-in-Differences (DD.2) model reflects that private programs also change parent views about public responsibility, best capturing my predictions about divergent preschool experience as the driver for preference adaptation. Had more \textit{Lottery Losers} been recruited, Equation 4.3 would be the core model and used to estimate preferences for public provision among \textit{Applicants} ($N=79$) too.

With my randomized lottery-based sample, predicted convergence over time in beliefs about financial responsibility among all parents with formal preschool -- \textit{Universal Pre-K Parents} and \textit{Private Pre-K Parents} in the same direction -- requires Equation 4.3’s extra term to differentiate among parents without a public seat. As

\textsuperscript{57} That is, using interacted binary indicators for arrangement ($PrivatePreK_{it} = 0; 1$) and post-treatment period ($t_1 = 1$).
discussed with results in Section VI, use of the “Dual Treatment” (DD.2) model to test change in beliefs about financial responsibility does not change the matter of too few *Lottery Losers* to reasonably split based on purchased versus family-provided policy-forced private alternatives to Universal Pre-K. That’s where the second set of quasi-experimental tests disproportionately contribute.

Moving to the latter quasi-experimental analysis, I begin with a demonstration focused on regression estimates for one age-specific attitude – the preference for publicly-provided care, considering 3-year-olds \( \text{Provide}_{3y}^{3y} = 1 \) – to transparently gauge the comparability of insights between samples and model specifications. I replicate natural experimental tests with the “Single Treatment” Diff-in-diff (DD) specification, then extend analysis by estimating the same change using the “Dual Treatment” model. This exercise contextualizes estimates obtained with random and nonrandom samples for group-average attitudes at baseline and one-semester change. It also transparently establishes the “Dual Treatment” Diff-in-diff (DD.2) specification best aligned with my theory of preschool-adaptive childcare preferences as empirically appropriate with the better powered sample.

I proceed with the “Dual Treatment” (DD.2) model in remaining quasi-experimental tests for changes in opinion about government provided infant and toddler-care \( \text{Provide}_{<3}^{<3} = 1 \). As before, my expectation of attitude change in the same direction on financial responsibility for childcare makes the additional treatment term more important to correctly modeling my predictions about convergent preference adaptation over months of mutual experience with the most expensive and school-like of early childhood arrangements.
Data

Citizen-level outcomes conventionally of interest to political scientists are exceedingly rare in high quality “off the shelf” panel surveys covering the United States. Quantitative feedback scholarship on the Affordable Care Act cited throughout this study has made remarkable progress in the few years since a quantitative metanalysis of feedback research turned up overwhelmingly cross-sectional designs; “this is interesting” Larsen put it, “as policy feedback effects often presume a process” (2018). Repeatedly measuring Affordable Care Act opinions and experiences among a very large sample of Americans since the legislation’s pre-passage period, the Kaiser Family Foundation (KFF)’s “Health Tracking Poll” now spanning more than has been invaluable to this advance.58

Notwithstanding severely limited secondary options for repeat political measures, original collection of the panel data necessary to test theoretical expectations of an adaptive process which reshapes politics over time has been exceedingly rare in citizen-level policy feedback research to date. Jacobs and Mettler deserve note as the exception, with an exemplary original panel tracking the Affordable Care Act’s attitudinal feedback effects since 2010. In scholarship extending empirical focus beyond their fall 2014 wave, control over data collection pays off with leverage into the dynamics which gave eventually yielded within-citizen improvements in ACA opinion among the Jacobs-Mettler panelists (Jacobs and Mettler 2020; Jacobs et al. 2019).

58 Aside from KFF’s HTC, the Fragile Families and Child Wellbeing Longitudinal data, and the Bureau of Labor Statistics’ National Longitudinal Studies have been utilized for quantitative feedback studies.
Repeat measurement of “Affordable Care Act” approval and actual experiences with impactful provisions of President Obama’s signature program alongside potentially mediating quantities, like the degree to which citizens perceive improved personal, familial, or community-level healthcare access and source tangible improvements back to the ACA, grants considerably more insight into the mechanisms likely responsible for eventual positive feedbacks now observable: direct policy experience, or so I argue. As mentioned in opening Section IV, this was not evident in joint scholarship utilizing their rich original panel data until it became so.\(^{59}\)

Substantive importance of documenting the Affordable Care Act’s citizen-level political effects justifies nationally representative insights with an oversample of low-income nonwhite Americans most likely to see improved access and affordability – the sample population for the Jacobs-Mettler panel. Yet, without an “untreated” comparison group against whom to gauge changes among ACA-affected citizens, inference with the Jacobs-Mettler panel data remains vulnerable to unobserved over time confounding; put somewhat differently, difference-in-differences estimation is methodologically out of reach.

In this sense, my panel study is inspired by the advantages of original data collection clearly demonstrated in scholarship drawing upon Jacobs and Mettler’s rich custom designed panel survey, as well as design-based inference closer in spirit to ACA feedback studies which leverage quasi-random aspects of policy rollout, and

\(^{59}\) Jacobs and Mettler’s preliminary theorizing about partisanship and cynicism as fundamental sources of contemporary interference in American politics that block policy benefits from inspiring political support (2018) remains influential despite the scholars’ updated thinking with survey waves subsequent to the fall 2014 wave responsible for (too) early conclusions that have proven sticky.
overwhelmingly use the Kaiser Family Foundation’s panel (Hopkins and Parish 2019), often combined with additional secondary sources (Hobbs and Hopkins 2021a; Lerman et al. 2017; McCabe 2016a).


Very large actual and eligible populations of Universal Pre-K claimants in New York State rendered microtargeted social media advertisements an effective and cost-conscious means for directly recruiting members of a particular policy-eligible population into my paid online panel study, “The New York Parenting Project.” Whereas New York State funded 112,653 full-day UPK seats in the fall of 2019, the Facebook Ad platform projected approximately 100,000 users in my campaign “audience” tailored to Universal Pre-K-eligibility.

In September of 2019 I delivered ads for a paid parenting study to an audience deemed by Facebook to be both “parents with preschoolers” and New York State residents. The ads included clickable links as well as the domain name for my online study’s enrollment page and baseline, a Qualtrics survey redirected to www.NewYorkParenting.org, literally speaking. To affordably leverage the social media conglomerate’s microtargeting power, I embedded URL trackers (Facebook “pixels”)

60 Pre-K enrollment numbers come from the New York State Education Department’s data website, data.nysed.gov (New York State Education Department 2021).

61 I purchased this URL through Google Domains for $10/year, on the advice on Brookman, Kalla, and Linzer’s extremely helpful “repeated online panel experiments” GitHub https://github.com/dbroockman/repeated-online-panel-experiments. Brookman et al.’s GitHub repository proved invaluable to the technical side of this study. The repository includes vendor suggestions, project notes, sample code, etc. for maintaining an online panel once recruited.
within the JavaScript of my Qualtrics survey. Although it required additional registration of business accounts and a Facebook page on behalf of the study, pixels enabled me to pay Facebook for a few hundred completed baselines, as opposed to 175,000 “impressions” (ad showings) or nearly three thousand landing page views.\(^{62}\) All in, my recruitment costs came to about $5 per parent for survey completion. Applying a strict standard closer to what researchers want than what can be purchased from online panel vendors, per head recruitment costs for parents who completed at least two waves and make it into analysis as (at least) *constituents* (N=172) of the Universal Pre-K program, potential applicants as opposed to merely eligible, recruitment costs come in around $11.50.\(^{63}\)

Also important, the study’s ads, Facebook presence, survey homepage, and prescreens were sufficiently vague to preclude potential respondents from deducing eligibility criteria (child born in 2015 or 2016). The first prescreen filtered out parents with children a bit too young or old; question sequencing and an “appreciate your interest” message were crafted to avoid deception, keep eligibility criteria opaque, and mitigate backlash. As detailed in appendix IV.B, a second prescreen gathered application history necessary to designate UPK-eligible parents as *Applicants* (N=79) and *constituents* (N=172). Technical details aside, deep case knowledge was most essential to directly recruiting my online panel and deploying successive prescreens to capture and

\(^{62}\) These among other “a la carte options” for ad pricing allow fast set-up of advertising campaigns and would not require additional registration with Facebook, or coding to embed the Pixel within Qualtrics, but would entail no guarantee or control over whether ad clickers passed the prescreens confirming eligibility, let alone that they complete the first survey. This is especially important given the way in which Facebook uses initial ads data to perfect serving future ads, presuming there are “types” more likely to click ads without sincere interest. More generally,

\(^{63}\) Notably, retention beyond the second wave has been extremely high. A pandemic and almost two years later, roughly 130 original parents continue to complete surveys.
validate application status without revealing Universal Pre-K-eligibility as the primary exclusion criteria for participation in a series of paid surveys, originally covering the 2019-2020 schoolyear, since interrupted and extended by COVID-19.64

Within the first few weeks of September 2019, hundreds of eligible parents followed my ads to complete New York Parenting I, a 15-minute baseline coinciding with roughly the second week of preschool.65 Of these parents, 199 finished the first follow-up in January 2020, when public schools resumed after winter break, and passed a data quality screening thereafter.66 Following each wave, parents were compensated with an e-gift card within 72 hours of completion ($5-7 per parent per wave).67 Although members of my panel, ten parents with Head Start-enrolled preschoolers and another seven with individualized learning plans (IEP) are excluded from my nonrandom sample. In the first instance, my subgroup is too small to include means-tested public preschool recipients of considerable theoretical interest,68 and in the latter, children’s

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64 See Appendix IV B for more information about the first and second survey prescreens.

65 Public School Districts control their own schedules but generally start after Labor Day in September. Private Schools have at least as much start date leeway. My baseline was in the field from 9/8 to 9/24.

66 As explained to parents during informed consent and in a reminder embedded within each survey, parents responses were screened for quality after the second wave. Eleven parents were dropped at this time. Pool quality responses were identified by unusually fast response times and multiple substantive inconsistencies in survey responses suggesting less than careful attention to the survey questions. See Appendix IV B for more detail.

67 Brookman, Kalla, and Linzer’s GitHub repository with vendors, notes, sample code, etc. for repeated online panel experiments proved invaluable. https://github.com/dbroockman/repeated-online-panel-experiments

68 Relatively few Head Start preschoolers in New York State is a direct implication of the state’s very large even though partial universal preschool program. Federal funding is pooled with state and local resources into a single early education program more so by NY than most states in which federally funded Head Start operates simultaneously with typically targeted state/local public preschool initiatives within the same communities. Some but not all states redirect some or
special needs render Universal Pre-K an early education mismatch. As discussed in Section II, I drop another ten parents for expressed or implied aversion likely to preclude uptake of a Universal Pre-K offer.

Beyond lottery-assignment for the restricted Applicants (N=79) subset, demographic similarities between preschool groups within and across my quasi- and natural experimental samples goes some distance to quell concerns about self-selection and generalizability. Even numbers of Republican identifiers across preschool arrangements in my study suggests that universal preschool is not subject to polarized uptake of the kind observed in early days of the ACA exchanges (Lerman et al. 2017).69

Figure 4.4  No sign of Ideological Sorting into Preschool

all of their allowable portion of federal “welfare” funding to Head Start rather than direct assistance through TANF.

69Notably, substantial numbers of self-identified conservatives abstained from identifying with the GOP, opting for “No preference” (n=38) and “Other” (n=10). Based on ideology, parents selecting “Other” or “No preference” place themselves slightly to the left of self-identified Republicans, on average, but considerably further right than Independents, and much further than Democrats
Figure 4.4 illustrates strikingly proportionate distributions of political ideology between preschool arrangements at baseline. On a scale of 1-7, each preschool-based subgroup averages slightly left of center, between 3 (light blue) and 4 (purple). Consistent with a notion in American politics that political interest increases with income, like participation, the relatively well-off Private Pre-K Parents (n=49) are visibly more polarized, with exceptionally few centrists and a larger share of conservatives despite the most-liberal group-average ideology in September.

The share of true centrists (purple) among No Pre-K Parents is notably large and by similar logic, could suggest that this group is especially disengaged or disaffected. Political confidence measures considered in Section V’s discussion of “interpretive” effects mitigate this concern. More formally, my difference-in-differences estimation approach adjusts for stable parent level confounding such as fewer (more) political extremists in one or another arrangement.

Ideological balance likewise holds when Universal Pre-K Parents (n=63) are instead considered within context of random and nonrandom subsamples analyzed in separate halves of my results (Section VI). The green bracket visually denotes the former,
Figure 4.5  Racial Self-Identification (Parents), by Pre-K Group

![Bar chart showing racial self-identification by Pre-K group]

Figure 4.6  Family Arrangements, by Pre-K Group

![Bar chart showing family arrangements by Pre-K group]

Notes: all parents identify as mothers with these exceptions:

* Five asterisks denote five fathers among UPK Constituents (N=172): two married No Pre-K Parents, and three married Private Pre-K Parents.

1 One of eighteen coupled respondents without preschool is an aunt.

2 One of seventeen coupled respondents with Universal Public Pre-K is a grandmother.
while a purple bracket distinguishes the three subgroups analyzed in my broader quasi-experimental analysis of potential Universal Pre-K applicants.

Ideological balance likewise holds when *Universal Pre-K Parents* \((n=63)\) are instead considered within context of random and nonrandom subsamples analyzed in separate halves of my results (Section VI). The green bracket visually denotes the former, while a purple bracket distinguishes the three subgroups analyzed in my broader quasi-experimental analysis of potential Universal Pre-K applicants.

Preschool arrangement-based comparisons by race, family-type, and household income\(^{70}\) show slight patterns, but little reason for alarm and none for surprise. Figure 4.5 reveals *Universal Pre-K Parents* are most racially diverse; *Private Pre-K Parents* \((n=49)\) are whitest.\(^{71}\) As shown with count data in Figure 4.6, *No Pre-K Parents* \((n=60)\) and *Universal Pre-K Parents* \((n=63)\) have almost the same shares of married, coupled, and single parents at baseline. *Private Pre-K Parents* are again outliers in a high-income correlated manner, near-universal marriage.

Juxtaposed with the income distribution for winning *Universal Pre-K Parents* \((n=63)\) in the middle panel, Figure 4.7 visualizes household income among *Lottery Losers* \((n=16)\) split into purchased (top) and no preschool (bottom) back-ups. If combined into a single distribution, earnings reported by *Private Pre-K Parents* in blue

\(^{70}\) Household income is self-reported before taxes.

\(^{71}\) Census ACS data from 2019 estimates New York State’s population as less than 70% white, suggesting that nonwhite parents are somewhat under-sampled. Using 70% as a benchmark, I recruited about half as many nonwhite parents as expected given population rates. Twenty-five in my Constituents sample identify as nonwhite when population statistics suggest closer to fifty would be representative in a sample of 172.
Notes: Two very high earning families reporting more than 1 million and $400k, respectively, are omitted from the Private Pre-K distribution in Figure 4.8 to maintain the same scale for direct comparison between natural and quasi-experiment samples.
and No Pre-K Parents in red would together look substantially similar to that observed among luckier peers in green.

Figure 4.8 repeats the middle distribution depicting household income for Universal Pre-K Parents \((n=63)\), this time sandwiched by larger quasi-experimental versions of the same arrangement-based comparison groups, Private Pre-K Parents \((n=49)\) and No Pre-K Parents \((n=60)\). Two outliers with very high income among private preschool purchasers in blue, one family reporting $400k and another more than $1 million in pre-tax household income for 2019. Notwithstanding this caveat, comparison of the two figures suggests that Applicants \((N=79)\) are not unusual in income, relative to the larger population of families eligible for the free public program.\(^2\) Despite far less density in the upper set of private arrangements, the distributional shapes and tails for purchased and non-preschool private arrangements look relatively similar in random and quasi-random parent samples.

Finally, comparing household income between groups within Figure 4.8, No Pre-K Parents \((n=60)\) look quite similar on average and in aggregate to Universal Pre-K Parents \((n=63)\). Combined with the similarities in political ideology and family composition between parents treated with free public preschool and those who go without formal early education, very similar green and red household income distributions in Figure 4.8 constitute a compelling descriptive case that the two sets of families are comparable before about half get a UPK seat.

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\(^2\) Appendix Figures 4.3. (A) and (B) visualize the same data differently, emphasizing similarity in the proportion of top and bottom earners across samples.
In addition to the previously mentioned underrepresentation of racial minorities in my panel, cost-conscious recruitment with Facebook ads skewed my sample with respect to gender. Although open to fathers, parents, and guardians broadly defined, 93% of my panel identify as mothers. This is not a substantive problem for the present study, but a data consideration worth mentioning in context of survey recruitment via social media. The Facebook ad platform can be further tailored to ensure demographic balance. I expect it would have been much more expensive to recruit fathers in comparable numbers, however acute path dependence in Facebook’s proprietary algorithms makes it difficult to speculate.

Appendices IV A, B, and C provide further details on recruitment of my panel with microtargeted social media advertisements (A), prescreens, application history questions, and qualitative information used to define natural and quasi-experimental subsamples (B), and additional descriptive statistics tables and figures demonstrating comparability on theoretically important demographics (C) between various samples and preschool groups used for hypothesis testing in Section VI.

The next section presents stylized facts about typical parenting experiences and beliefs about childcare responsibilities according to preschool arrangement. For preschool-based subgroups large enough to stand alone, I primarily focus on descriptive comparisons of Universal Pre-K Parents \((n=63)\) to nonrandom Private Pre-K Parents \((n=49)\) and No Pre-K Parents \((n=60)\). After considering resource effects which likely

\[\text{\textsuperscript{73}}\]

For married and coupled respondents, my survey includes parallel series about partners’ time use, employment, and basic demographics. Still, aggregates comparing gendered weekly time use in Section V drop five fathers so that respondent values reflect women’s paid work and parent care experiences, conditional on employment and partnership statuses.
move with or contribute to adaptation in parents’ childcare preferences, descriptive analysis in this section concludes with introduction to the two primary attitudinal outcomes themselves.
V. STYLIZED FACTS: PARENTING EXPERIENCES & CHILDCARE ATTITUDES

Parenting Experiences

An American policy landscape skewed to promote marriage (Jacobson forthcoming) instead produces politically consequential tendencies toward codependent couples and working-poor single mothers. Absent public support in decades since women entered work en masse, American families individually shoulder costs and risks of raising children in modern society, where most parents work. Peer nations foster this same social and macroeconomic reality with universal preschool and public childcare programs, among other family supports long standard policy elsewhere in the rich democratic world.74

Under status quo conditions—no public support before kindergarten—class-stratified childcare strategies entrench relative advantage for particular types of families, undermining collective identity among parents while reinforcing nonexpectations of public responsibility. Conversely, life with universal preschool generates mutual recognition among parents of political interest in government playing a more substantial childcare role – so I have argued.

74 The comparison to rich democracies is apt on the basis of similar political economies to the United States. This is not to say that developing democratic and non-democratic regimes lack supportive family policies. Modern nations of all variety overwhelmingly provide benefits that American families lack, paid maternity leave and preschool.
**Resource Effects**

My basic distinction among families by preschool arrangement requires a caveat or two. Treating families as units, and parents without regard for gender, masks fundamental day-to-day similarities in modern American motherhood. Parallels in time use (Figure 5.1) hold across preschool groups within New York State. First and foremost, extreme hours spent on childcare emerges as a common denominator for mothers, bridging meaningful employment and relationship differences.

In September 2019, mothers of all circumstances in my study reported an average of 35-50 hours per week on childcare – 5 to 7 hours each day, including weekends. Whether partnered or single and regardless of pre-k arrangement, working moms reported weekly childcare hours in the range of 35-45 hours. Smaller subsets of nonworking mothers within each preschool group – about 20% of those with Private Pre-K and 30-40% of mothers with a universal public seat or no preschool at all75 – tended to max out the scale, most selecting “more than 50 hours per week” no matter their relationship status.

With this similarity in mind, Figure 5.1 excludes comparable panels for employed and nonworking single mothers to facilitate focus on core descriptive facts of parental time use.76 Mothers’ time in paid work (blues) and childcare hours (greens) are first within each panel, followed by partners’ paid work and weekly childcare hours. In each

75 Considering both samples, 60-65% of No Pre-K and 65-70% of Universal Pre-K mothers were employed in September 2019 and January 2020; for Private Pre-K mothers the employment rate is 79-81%. Partners generally have employment rates in the 90s and almost always work full time. See Appendix V, Figure A.5.2.

76 Appendix Figure A.5.2 repeats the figure with panels for single mothers in each employment status and survey period.
color, light, medium, and deep shades show weekly average hours spent by No Pre-K Parents, Universal Pre-K Parents, and Private Pre-K Parents respectively.

Inverse to the effect of paid work on mothers’ own caretaking time, partners spent considerably more hours on childcare each week in families where mothers also had paid jobs at the time of my baseline survey. Mothers living with spouses or partners uniformly reported that co-parents devote substantially less time to childcare, averaging 13.5–25 hours each week (<2 to 3.5 hours per day). Still, the employed among married and coupled mothers report considerably greater weekly childcare contributions from their partners.

Considering paid work patterns, gender once again dominates preschool access. Compounding differences in the likelihood of working (Appendix Figure A.5.1), mothers with preschoolers and jobs worked, on average, substantially less than full-time. Male partners, on the other hand, almost always average a more than full-time work schedule. At least descriptively, more than full-time work appears most pronounced in families where mothers do not also have jobs.

Conditional on working, partnered mothers averaged 31–34 hours of paid work per week in September while single mothers (shown in the appendix) worked almost exactly the same number. This reflects high rates of part-time work and self-employment at less than forty hours among mothers in each preschool group, and more fundamentally, working mothers’ 5–7 daily hours of childcare.

Measured again in January 2020 (the right-side panels of Figure 5.1), typical weekly time spent on parent care and paid work remained the same across and within families with one exception. No Pre-K mothers consistently provide more childcare in
January, ranging from 5-10 hours more based on employment and relationship status, when other groups of mothers hold steady or provide less over time.\textsuperscript{77}

Compared to overwhelmingly male partners,\textsuperscript{78} fewer average hours on paid work if working, and much more time on childcare in every case, could reflect mass deference to gendered parenting roles among mothers in my study. But reasonable response to systemic motherhood penalties in pay and promotion within unprotected American labor markets, plus unaffordable and/or unavailable private care for purchase, offers an observationally equivalent alternative interpretation. That single and coupled mothers report almost the same weekly hours on childcare and paid work suggest systemic gendered forces outside of households drives similarity in maternal time use (for single mothers’ time use see Figure A.5.2 in appendix).

Relatively-larger childcare contributions from the husbands and partners of working mothers, versus non-working ones, can optimistically be read through a lens of enhanced female bargaining power. But time use evidence also shows signs that all parents contribute as much as they can. Marital outside options attributed with liberalizing working women’s social policy attitudes as they acquire economic

\begin{flushleft}
\textsuperscript{77} Considering only the random sample of Applicants, losing the lottery is associated with about an 8 hour increase in weekly childcare personally provided, while the difference is closer to 5 hours for the broader sample. Due to missingness in the time use data reducing my effective sample sizes, I do not present regression results for this finding. With the caveat of missing data, a decline of about 5 hours per week in mothers’ childcare approaches statistical significance for Universal Pre-K Mothers too (p<.10). Figure 5.1 emphasizes the descriptive increase in childcare hours for mothers without formal preschool.

\textsuperscript{78} Considering 152 mothers with partners as of the September 2019 baseline survey, only two have wives while 136 live with either a husband or boyfriend. Gender is ambiguous in 14 families where a mother indicates living with a spouse or partner. Gendered time use among other factors like profession suggest that most of these partners are male too. This establishes lower and upper-bounds of 89%-99% male coparents.
\end{flushleft}
Notes: bar graphs visualize parents’ weekly hours spent on paid employment and childcare in September of 2019 (L) and January 2020 (R) by mothers’ employment and partnership statuses in September. Thus, nonworking mothers in the bottom row have zero average hours in paid work. Five respondents who are fathers are filtered out.

In each panel, light, medium, and deep shades show weekly average hours spent by No Pre-K, UPK, and Private Pre-K parents, respectively. Mothers’ paid work and childcare hours are first, followed by partners’ paid work and childcare hours within each panel. Baseline September views are in the left-side while January attitudes for the same parents are on the right. Figure 5.1 also distinguishes families based on mothers’ employment status in September. Top panels show time use among employed mothers (full-time; part-time; self-employed), while bottom panels show trends for families in which mothers were not working in September (stay at home parent; disabled; student; not employed & not looking; or, unemployed & looking).
independence (Iversen and Rosenbluth 2010) are severely constrained by family living costs exceeding one full-time wage and an American political economy marked by pervasive risk. Altogether parental time use patterns suggest that couples are economically codependent in this expensive lifecycle moment distinguished from later-parenting by limited-to-no public support. Even with Universal Pre-K, and especially with Private Pre-K, parents appear to expend maximum effort to properly provide.

Focus upon preschool as the primary dimension of difference also masks the fact that most families rely on at least one regular non-parental care arrangement, on top of preschool for those who have it. Grandparents emerge as the clear first choice for additional regular weekly childcare, supplemental to formal early education for pluralities of Universal Pre-K Parents (n=63) and Private Pre-K Parents (n=49) despite large class differences between the groups.

Reiterating the upshot of descriptive findings on gendered parental time use, No Pre-K Parents stand out for a stunning drop in reliance upon grandparents between the fall and spring semesters in which their preschool-aged children do not attend preschool. From a use rate on par with peer groups with preschool seats in the fall, regular reliance upon grandparent care five or more hours per week drops by more than half over the same four months in which mothers without public or private preschool report spending much more time on childcare themselves. Relatives and siblings are also important to many families but not, as a descriptive matter, relatively wealthy ones studied here. Purchased private childcare provided by nonrelatives in home-based settings appears to be a last resort insofar as families reporting regular use of nonrelative care almost never did so bundled with one or more of the family-provided options.
Figure 5.2  Regular Reliance on Family Care, by PreK Group

- **Grandparent care**
  - No PreK 9/19: 10%
  - No PreK 1/20: 17%
  - Universal PreK 9/19: 43%
  - Universal PreK 1/20: 20%

- **Relative Care**
  - No PreK 9/19: 14%
  - No PreK 1/20: 49%
  - Universal PreK 9/19: 14%
  - Universal PreK 1/20: 54%

- **Sibling Care**
  - No PreK 9/19: 19%
  - No PreK 1/20: 17%
  - Universal PreK 9/19: 19%
  - Universal PreK 1/20: 40%

Notes:

Figure 5.3  % of Families Paying $0/wk. on Childcare, July ‘19 – Jan. ‘20

- **UPK**
  - 7/19: 52%
  - 9/19: 46%
  - 11/19: 35%
  - 01/20: 8%

- **Private PreK**
  - 7/19: 53%
  - 9/19: 46%
  - 01/20: 2%

- **No PreK**
  - 7/19: 46%
  - 9/19: 37%
  - 11/19: 2%

Notes:
Childcare spending is an entirely different story, temporally speaking. Very large differences between groups emerge when school starts. In July of 2019, the same share of public and private preschool users spend $0 per week on childcare, putting both groups well below the half of No Pre-K Parents without summer childcare spending. As soon as school starts, another 10% of Universal Pre-K Parents join the ranks of no regular spending while the bottom drops out for Private Pre-K Parents at the same time. In short, over-time patterns in spending $0 per week on childcare follow about the same trajectory that the maximization alternative (B) expects parents childcare attitudes to follow upon beginning free public preschool, or alternatively, a high-priced substitute. The patterns are mirrored in Figure 5.4 showing average weekly spending conditional on non-zero costs. Low spending No Pre-K Parents are flat over time, whereas Private Pre-K Parents begin spending and Universal Pre-K Parents saving as soon as school begins.

Figure 5.4 Average $/wk. Among Families Spending > $0

Notes:
**Childcare Attitudes**

Adapted from items included in the 2012 International Social Survey Programme’s Gender & Parenting Roles IV survey, two primary outcomes of interest measure parent-level preferences for **public provision** (1) and **public financing** (2) of childcare.⁷⁹

Prefaced with “people have different views on childcare for children under school age,” the first asks: “who do you think should primarily provide childcare?” Categorical beliefs about practical responsibility for care are reduced to a binary preference for **public childcare provision.** For parent i in survey period t, provision preferences are coded positive if “government agencies” or "public schools" is designated favorite over all private options:

\[
\text{Provide} <^K_{it} = 1, \quad \text{if } \{ \ "public schools" \"government agencies" \}
\]

Parents who select "family members," “private childcare providers," “private centers," "employers," or "someone else" with some nongovernment source typed-in, are coded zero to denote preference for privately-provided care arrangements, 

\[
\text{Provide} <^K_{it} = 0.
\]

The online survey instrument permits parents to specify separate preferences for young children of different ages. Granted this opportunity, 7 in 10 New York parents

---

⁷⁹ While these two childcare questions were fielded in several dozen countries for the 2012 ISSP, the gender module including them has not been repeated since. For variable details and original wording, See V33 and V34 in the Variable Report of the 2012 ISSP Gender and Changing Family Roles Survey [International Social Survey Programme](https://www.rivista.org).
indeed distinguish infants and toddlers from preschoolers and kindergartners when selecting most appropriate arrangements. Of parents in the remaining minority who do not indicate conditionally on age at baseline, 2 in 3 indicate "family members" should always provide care. Very few, less than 10% of all parents, say that practical responsibility for early care and education always rests with "government agencies" and/or "public schools."

Drawn from the same cross-national survey, my second attitudinal outcome taps the financial dimension of parents’ beliefs about childcare responsibility: “who do you think should primarily cover the costs of childcare for children under school age?” (International Social Survey Programme, 122.). Here again, categorical choices are reduced to a binary preference for public childcare financing. Parents are positives, $Pay_{it} = 1$, if "the government/public funds" is elected. Preferences for private financial responsibility are zeros once again:

$$Pay_{it} = 0, \quad \text{if } \begin{cases} 
"family itself" \\
"employers" \\
"other" (nongovt) 
\end{cases}$$

A small middle category — "other: shared responsibility," $0 < Pay_{it} < 1$ — arose organically among parents electing to type their own response who indicated government should split costs with private sources, or that government should have responsibility for some families that need help. These parents are distinguished descriptively in Figure 5.6. For hypothesis tests in Section VI, the relatively few parents indicating partially-public or shared responsibility are considered proponents of non-public financial responsibility and coded zero, just like those who select one among the private sources listed above.
Once again grouping parents by preschool arrangement – none, universal public, and private – the bar graphs in this subsection show unadjusted group-mean preferences for public childcare responsibility. Figure 5.5 depicts the first attitudinal outcome discussed above, beliefs about **public responsibility for childcare provision**. Descriptive analysis in this subsection draws on my broader quasi-experimental sample for comparison groups. Comparable figures including the *Lottery Losers* (n=16) comparison group used in analysis with my randomized sample can be found in the Appendix for Section V.

Flexibility allowing respondents to specify different optimal arrangements for infants (≤ *age* 1), toddlers (age 2), preschoolers (age 3-4), and kindergartners (age 5) is captured visually by the steps-like quintet of colored bars repeated for each of three preschool arrangement-based groups. These colored bars measure September levels of support for public (*Y* _it_ = 1) over private (*Y* _it_ = 0) provision; black dashes demark the same view among the same parents, one semester later. First considering baseline views (colored bars), two striking patterns emerge. Despite differences in preschool access, parents’ sharp distinction between the government run, center-based settings deemed appropriate for kindergarteners (yellow), and to a lesser extent 4-year-olds (green), versus uniformly-private care preferences when younger age cohorts are considered.

Descriptively comparing views over the course of early childhood between parents grouped by preschool arrangement, baseline support for publicly-provided care is never highest among *Universal Pre-K Parents*, the expectation of both behavioral alternatives to my hypothesis of policy-adaptive preferences. Casting the same pattern a bit differently, *Private Pre-K Parents* are the strongest supporters of publicly-provided
care for each age-cohort at baseline; the opposite should be true if self-selection and/or maximization drives parent attitudes. Even allowing for a distinction in preferences between lottery losers and parents who do not play, the proportion of nonapplicants among Private Pre-K Parents and No Pre-K Parents included among Constituents (N=172) makes it extremely unlikely that relatively few lottery losers drive these group-level trends.

Speaking much more closely to my predictions of between group differences in parent-level attitude changes over time, gaps between each black dash and colored bar

**Figure 5.5 Belief in Public Responsibility for Childcare Provision**

![Bar chart showing belief in public responsibility for childcare provision by age and group]

Notes: Group average preferences for publicly provided childcare, over all private arrangements are measured separately for children up to 5 years of age.

For each age-cohort, colored bars indicate support in September 2019 & black dashes show support in January 2020.

---

80 These between group differences in baseline support are significant for ages 1, 3, and 5, but not for Age 3 and as obvious from glancing at Figure V.C.1, not when 4-year-olds’ early care and education needs are considered.
indicate change in respective group average attitudes in January 2020, after one full semester of the school year had passed. Consistent with predictions laid out in Section III and to be tested next, in Section VI, the descriptive evidence on parents’ beliefs about responsibility for childcare provision strongly suggests a causal increase in favorability toward public-arrangements for very young children among those with a child in Universal Pre-K. Private Pre-K Parents \( (n=49) \) appear to move in the opposite direction. Individual-level data may still tell a different story, in either or neither case.

Figure 5.6 depicts with blue-to-gray gradient stacked bars unadjusted group-average beliefs about primary responsibility for the costs of childcare before children reach school age. Here, September 2019 and January 2020 views for each preschool group are paired, with Universal Pre-K Parents \( (n=63) \) in the middle set, No Pre-K Parents \( (n=60) \) on their left, and Private Pre-K Parents \( (n=49) \) shown with the right-side set of bars.

Deep blue at the base of the bar for each Pre-K group and survey period depicts the average rate of preference for public childcare financing, parents selecting “the government/ public funds” as primarily responsible for covering costs, \( Pay_{it} = 1 \). Medium blue next in the stack captures responses indicating shared public/private responsibility, \( 0 < Pay_{it} < 1 \). Toward the top, light blues designate two separate private sources, \( Pay_{it} = 0 \), “employers,” and much more substantially “the family,” represented by second-from-top share of each bar.
Notably different than the baseline distribution of views on public provision, there is near majority support for public financial responsibility in each preschool group and both survey waves. This underscores the important distinction in financial and practical views about responsibility for childcare. While publicly provided preschool and childcare programs are generally publicly funded, a wide range of policies in practice in the United States (Child Tax Credit; Child and Dependent Care Tax Credit; Child Care Vouchers), and not (paid maternity, parental, & family leave), target government funds directly to parents and market-based private childcare providers. An over-time rise in views that government should cover childcare costs is clear in Figure 5.6, increasing across groups on average, but disproportionately so for the Universal Pre-K Parents

**Figure 5.6 Beliefs about Primary Financial Responsibility for Childcare**
$(n=63)$ in the middle panel. As with the first attitude, *Private Pre-K Parents* $(n=49)$ begin with highest-levels of endorsement for government responsibility in September. Departing most from my expectations of adaptive preferences detailed in Section III, over-time rise in *Private Pre-K Parents* $(n=49)$ appears in aggregate to be trivial after discounting an almost same-sized rise over the semester for *No Pre-K Parents* $(n=60)$, the common time trend which bumped up support among all parents regardless of arrangement.

All considered, in aggregate, parent beliefs about government responsibility appear consistent with preschool-adapted political preferences, and problematic in certain clear respects from perspectives privileging ideological predisposition or material self-interest to explain covaried parent preferences and preschool arrangements. Nonetheless, surprising dissimilarities between aggregate and citizen-level effects on political participation ([Haselswerdt 2017](https://example.com); [Hobbs and Hopkins 2021a](https://example.com)) and attitudes ([Haselswerdt 2017](https://example.com); [Hobbs and Hopkins 2021a](https://example.com)) is a hallmark of recent policy feedback research.
VI. **One Semester Results: Testing Policy-Adaptive Preferences against Self-Selection & Maximization Alternatives**

This sixth section presents complimentary sets of hypothesis tests, pitting my theory of policy-adaptive political preferences against two plausible alternatives drawn from recent feedback research which expect covariation in policy attitudes and preschool uptake among program-eligible parents: A) self-selection by ideology, party, or affect into preferred arrangements (Lerman and McCabe 2017; Lerman 2018; McCabe 2016b); and, B) beneficiary maximization offset by retaliation from the extra-burdened (Hopkins and Parish 2019; Hobbs and Hopkins 2021a; Jacobs and Mettler 2018).

As theorized in Section III, I expect cross-cutting attitudinal feedback according to arrangement over months, once preschool starts, along discrete practical and financial dimensions of public childcare responsibility. Compared to my later onset but rapidly cumulative mechanism, self-selection and maximization alternatives yield much stronger expectations about group differences in typical views on the public-versus-private nature of childcare responsibilities by September of 2019, a few weeks into preschool.

Moreover, adaptation in preferences caused by parents’ impactful personal experiences with a mutual preschool claim lends to parallel expectations of attitude change over one semester in each set of results below, with potential UPK applicants much like a smaller subset of actual ones (see Table 3.2 for summary). By contrast, self-selection expects null results in analysis with my fully randomized Applicants sample, but significant baseline differences corresponding to own public or private arrangement among parents with preschool seats in the quasi-experimental latter tests (Part B).

In theory, the faster-acting and earlier maximization interpretation of positive and negative feedbacks expects lottery-losing to wipeout once stronger support for public
provision, shortly before the schoolyear starts. This lends to a between-sample difference in expectations under this alternative regarding the evolution of preferences for public provision among Private Pre-K Parents who do, and mostly do not, first try their luck for free public seats; however, this particular observable empirical implication of feedbacks driven by a maximization mechanism is unobserved because it precedes my first survey, a few weeks into the schoolyear.

Nonetheless, my mid-to-late September baseline is timed specifically to adjudicate between a mechanism of maximization and the experiential adaption I propose. In particular, my baseline permits differentiation between attitude change among Universal Pre-K Parents attributable to direct policy experience and that plausibly due to sooner-set interests inherent to (non)receipt of a free public Pre-K seat.

**Part A. Randomized Natural Experiment (N = 79)**

I start with the simplest tests, restricting analysis to “Applicants” (N=79) for whom Universal Pre-K is a fully randomized natural experiment. Toward validating my independent measurement of real-world policy lotteries with an original online survey panel, Tables 6.1 and 6.2 summarize theoretically important pretreatment covariates for Universal Pre-K Parents (n=63) and Lottery Losers (n=16). If these two subgroups looked extremely different from one another, it would call into question the success of my microtargeting and screening procedures for distinguishing Universal Pre-K applicants among program eligible and ineligible parents, recruited myself with survey advertisements delivered on Facebook-owned social media platforms. Fortunately, this is not so.
By chance, Lottery Losers \((n=16)\) recruited into my study are a bit higher income, on average, than the moderate-sized set of winners, Universal Pre-K Parents \((n=63)\). Given relatively few unlucky parents among my fully random sample, the observed $8,000 difference in average household incomes is consistent with expectations of preschool seats granted randomly among those who applied.\(^81\) Once bids for free public ones fall through, seven among sixteen unlucky lottery participants in my study have the means to afford private preschool seats. Compared to a larger set of lucky lottery players with lower average earnings (Table 6.1), the minority of relatively high-earners responsible for pulling up Lottery Losers’ household income on average also drive minor upticks in whiteness, educational attainment, and subjective economic security in Table 2 relative to the first set of sample statistics.\(^82\)

Notwithstanding minor income-correlated demographic differences, successful (Table 6.1) and unsuccessful (Table 6.2) participants in New York State’s public preschool lotteries are very similar in baseline measures of affiliation with each major political party, as well as average ideology. On a 1-7 scale increasing in conservatism,

\(^{81}\) This is clearer when household averages for quasi-experimental comparison groups are considered plausible upper and lower bounds — $64,100 among No Pre-K Parents \((n=60)\) and $127,900 by Private Pre-K Parents \((n=49)\). Although too few to split formally, fully randomized control parents, Lottery Losers \((n=16)\), are a mix of private approaches slightly weighted toward the former, lower earners.

\(^{82}\) My measure for subjective economic insecurity asks respondents about the capacity of their family to sustain without serious hardship if current income sources lost and not replaced. Subjective economic security operationalized this way captures psychologically mediated household-level differences in private capacity to “buffer” against economic risk (Chapter 8, Economic security, OECD 2018). Baseline measures are shown in terms of days in the tables above. Later analysis in Section VII rescales to months for more-reasonably scaled coefficients to compare against the (large) effects of time spent with preschool upon change in parents’ childcare attitudes.
### Table 6.1: *Universal Pre-K Parents*

<table>
<thead>
<tr>
<th>Statistic</th>
<th>Mean</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>Universal Public Preschool</td>
<td>1.00</td>
<td>63</td>
</tr>
<tr>
<td>Private Preschool</td>
<td>0.00</td>
<td>63</td>
</tr>
<tr>
<td>2019 household income</td>
<td>$65.8k</td>
<td>63</td>
</tr>
<tr>
<td>get by without income</td>
<td>37 days</td>
<td>63</td>
</tr>
<tr>
<td>bachelor's degree or higher</td>
<td>0.27</td>
<td>63</td>
</tr>
<tr>
<td>married</td>
<td>0.65</td>
<td>63</td>
</tr>
<tr>
<td>single</td>
<td>0.08</td>
<td>63</td>
</tr>
<tr>
<td>white</td>
<td>0.78</td>
<td>63</td>
</tr>
<tr>
<td>ideology (7 pt. scale)</td>
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<td>63</td>
</tr>
<tr>
<td>Republican</td>
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<td>63</td>
</tr>
<tr>
<td>Democrat</td>
<td>0.33</td>
<td>63</td>
</tr>
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</table>

### Table 6.2: *Lottery Losers* (Applicants)

<table>
<thead>
<tr>
<th>Statistic</th>
<th>Mean</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>Universal Public Preschool</td>
<td>0.00</td>
<td>16</td>
</tr>
<tr>
<td>Private Preschool</td>
<td>0.44</td>
<td>16</td>
</tr>
<tr>
<td>2019 household income</td>
<td>$73.8k</td>
<td>16</td>
</tr>
<tr>
<td>get by without income</td>
<td>58 days</td>
<td>16</td>
</tr>
<tr>
<td>bachelor's or higher child</td>
<td>0.62</td>
<td>16</td>
</tr>
<tr>
<td>married</td>
<td>0.62</td>
<td>16</td>
</tr>
<tr>
<td>single</td>
<td>0.06</td>
<td>16</td>
</tr>
<tr>
<td>white</td>
<td>0.94</td>
<td>16</td>
</tr>
<tr>
<td>ideology (7 pt. scale)</td>
<td>3.44</td>
<td>16</td>
</tr>
<tr>
<td>Republican</td>
<td>0.19</td>
<td>16</td>
</tr>
<tr>
<td>Democrat</td>
<td>0.38</td>
<td>16</td>
</tr>
</tbody>
</table>
September self-ratings average 3.5 for *Universal Pre-K Parents*; average ideology for *Lottery Losers* rounds down to 3.4, ever-so-slightly left of center, reflecting the ideological balance across subgroups and samples demonstrated previously in Figure 4.4, and discussed thereabout.

These first tables, 6.1 and 6.2, also affirm that parenting arrangements are distributed about the same in winning and losing subsets; married couples comprise 62-65% of households, and single mothers a little under 10%, leaving similar shares of one remaining (unlisted) family arrangement, parents raising preschoolers alongside live-in partners. Considered descriptively in Section IV with analysis underscoring the same commonality between *Universal Pre-K Parents* (n=63) and *No Pre-K Parents* (n=60) the quasi-experimental group to whom treated parents are compared below, it seems the main takeaway should be recast to unusually high rates of marriage among the highest income comparison group, *Private Pre-K Parents* (n=49). Marriage is hardly sufficient and all but necessary for the high family income required to purchase into this subgroup.

Turning to regression results, Table 6.3 presents difference-in-differences estimates for one-semester preference changes attributable to divergent experience with New York State’s universal public preschool program. For this first half of analysis in Section VI, Universal Pre-K “treatment” and lack thereof are randomly assigned.

Although the subsample of my panel for whom Universal Pre-K treatment is truly random is somewhat small and weighted toward successful parents over *Lottery Losers* (n=16), I find very large and statistically significant (.01 < p < .11) average treatment
effects, along the lines theorized for both practical and financial attitudes. Relative to losing, winning access to universal public preschool causes parents to desire substantially more government earlier in the lives of children like their own, preferring public provision 13-30 percentage points more than they start the school year when resurveyed after one semester, depending upon whether the hypothetical children under consideration are infants, toddlers, or preschool-aged.

Recall from Figure 3.1, the beginning of schoolyear baseline survey \( (t_0 = September\ 2019) \) is effectively pre-treatment on my theory of preference adaptation through policy experience, but not so through either alternative lens, self-selection (A), or maximization (B). Group-level differences should be mostly set several weeks into the preschool year, if varied political ideologies or economic interests account for patterned childcare preferences and practices among Universal Pre-K eligible parents. If self-selection into preferred arrangements rather than policy feedback underlies differences in parent beliefs about childcare responsibility, Table 6.3 should be filled with null results. Self-interest important in the ways predicted by the second alternative, maximization, will surface in the most substantial significant findings on the “Universal PreK” coefficients in Table 6.3. In each of five difference-in-differences models reported, Universal PreK captures the influence of winning rather than losing the preceding summer’s public preschool lottery upon September beliefs about public childcare responsibility.

\[83\] All hypothesis tests are two-tailed and reported with heteroskedasticity-robust standard errors clustered by parent in parentheses.
By contrast, I theorize differential over-time change in attitudes, onset after school starts. If adaptation induced by divergent personal experience with a mutual claim to universal preschool policy better explains parent preferences, empirical support for my cumulative mechanism will instead be reflected in large and significant coefficients on the interaction term(s) estimating one-semester changes attributable to preschool treatment, the difference-in-differences estimates ($\beta_1$ in Equation 4.2; $\beta_1$ and $\beta_2$ in Equation 4.3).84

The first three columns test competing trait-based and adaptive predictions about parent preferences for publically provided arrangements for very young children over all private childcare options. Guided by descriptive insights from the previous section (V), I model differential change in parent beliefs separately for young children by age cohort, considering beliefs about optimal care for one, two, and three-year-olds separately and in sequence.

Starting in column one with parent views about the care most appropriate for children up to a year old, lived experience with universal preschool for a three or four-year-old boosts support for government provided infant-care by an average of 13 percentage points. Even for this fringe policy position, universal preschool’s impact upon change in parent attitudes approaches statistical significance after about four months with similar coverage for a slightly older young child.

84 As detailed in the fourth section, I use the following OLS regression model to estimate preferences over provision: $Y_{it} = a_i + \lambda_t + \beta_1 Universal PreK_{it} + \epsilon_{it}$. For preferences over public financing of childcare, and all tests in Part B, I extend the basic model with an additional interaction term for Private Pre-K treatment: $Y_{it} = a_i + \lambda_t + \beta_1 Universal PreK_{it} + \beta_2 Private PreK_{it} + \epsilon_{it}$.
Table 6.3: One Semester Difference-in-Differences: Sept.'19–Jan.'20 (Natural Experiment)

<table>
<thead>
<tr>
<th></th>
<th>Government Should Be Primarily Responsible</th>
<th>Childcare Costs</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Childcare Provision</td>
<td></td>
</tr>
<tr>
<td></td>
<td>1-Year-Olds</td>
<td>2-Year-Olds</td>
</tr>
<tr>
<td>Universal PreK</td>
<td>-0.11</td>
<td>-0.26**</td>
</tr>
<tr>
<td></td>
<td>(0.15)</td>
<td>(0.13)</td>
</tr>
<tr>
<td>January 2020</td>
<td>-0.06</td>
<td>-0.19</td>
</tr>
<tr>
<td></td>
<td>(0.14)</td>
<td>(0.13)</td>
</tr>
<tr>
<td>Private PreK</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Universal PreK x Jan. '20</td>
<td>0.13†</td>
<td>0.30**</td>
</tr>
<tr>
<td></td>
<td>(0.08)</td>
<td>(0.14)</td>
</tr>
<tr>
<td>Private PreK x Jan. '20</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

- Fixed effects (two-way)? Yes Yes Yes Yes Yes
- Demographic controls? No No No No No
- Respondents 79 79 79 79 79
- Periods 2 2 2 2 2

Note: Robust Standard Errors in Parentheses. †p<0.11; *p<0.1; **p<0.05; ***p<0.01
Models 2 and 3 take up the same childcare attitude, considering children ages two and three in the 2nd and 3rd columns. In both cases, attitude change attributable to one semester with the public preschool program are staggering in size and statistically significant. Average support for government childcare over private options grows an average of 28-30 percentage points over treated parents’ own views measured a few weeks into free public preschool, mid-to-late September of 2019. Once again, as the difference-in-differences, these estimates already adjust for a common time trend between survey waves (“January 2020”) and parent-level predictors with (unreported) individual fixed effects. Noted at the bottom of Table 6.3, and Tables 6.6 through 6.8 in Part B of this section, heteroskedasticity-robust standard errors clustered by parent are used in all instances to account for the panel structure of data collected for this study.

Notably, aside from predicted changes attributable to a semester spent sending a child to public preschool – evidenced by correctly signed and significant (with the exception of $\beta_2$ capturing Private Pre-K’s effects in Col. 5) interaction terms – Table 6.3’s only statistically significant difference relative to the average Lottery Loser in September is a negative coefficient on Universal PreK in Column 2 modeling public childcare provision preferences for two-year-olds. Substantively, negative signs on this coefficient in the first three columns reveal, with varying degrees of uncertainty, that lottery-winners were generally less supportive of publicly provided care than unlucky peers several weeks into the preschool year. Alongside descriptive findings from Sections IV and V, this baseline difference also appears to be an artifact of happenchance higher income among lottery losing families. In any case, this empirical fact about relative support for public care arrangements as of September is very difficult to reconcile with a parent-level story of reactionary maximizing, and impossible to square with the first alternative hypothesis, self-selection.
Ideological sorting into lotteries along the lines of conditional policy feedback theories developed with puzzling ACA findings (Lerman and McCabe 2017; Lerman et al. 2017) implies little-to-no meaningful difference in baseline preferences for public childcare, over privately provided options, among the randomized Applicants (N=79) sample. Parents analyzed in this first set of empirical tests for parent-level feedback should be strong proponents of public childcare no matter how lotteries turnout for them. Maximization, on the other hand, suggests that Universal Pre-K lottery winners (losers) should be substantially more (less) favorable to government provided care arrangements immediately following their (un)lucky draw, weeks to months before my first measurement of attitudes, depending on the lottery timelines independently set by each school district with a NY State universal preschool grant.

Allowing a slight modification inspired by descriptive findings about group trends in average weekly childcare spending (Figures 5.5 & 5.6) such that maximization sets in soon once free public preschool starts rather than the moment of random assignment, it is a challenge to interpret greater support for government provided childcare among Lottery Losers compared to winners at a point in September when material savings in weekly childcare spending were almost fully realized.

Moving to my second attitudinal outcome, beliefs about public responsibility for childcare costs, two specifications are presented in the right-most columns. Column 4 repeats the same “Single Treatment” diff-in-diff (DD) model used to estimate changes in preferences for public provision among Applicants (N=79). In this case, testing for differential change in beliefs about financial responsibility for childcare, the interaction between binary indicators for universal preschool treatment

123
Because I hypothesize convergence along this attitudinal dimension for this hypothesis test, a second interaction between private preschool treatment \((PrivatePreK_{it} = 0; 1)\) and the January 2020 survey wave \((t_1 = 1)\) is necessary because I expect attitudinal convergence. Recall, my theory of preference adaptation expects both \textit{Private Pre-K Parents} and \textit{Universal Pre-K Parents} to become more supportive over months of public financial responsibility for childcare. Lived experience with a child in preschool, the costliest of arrangements before school age, should lead to views placing greater weight with government, whether parents themselves or taxpayers pick up tuition in their child’s case.

Column 5 properly tests this with the “Dual Treatment” difference-in-differences (DD.2) model. As detailed in the fourth section introducing my research design, this extended specification tests for statistically significant over-time change in attitudes attributable to two separate treatments simultaneously, compared to an implicit baseline of no preschool in September. In the farthest-right column, the effect of a semester in universal public preschool is once again very large and statistically significant. About four months with universal preschool raises the average preference for \textbf{public childcare financing} by 33 percentage points over UPK parent’s beliefs in September. Again, as the difference-in-differences, this coefficient estimate already accounts for a common time-trend and stable individual-level predictors of childcare attitudes.

With only a handful of \textit{Private Pre-K Parents} \((n=49)\) among the small-ish set of \textit{Lottery Losers} \((n=16)\) in my random sample, \(\beta_2\), the second difference-in-differences estimate for \textit{private} preschool treatment added to Model 5 on the far-right, is correctly
signed but quite imprecise. More to the point, including it recovers the significant causal effect of a semester spent with a child in universal preschool upon adaptation in preferences over financial responsibility for childcare, as I theorized it would.

Differently put, my hypothesis of convergent preferences among parents with any preschool along the second financial dimension of childcare responsibility is well supported by Column 5’s significant finding, and by this model’s confirmed better fit than the “Single Treatment” Diff-in-diff (DD) specification in Column 4. Time spent with a child in costly preschool raises expectations that government, instead of families, employers, and other private sources, ought to be financially culpable for childcare. Attitude change through lived experience is similar in magnitude across public and private preschool users, but it is too noisy to say anything certain about average change for the latter subgroup.

These results support my hypothesis with a high degree of internal validity. We know that preschool seats are allocated by lottery; balance in pretreatment covariates important to beliefs about childcare policy lends confidence in my independent recovery of political feedbacks following from this naturally occurring policy experiment. Taken together, qualitative, and quantitative evidence of random assignment plus large and statistically significant differences in attitude change as well as at baseline strongly contradict self-selection (Alternative A) as a plausible interpretation for patterned attitudes I explain with reference to parents’ diverging preschool experiences.

Nonetheless, the relatively small-size and specificity of my random subsample is not without limits. Already demonstrated in the need to resort to the “Dual Treatment” (DD.2) specification for testing belief change along the second financial dimension (far-right columns of Table 3), Lottery Losers (n=16) are not of one kind. They split into
classed private strategies after losing the lottery. Wealthier, whiter, more-educated, almost-always-married parents purchase a private substitute; other families who lose and do not meet all or most of these criteria go without preschool. As discussed in Section V, these mothers stand out for appearing to personally pick up more slack over time without a formal seat. In this group only, mothers report substantially more hours of personally provided parent care and fewer helping hands from grandparents in January 2020 than the fall prior.

Separating Private Pre-K Parents from No Pre-K Parents as below in Part B, is important to contextualizing, and hopefully generalizing, these narrow and credibly causal findings. It is also critical to further differentiating preference adaptation from a second behaviorally-influenced alternative proffered in some recent studies of the ACA’s mixed attitudinal feedbacks, the hypothesis I label “maximization” (Alternative B).

The extended sample of parents analyzed next allows for cleaner differentiation between dynamics of preference adaptation and maximization in stark conflicting expectations once non-applicants enter the picture. As discussed in previous sections, the importance of applicant-status among Private Pre-K Parents is a primary point of theoretical conflict between my mechanism and the maximization alternative principally focused on the uneven statuses policy assigns than the substance by which government programs do and do not palpably affect citizens’ lives.
Part B. Quasi-Experiment (N=172)

Measured against adaptation in the preferences of peers with worse luck, between mid-September of 2019 and mid-January of 2020, random assignment of a Universal Pre-K seat causes an average increase of 13-33 percentage points in parent beliefs that childcare is in some sense the government’s responsibility. This second set of tests seeks to replicate and extend Part A’s natural experimental findings, substantial adaptation in public childcare preferences of both types: support for government provision, and support for government financing (Table 6.3, Cols. 1-3 and 4-5).

As a matter of theory, preference adaptation driven by impactful policy experience will hold with this broader sample of Universal Pre-K “Constituents,” not merely eligible but potential applicants. If experiential adaptation causes parents’ childcare beliefs to covary with preschool uptake, as I argue, neither lottery winning nor losing underlies substantial attitude differences among parents by arrangement.

More than an empirical basis for probing generalizability of the large causal effects uncovered in narrow analysis above, with a fully random but small subsample, increased power in the present analysis permits use of the “Dual Treatment” Difference-in-differences (DD.2) model best capturing my expectation – crosscutting preference adaptation over months due to divergent impactful experiences with a shared policy claim, once preschool starts and routines take hold.

Although demographic balance is not expected between quasi-experimental comparison groups of No Pre-K Parents (n =60) and Private Pre-K Parents (n =49), indeed the opposite, and my estimation approach controls for parent-level confounding, sample statistics tables comparable to the pair presented for Universal Pre-K Parents (6.1) and Lottery Losers (6.2) are included in the interest of symmetry with the first set.
Table 6.4: *No Pre-K Parents* (Constituents)

<table>
<thead>
<tr>
<th>Statistic</th>
<th>Mean</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>Universal Public Preschool</td>
<td>0.00</td>
<td>60</td>
</tr>
<tr>
<td>Private Preschool</td>
<td>0.00</td>
<td>60</td>
</tr>
<tr>
<td>2019 household income</td>
<td>$64.1k</td>
<td>60</td>
</tr>
<tr>
<td>get by without income</td>
<td>35 days</td>
<td>60</td>
</tr>
<tr>
<td>bachelor’s or higher child</td>
<td>0.35</td>
<td>60</td>
</tr>
<tr>
<td>married</td>
<td>0.58</td>
<td>60</td>
</tr>
<tr>
<td>single</td>
<td>0.12</td>
<td>60</td>
</tr>
<tr>
<td>white</td>
<td>0.87</td>
<td>60</td>
</tr>
<tr>
<td>ideology (7 pt. scale)</td>
<td>3.63</td>
<td>60</td>
</tr>
<tr>
<td>Republican</td>
<td>0.13</td>
<td>60</td>
</tr>
<tr>
<td>Democrat</td>
<td>0.28</td>
<td>60</td>
</tr>
</tbody>
</table>

Table 6.5: *Private Pre-K Parents* (Constituents)

<table>
<thead>
<tr>
<th>Statistic</th>
<th>Mean</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>Universal Public Preschool</td>
<td>0.00</td>
<td>49</td>
</tr>
<tr>
<td>Private Preschool</td>
<td>1.00</td>
<td>49</td>
</tr>
<tr>
<td>2019 household income</td>
<td>$127.9k</td>
<td>49</td>
</tr>
<tr>
<td>get by without income</td>
<td>173 days</td>
<td>49</td>
</tr>
<tr>
<td>bachelors degree or higher</td>
<td>0.76</td>
<td>49</td>
</tr>
<tr>
<td>married</td>
<td>0.88</td>
<td>49</td>
</tr>
<tr>
<td>single</td>
<td>0.06</td>
<td>49</td>
</tr>
<tr>
<td>white</td>
<td>0.94</td>
<td>49</td>
</tr>
<tr>
<td>ideology (7 pt. scale)</td>
<td>3.51</td>
<td>49</td>
</tr>
<tr>
<td>Republican</td>
<td>0.24</td>
<td>49</td>
</tr>
<tr>
<td>Democrat</td>
<td>0.47</td>
<td>49</td>
</tr>
</tbody>
</table>
of results. Profound and class-stratified inducements to either purchase or personally-provide childcare when public options are scarce belies a rhetoric of parental “choice” in American childcare, as theorized (III) and discussed descriptively (IV) in prior sections. The upshot is an almost same-sized comparison set of potential applicants without any preschool who, on average, look quite similar to peers who win public seats. Standing apart, a relatively privileged, married, and white population who purchase preschool look different in income-correlated demographics, on average, from parents who do not.

Table 6.6 proceeds to laying the groundwork for extended analysis with a direct comparison of natural and quasi-experimental estimates for differential one semester change in the same, age-specific preference – support for government provided care for three-year-olds -- and then for the latter more-inclusive set of parents, a comparison between my basic and preferred difference-in-differences specifications, with one and two interaction terms estimating significant group-differences in the over-time impacts of preschool experience upon parent preferences.

Column 1 of Table 6.6 reprints the third column of Table 6.3, predicting change in preferences over public childcare provision for three-year-olds among Applicants (Part A). In the middle column (2), Table 6.6 replicates these estimates using the exact same model but my broader, quasi-experimental parent sample of Universal Pre-K Constituents (N=172), instead of actual Applicants (N=79) only. Although the point estimate is a bit smaller (.20 vs. .28 ppts.), moving from my small to medium-sized effective sample improves estimation.

Again using the quasi-experimental sample comprised mostly of potential lottery participants, Column 3 (Table 6.6) adds an additional interaction term for Private Pre-K “treatment” to upgrade the basic “Single Treatment” Difference-in-differences
specification into my preferred, “Dual Treatment” (DD.2)85 version.

As expected, the extended model in Column 3 discerns directionally-split adaptation in beliefs about public responsibility for childcare provision. Estimating changes attributable to a semester spent spending on private preschool simultaneously with the over-time effects of Universal Pre-K, a substantively large negative feedback in support for public provision emerges over months among Private Pre-K Parents—evidenced by a significant and negatively-signed coefficient on the second interaction term included only in the third model of Table 6.6. A more modest remaining positive coefficient on the first interaction term capturing Universal Pre-K’s one semester impact

85 As introduced with Equation 4.3 in Section IV, the difference-in-differences specification best suited to testing my expectations:

\[ Y_{it} = \alpha_i + \lambda_t + \beta_1 Universal\ PreK_{it} + \beta_2 Private\ PreK_{it} + \epsilon_{it} \]
upon preference change is no longer distinguishable from zero. When it comes to three-year-olds, declining average support for public provision among well-off natural-proponents due to experience with purchased preschool accounts for the bulk of one-semester change.

An equally-sized and similarly significant \((.10 < p < .05)\) baseline difference in Model 3 reveals that parents with sufficient need and means to purchase early education despite market rates remain, several weeks into the schoolyear, distinctly-supportive of government provided arrangements for UPK-eligible three-year-olds. This finding about *Private Pre-K Parents* in September 2019 again suggests that my baseline successfully captures parent preferences before mutual experience systematically changes minds, over months, about the desirable degree and nature of government involvement.

The finding is also exactly the opposite of predictions regarding mid-to-late September group-average differences in support for public childcare provision under both Alternative Hypotheses, A and B alike. Theories of self-selection and maximization each expect a **negatively signed** significant coefficient on *Private PreK* in Column 3 (Table 6.7). As emphasized in Part A, significant baseline differences rule out self-selection, the first competing account, and pose preliminary challenges for the second, maximization offset by retaliation. More clear-cut evidence contradictory to Alternative B lies in the significant finding of negative over-time change in public provision preferences among mostly potential applicants in my quasi-experimental set of *Private Pre-K Parents* \((n = 49)\).

Having shown comparability between parent samples and grounded the distinction in findings between basic and preferred models, Table 6.7 transitions to focus fully on hypothesis tests with the broader, quasi-experimental sample. Applying the
same pair of “Single” and “Dual Treatment” difference-in-differences models reported in the middle and right-side columns of the previous table, Table 6.7 reports one semester changes attributable to preschool for closely related attitudes - preferences for publicly-provided care for very young children, ages one (left) and two (right).

Considering the youngest first, Models 1 and 2 estimate average views according to preschool arrangement before and after a semester with public preschool, or private preschool, relative to none. Comparing columns 1 and 2 predicting differential one-semester change in preferences over publicly provided care for one-year-olds, an apparent weakly-significant boost of 10 percentage points in demand for government-provided infant care turns out to be two effects; private and public preschool-using parents split over time. This is consistent with my expectations even though the coefficients lose significance in the second, fully-specified model. Like quasi-experimental findings for 3-year-olds, a large, statistically-significant, and positive coefficient on Private PreK in Column II runs counter to baseline expectations of sharply negative views for this group under each alternative mechanism of attitude formation, self-selection and maximization.

Models 3 and 4 on the right-side of Table 6.7 repeat the exercise for two-year-olds. For this age cohort like the previous two, one larger positive effect attributed to Universal Pre-K treatment is revealed on deeper investigation to be two diverging feedbacks. A small and noisy negative one for over-time impact upon Private Pre-K Parents and a moderate-sized, statistically significant positive feedback among universal preschool beneficiaries. Relative to No Pre-K Parents’ September baseline views
Table 6.7: One Semester Difference-in-Differences: Sept.'19-Jan.'20 (Quasi-Experiment)

<table>
<thead>
<tr>
<th></th>
<th>1-Year-Olds</th>
<th>2-Year-Olds</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td><strong>Universal PreK</strong></td>
<td>-0.05</td>
<td>0.01</td>
</tr>
<tr>
<td></td>
<td>(0.05)</td>
<td>(0.06)</td>
</tr>
<tr>
<td><strong>January 2020</strong></td>
<td>-0.04</td>
<td>-0.00</td>
</tr>
<tr>
<td></td>
<td>(0.03)</td>
<td>(0.06)</td>
</tr>
<tr>
<td><strong>Private PreK</strong></td>
<td>0.14**</td>
<td>(0.06)</td>
</tr>
<tr>
<td><strong>Universal PreK x Jan. '20</strong></td>
<td>0.10*</td>
<td>0.06</td>
</tr>
<tr>
<td></td>
<td>(0.06)</td>
<td>(0.06)</td>
</tr>
<tr>
<td><strong>Private PreK x Jan. '20</strong></td>
<td>-0.08</td>
<td>(0.07)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Fixed effects (two-way)?</th>
<th>Yes</th>
<th>Yes</th>
<th>Yes</th>
<th>Yes</th>
</tr>
</thead>
<tbody>
<tr>
<td>Demographic controls?</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>Respondents</td>
<td>172</td>
<td>172</td>
<td>172</td>
<td>172</td>
</tr>
<tr>
<td>Periods</td>
<td>2</td>
<td>2</td>
<td>2</td>
<td>2</td>
</tr>
</tbody>
</table>

*Note: Robust standard errors are reported.*

*p<0.1; **p<0.05; ***p<0.01
and accounting for a common time trend as well as Private Pre-K’s impact over the same months, one semester with free public preschool increases support for government provided toddler-care, over all private options, by an average of 16 percentage points.

Moving last to views about responsibility for childcare costs, the second type of attitude on my account although not meaningfully different from provision with respect to parent-level politics under maximization, Table 6.8 estimates group-differences in one-semester change in beliefs about public versus private primary responsibility for covering childcare costs. On this matter, my theory of policy-adaptive political preferences expects preschool users of all kinds to converge upon heightened demand for government financing of childcare after several months of formal preschool.

Along these lines, Model 1 (Table 6.8) missing the second interaction term should return null results. It does. With my preferred specification instead, in Column 2, Universal Pre-K’s one semester effects are substantial and significant. Although smaller than the same estimate for actual Applicants (33 percentage point increase, p <.05), I find an average one semester rise of 18 percentage points in the view that childcare costs should be government’s responsibility. As before when testing this financial attitude, specifying an extra interaction term to distinguish experiences among Private Pre-K Parents from the baseline condition of no formal early education (“No Pre-K”) is essential to recovering positive over-time effects of the public program, even though the boost in demand for public subsidization of childcare costs among private users is noisy.
Table 6.8: One Semester Difference-in-Differences: Sept.'19-Jan.'20

<table>
<thead>
<tr>
<th></th>
<th>Constituents (1)</th>
<th>Constituents (DD.2)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>January 2020</strong></td>
<td>-0.01</td>
<td>-0.07</td>
</tr>
<tr>
<td></td>
<td>(0.05)</td>
<td>(0.07)</td>
</tr>
<tr>
<td><strong>Universal PreK</strong></td>
<td>0.05</td>
<td>0.07</td>
</tr>
<tr>
<td></td>
<td>(0.08)</td>
<td>(0.09)</td>
</tr>
<tr>
<td><strong>Private PreK</strong></td>
<td></td>
<td>0.05</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.10)</td>
</tr>
<tr>
<td><strong>Universal PreK x Jan. '20</strong></td>
<td>0.12</td>
<td>0.18*</td>
</tr>
<tr>
<td></td>
<td>(0.08)</td>
<td>(0.09)</td>
</tr>
<tr>
<td><strong>Private PreK x Jan. '20</strong></td>
<td></td>
<td>0.13</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.11)</td>
</tr>
</tbody>
</table>

| Fixed effects (two-way)?     | Yes              | Yes                 |
| Demographic controls?        | No               | No                  |
| Respondents Sample           | 172              | 172                 |
| Periods                      | QuasiExp.        | QuasiExp.           |
|                              | 2                | 2                   |

*Note: Robust standard errors in parentheses.*

*p<0.1; **p<0.05; ***p<0.01
VII. DISCUSSION

Complementary natural and quasi-experimental tests of the adaptive preferences hypothesis against alternatives yield credibly causal and generalizable evidence that public investments in preschool can be politically transformative for families, adding to economic and educational impacts long-recognized as extraordinary at the micro and macro-levels. More than finding modern evidence of substantial positive policy feedbacks alongside negative ones, I explain both types of attitude shift with reference to direct, personal experience with valuable, visible policy – partway-funded public pre-K.

Taken together, correctly signed and generally significant over-time findings provide strong support for the experiential basis of adaptation in parent preferences over months, once daily experience with mutual UPK claims diverge. This range of evidence is incompatible with either alternative, self-selection or maximization, as the mechanism responsible for varied parent beliefs about the appropriate allocation of public versus private childcare responsibility. In particular, Private Pre-K Parents are the greatest proponents of government provided childcare several weeks into school. Then, new beliefs emerge in subsequent months, through mutual experience with pricey preschool.

More or less stable demographic traits important in American politics are in large part accounted for by design, with parent-level predictors as well as common time trends controlled for by “two way” fixed effects in OLS regression estimates that identify average between group differences in citizen-level attitude change over one semester. The difference in differences, that is.

Even so, it bears note that I find no sign that Universal Pre-K’s political effects are conditional on pretreatment traits such as race, political party, and relative household income. This breaks from past policy feedback findings, new and old. Specifically, Universal Pre-K engenders greater expectation of government responsibility
among all kinds of parents who receive it: white and nonwhite; the relatively poor, average income, and well-off; Democrats and Republicans, much like Independents and the unaffiliated.

As discussed throughout this study, heterogenous effects for citizens of varied socioeconomic, racial, ideological, and geographic backgrounds have become an increasingly prevalent feature in policy feedback research as these conceptually-distinct cleavages within the polity have increasingly sorted citizens into polarized party teams. Lerman and McCabe, in joint and separate work, as well as Jacobs and Mettler make similar cases about the conditionality of feedback according to degree of trust in government, as well as levels of “political knowledge,” operationalized by the former pair of scholars as attunement to elite partisan cues and policy positions.

Foundational accounts of citizen-level feedback also suggest that economically vulnerable and otherwise marginalized citizens who experience the most meaningful material and interpretive gains through personal policy encounters ought to incur the largest increases in policy support and political engagement (Campbell 2003; Mettler 2005). Notably, Mettler’s (2002) evidence points to an exception for the most economically disadvantaged among white GIs; feedback boosting civic and political participation was most pronounced among returned soldiers from low-to-moderate income backgrounds who were not otherwise predisposed to deeply engage, or attend college.

My findings are different. Specifically, preference adaptation caused by preschool experience appears to be homogenous. One semester changes swamp the magnitude of baseline attitudinal differences according to personal traits.
Although my effective samples are small (natural experiment) and modest-sized (quasi-experiment), theoretically guided introduction of demographic controls in the next few pages reveal that preschool changes parents’ beliefs about government responsibility regardless of partisanship, relationship status, race, or relative income. Accounting for schoolyear-coincident changes in ideology, perceived economic security, and each parent’s degree of government trust has no discernable impact whatsoever.

Departing with recent and classic feedback scholarship alike, I find no evidence among my panel of subgroup heterogeneity within treatment conditions in the emergence or nature of preference adaptation. Similarly-aimed analyses with demographically-restricted subsets inspired by several recent feedback studies (McCabe 2016b; Hopkins and Parish 2019) are excluded from formal discussion because they too show no sign of heterogeneity. More generally, my same difference-in-differences tests for political adaptation with subsets of parents restricted according to baseline ideology (most conservative; most liberal), income (top, middle, and bottom categories within 5-level ordered factor), and race (white; nonwhite), among other traits considered in this discussion dramatically undercut power without contributing or amending any of the empirical insights gleaned through comparing estimates with my full Applicants (N=79) and Constituents (N=172) samples, using one or two interaction terms to specify one semester changes attributable to preschool “treatment,” without and with demographic controls – including stable traits and/or potentially-variable confounders measured in each survey wave.

To provide substantive context for the attitudinal impacts of public and private preschool presented in Section VI without covariate adjustment, Figure 7.1 plots a total of seven unadjusted and adjusted (two way fixed effect) OLS regression model estimates of Universal Pre-K’s average one semester treatment effects upon change in support for
government-provided care for toddlers (age 2). For all point estimates, thin lines connote 95% confidence intervals while the thicker segments show 90% intervals.

Toward the middle of the figure, the primary coefficient of interest labeled “UPK x Jan. ‘20” is always large and significant at the 90%, if not the 95% level. For OLS regression coefficients, the -1 to 1 scale is interpretable in straightforward percentage point terms. For example, the “UPK x Jan. ‘20” coefficient estimates ranging from .16 to more than .30 indicate an average 16-30 percentage point increase in support for government provided toddler-care attributable to one semester’s “treatment” with Universal Pre-K. Like regression tables in the previous section, baseline attitudes among public (“UPK”) and private (“Private”) preschool users as well as any common over-time trend in views between September of 2019 and January of 2020 (“Jan. ‘20”) are reported in the next set of figures.

Only three sets of baseline and over-time change estimates appear for Private Pre-K Parents, corresponding to three of seven models estimated with my preferred “Dual Treatment” Difference-in-differences specification. One visual indicator of this model’s better theoretical fit is the difference between time trend estimates (bottom set of estimates in the figure) with and without the second interaction term. Models 2 in teal, 3 in green, and 7 in salmon specify Private Pre-K treatment and show insignificant over time changes which cross the zero-line when “Jan. ‘20” captures the trajectory for No Pre-K Parents only. By inverse, the pink, purple, and blue time trend estimates for fully random Applicants (models 1, 4 and 5) as well as the yellow estimate corresponding to my quasi-experimental Constituents sample (model 6), are inflated by the unspecified and negatively signed over-time effects of Private Pre-K treatment.
Figure 7.1 Coefficient Plot Government Should Provide Care for 2-Year-Olds

*Key continues next page with “stable” and “variable” control sets and Diff-in-diff spec. details."
Directionally speaking and with respect to magnitude, the same pattern affects my main estimates of interest, the middle set for the interaction term capturing one semester effects of UPK treatment. Models 2, 3, and 7 based upon my “Dual Treatment” (DD.2) specification estimate smaller treatment effects than pink, purple, blue, and yellow estimates.

However, adding the second interaction does not kill the statistical significance in this instance. Across all seven models, one semester with universal preschool yields...
increases in support for government provided toddler-care significantly different than the experiences of comparison parents. Whether the gain is almost 20 percentage points or more than 30, the substantive interpretation that experience with universal public preschool causes parents’ attitudes to change within four months holds unscathed.

Models 5-7 plot the unadjusted difference-in-differences estimates reported in Section VI. In blue and yellow, Models 5 and 6 show point estimates for Applicants (N=79) and Constituents (N=172) obtained with my “Single Treatment” Difference-in-differences specification. Model 7 (salmon color) shows estimates from my preferred “Dual Treatment” Diff-in-diff, including an extra interaction term for Private Pre-K “treatment” in estimates with my larger, inclusive sample of parents.

Models 3 (green) and 4 (purple) include time-changing controls measured with each survey wave. Specifically, I test for schoolyear coincident group-specific changes in political ideology (1-7 scale increasing in conservatism), subjective economic security (number of months able to get by without serious hardship if family lost all sources if income), and government trust (4-point Likert ranging from strongly disagree to strongly agree). For comparability with previous research, I use a measure popular despite its ambiguity prompting citizens as to whether government “can usually be trusted to do what’s right.”

Finally, models 1 (pink) and 2 (teal) add stable demographics to the variant controls included in 3 and 4. Although all estimates reported in the study account for stable confounding with parent-level fixed effects, these two models include parent-level dummy variables for identification as a Republican, a single mother, and nonwhite (coded 1 for parents identifying as nonwhite or belonging to more than one race). These
specifications also include a 5-level ordered factor capturing 2019 pretax household income, self-reported in Jan. 2020, in which “$100,000 or more” is the highest category.

These treatment effect, baseline, and time-trend estimates are plotted alongside corresponding coefficients for a selection of the theoretically important traits and time-variant potential confounders suggested by prior research and controlled for in models 1-4, as just detailed.

Starting from the top of Figure 7.1 with a stable trait understood to profoundly shape modern American politics, and increasingly suspected of interfering with policy’s positive feedback prospects, Republican Applicants in pink are significantly less supportive (about 15 percentage points) than non-Republican Applicants at baseline. This expected partisan difference is less than half the size of Universal Pre-K’s more than 30 percentage point positive effect upon attitude change. Impactful personal policy experience compensates for the drag of GOP membership twice-over within about four months. Also at the top of Figure 7.1, 90% and 95% confidence intervals crossing the zero line indicate that baseline differences in public childcare support attributable to Republican partisanship are not statistically significant among my broader, quasi-experimental sample, estimated in teal with the same set of variable and stable controls.

Consistent with descriptive findings about relatively high-income and strong baseline support for government childcare responsibilities, income above $100,000 is shown to have a similarly modest (about 15 percentage points) but more reliable positive impact upon support as of mid-to-late September, a few weeks into preschool, regardless of lottery participation. Here again, the point estimate for Applicants from a fully-controlled difference-in-differences model (#1) is in pink while the corresponding estimate for Constituents is right below in teal (model 2).
Potential over-time confounders measured in each survey wave are grouped toward the middle of Figure 7.1. In each case and every model specification, these time changing factors are meaningless to parents’ change in beliefs about public versus private childcare responsibility. Changes in ideology, economic security, government trust do not affect childcare attitudes.

Juxtaposed with citizen-level traits and quantities understood to profoundly affect Americans’ social policy attitudes, the impact of divergent preschool experience upon parents’ preferences for government in childcare is overwhelming, persistent, and quite stable across a variety of models.

Given magnitudes at best on the order of Republicanism in Figure 7.1 but usually much more like the non-finding on government distrust, Figure 7.2 does not plot point estimates and confidence intervals for the control variable coefficients listed at the top of 7.1. Instead, the pair of figures focus comparison on my unadjusted estimates with adjusted versions predicting Universal Pre-K’s one-semester treatment effects and group differences at baseline. Estimates obtained with my larger quasi-experimental sample additionally plot one-semester changes attributable to months spent spending on private preschool. Views about government provision for 3-year-olds are on the left side, and 1-year-olds are on the right.

There are some differences by age. For example, change in attitudes about 3 year-olds is dominated by the negative shift among Private Pre-K users, whereas positive shifts in support among UPK users appear more important to the politics of government provided infant care in the left panel. But, taking a step out, the general finding that UPK boosts support over months among beneficiaries while natural proponents of government childcare are turned off over the same time by experience with pricey
Figure 7.2 Coefficient Plot Government Should Provide Care for 3-Year-Olds (L) & 1-Year-Olds (R)

1. Applicants (N=79): Single Treatment DD, variable and stable demographics
2. Constituents (N=172): Dual Treatment DD.2, variable and stable demographics
3. Constituents (N=172): Dual Treatment DD.2, variable demographics
4. Applicants (N=79): Single Treatment DD, variable demographics
5. Applicants (N=79): Single Treatment DD
6. Constituents (N=172): Single Treatment DD
7. Constituents (N=172): Dual Treatment DD.2
early education substitutes.

All considered, adding demographic controls has no impact at all upon the interpretations offered previously, in the results section (VI). Large and significant one-semester effects estimated with my fully randomized Applicants in Part A and broader sample of UPK Constituents in the latter results hold up to the personal traits most prevalent in conventional behavioral theorizing of citizen preferences. Changes in ideology, economic security, and government trust cannot account for the adaptation caused by divergent experience with universal preschool. Extending my basic difference-in-differences specification with an extra interaction term for private preschool treatment also continues to perform exactly as theorized and discussed above, once additional sets of demographic controls are brought into the picture.
VIII. **Conclusion**

In conclusion, I briefly elaborate upon two kinds of unforeseen and unintended, perverse consequence following from New York State’s seemingly fair approach to administering supply-constrained “universal” preschool. For many feedback theory-based reasons this public program stands out for particular potential to swiftly change parents’ fundamental beliefs about government’s appropriate and desirable role. The same features in context of the uncertainty induced by lottery assignment of these life-changing preschool seats suggest that the same experiences may also serve as especially profound reinforcement, for some parents, of negative priors about how and for whom democracy works in the United States.

Within context of characteristically decentralized and capricious contemporary social provision, American government may be most susceptible to deeper delegitimization in rare and random instances where benefits make a profound difference in citizen’s lives. New York State’s underfunded and arbitrary implementation approach might, for some parents, reinforce distrust of government intentions and motives because preschool benefits are transformative. Underfunded public tools capable of improving families’ lives and broadening opportunity for the youngest citizens are in plain view among Universal Pre-K beneficiaries, more and more over months. Such threats loom even when Americans attribute gains in wellbeing to identifiable government benefits in their own lives (Jacobs and Mettler 2018; Mettler 2018). Beneficiaries may come to deeply appreciate an instance of public support, and from it conclude they cannot generally rely on similar. Who expects to win the lottery twice?

Stepping back from micro-level speculation to observed trends, cross-cutting patterns of attitude change according to preschool use suggests geographically
concentrated rollout closer to scale in select districts as a wiser growth strategy. Sprinkling at least a few funded classrooms in most of the Empire State’s eight-hundred school districts is sound politics by conventional Downsian thinking about the given population among parents ideologically (or economically) inclined to participate in a free public preschool program; this logic breaks badly if, as I argue, parent preferences are largely endogenous to personal experiences with impactful policy, importantly including policy-forced reliance on private resources to meet early care and education needs.

From a policy feedback perspective, partial public funding can be spent more wisely toward shoring up, and ideally growing, Universal Pre-K’s base of core support. By investing in geographically concentrated access and incorporating more high-income families into public education extra-early, reallocation of current spending would be expected to build broader and cross-class local-coalitions in support of Universal Pre-K. Additional locations would be added as funds permit, rather than auctioning off more seats in each School District with a program grant.

Above all, my research suggests acute need will not suffice to inspire mass demand for policies that support overstretched and struggling families in the United States. Absent lived experience with a valuable and visible local benefit on par with universal preschool, Americans have neither the political resources nor relevant policy-

86 In addition to state and local universal preschool programs approximating the one studied here, paid family and medical leave laws now enacted in 9 states and Washington D.C. ("Paid Family and Sick Leave in the U.S" 2020) is another example of a policy expected to have similar potential, if sufficiently generous and available to offer a realistic option for parents among the roughly 8 in 10 American workers who do not have employment-based access to paid family leave. See Table 31 of the (Bureau of Labor Statistics 2020).
referents to recognize shared political identity and interests as parents, let alone to mobilize around mutual expectations and identified governmental solutions.\footnote{I thank Andrea Campbell for referring me to Sandra Levitsky’s excellent Caring for our Own, on the closely-related social policy matter of eldercare in the United States (\textit{Levitsky 2014}).}

Instead, the dynamics of political adaptation uncovered here suggest American families without access to public childcare options will continue to pursue class-stratified private strategies. Born of necessity, the decades long American practice of (mothers) cobbling family resources to personally meet own childcare needs \textit{only} powerfully reinforces status quo nonexpectations of government; in turn, routine private childcare reproduces gendered and racialized intergenerational disparities that fragment experiences of democratic citizenship before day one in the United States. From the adaptive political feedback perspective advanced here, it will require more, actually-universal, and permanent federal policies designed to meaningfully support families while inspiring their confident support to reorient vicious politics of private responsibility into a virtuous cycle in which the American political economy no longer precludes but promotes accessible opportunities regardless of birth circumstances.

My study provides rare evidence of a partial remedy with profound potential to build the United States back better, improving the inclusivity and productivity of American democracy as much as our macroeconomy.
IX. SUPPLEMENTAL APPENDICES

Section IV Appendix: Research Design & Data

A. Direct Recruitment of Panel: Microtargeted Social Media Campaign

Advertising Creative

![Image of the New York Parenting Project advertisement]
The recruitments advertisements in this study consisted of still images with a short text description and shorter text banner. Ads were shown across the platforms owned by Facebook in the company’s discretion within my audience parameters (New York residents with preschoolers). The primary campaign graphic and sample ad text are provided above.

### Advertising Exposure Statistics

<table>
<thead>
<tr>
<th>Facebook Recruitment Campaign per parent costs, by gender</th>
<th>total (#) completions</th>
<th>$ per completion</th>
</tr>
</thead>
<tbody>
<tr>
<td>September-19 male</td>
<td>8</td>
<td>$9.43</td>
</tr>
<tr>
<td>September-19 female</td>
<td>191</td>
<td>$5.20</td>
</tr>
<tr>
<td>September-19 unknown</td>
<td>2</td>
<td>$2.61</td>
</tr>
<tr>
<td>overall</td>
<td>201</td>
<td>$5.34</td>
</tr>
</tbody>
</table>
B. Panel Prescreens & Data Processing

New Yorkers with preschoolers were microtargeted using Facebook’s advertising platform. Interested parents who clicked the ad or typed the study URL featured in the social media ads into a browser directly, landed on a study homepage providing full details about participation in a series of paid online parenting surveys conducted for academic research and invited interested parents to proceed with informed consent and eligibility prescreens, before proceeding to the baseline survey, fielded in mid-to-late September of 2019 to measure parents attitudes and experiences just after the start of school.

Interested parents who completed informed consent were further prescreened, (1st) to verify successful microtargeting of New Yorkers with preschoolers, and (2nd) second to collect UPK applications history details, including lottery participation and reasons for nonapplication.

This first prescreen confirmed that willing participants met the sole membership criteria for my panel, New York State residents UPK-eligible as parents or guardians to children ages three and four, within context of family background. To mask the study’s more specific interests, similar details about children’s ages and school arrangements were gathered for all household members aged 10 or younger. This weeded out some New Yorkers with 5-year-olds and a few who’s oldest were still two; Facebook was stunningly accurate overall.

Parents without an eligible UPK-aged child were screened out for “failing” the first prescreen at this phase, thanked with a vague message about ineligibility, and never counted among my panel.
Upon confirming an eligible three or four-year old, the second prescreen is designed to distinguish actual and potential UPK applicants, the broadest population of interest for the current study, from my panel including two types of UPK-eligible who are not plausibly viewed as potential applicants due to an aversion or special needs.

Using preliminary preschool-groupings based on school arrangement filed of this question ascertains whether the preschoolers attend public, private, or no preschool. The same question led to inductive discovery of 12 families with special needs preschoolers. IEP and Head Start parents remain part of my panel study although they are excluded from analysis here.
Are you @first$Name@last$Name?

What is your name?

First Name

Last Name

Please enter your personal email address below. Note: a valid email address is required for this project so that we can pay and communicate with participants.

(Optional) Consider providing a personal phone number as a helpful backup form of contact in case we cannot reach you by email. (Not required but recommended)

Invalid email 1

It looks like you made a typo. We’ve detected that @entered$Email is not a valid email address. After 3 invalid attempts, you will lose access to this online survey.

Please enter your valid email address below if you want to participate in The New York Parenting Project.

Invalid email 2

We’ve detected that @entered$Email2 is not a valid email address. You have 1 more chance to enter a valid email address.

Please enter your valid email address below if you want to participate.
Are you the parent, guardian, or caretaker of a child(ren) age 6 or younger with whom you live?

- Yes, I'm a mother
- Yes, I'm a father
- Yes, I'm a caretaker with responsibility for young children in my household (such as a grandparent, stepparent, the partner of a parent, or another adult who shares responsibility for supporting the children).
- No, my child(ren) age 6 or younger do not live with me.
- No, I'm not a parent or guardian for any young children.
- Other:

You do not live in a household with elementary school or preschool-aged children?

- Yes, that's correct.
- No, that's incorrect. I live in a household with children younger than 6 years old.

Do you help with or share responsibility for raising the child(ren) you live with?

- 
- 

how are you related to the child(ren) you live with and help raise?

PreK 2b -- Birth Years

What ages are your children? Please select all applicable birth years from this list.

For each year marked "YES," indicate how many of your children were born then in the far-right column.

<table>
<thead>
<tr>
<th>My Child's Birth Year?</th>
<th># of children born that year:</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>0</td>
</tr>
<tr>
<td>2019</td>
<td>0</td>
</tr>
<tr>
<td>2018</td>
<td>0</td>
</tr>
<tr>
<td>2017</td>
<td>0</td>
</tr>
<tr>
<td>2016</td>
<td>0</td>
</tr>
<tr>
<td>2015</td>
<td>0</td>
</tr>
<tr>
<td>2014</td>
<td>0</td>
</tr>
<tr>
<td>2013</td>
<td>0</td>
</tr>
</tbody>
</table>

PreK details

One goal of this project is to learn about families' early education options and choices in the Empire State. Tell us about your family's early education arrangements for 2019-2020.

Please Note: the survey requires at least your family's School District of residence and current school/preschool arrangements.

It is optional to provide a first name or initial for each child. This information allows for a personalized survey experience and will help avoid confusion in later surveys. If you choose to leave the name fields blank, each child will be referred to by their birth year and order for the remainder of this study (for example, twin 3-year-olds without names or initials will be "Child Born 2016" and "2nd Child Born 2016").
Constituents (N=172) Subsample

Of 182 parents completing both the September 2019 and January 2020 surveys, only one indicated lack of support as the reason for not applying for universal preschool. Six parents who selected the “other” response and explained nonapplication in their own words, along with three more who said “other” but did not provide reasoning to rule aversion in or out, are coded as potentially-adverse to the public program. In total then, all but ten parents verified for eligible children are included in quasi-experimental analysis of attitude changes among, UPK part B of Section VI. Among respondents who selected “other” and provided their own explanations for not applying to UPK, the following statements were judged adverse in the sense that these respondents would not likely accept a UPK seat on offer:
Among respondents who selected “other” and provided their own explanations for not applying to UPK, the following statements were judged adverse in the sense that these respondents would not likely accept a UPK seat on offer:

1. We homeschool preschool
2. I prefer to keep my children home with me and delay formal education until kindergarten.
3. Don't send our kids to school
4. Do not trust the city's schools, would need full day pre-k.
5. Just didn’t feel comfortable sending my children
6. Felt more comfortable doing a private, few days a week before transitioning to UPK
C. Sample Demographics

Figure A.4.1 2019 Household Income, by Pre-K Group & Sample

A. 2019 Household Income, Applicants (N=79)

- No PreK (n=9)
- UPK (n=63)
- Private PreK (n=7)

B. 2019 Household Income, Constituents (N=172)

- No PreK (n=60)
- UPK (n=63)
- Private PreK (n=49)
Figure A.4.2 2019 Household Income, by Pre-K Arrangement

- **Lottery Losers (n=16)**: $100,000 or more, $20,000 - $34,999, $50,000 - $99,000, $35,000 - $59,999
- **UPK (n=63)**: $100,000 or more, $20,000 - $34,999, $35,000 - $49,999
- **No PreK (n=60)**: $20,000 - $34,999, $35,000 - $49,999
- **Private PreK (n=49)**: Under $20,000, $20,000 - $34,999, $35,000 - $59,999

The chart shows the distribution of household income for different groups based on their pre-kindergarten arrangement.
X. **Appendix: Parenting Experiences & Childcare Attitudes**

**Parenting Experiences**

**Resource Effects**

Figure A.5.1 Maternal Employment Rates by Pre-K Group
FIGURE A.5.2 WEEKLY HOURS OF PAID WORK & PARENT CARE BY PRE-K GROUP & MATERNAL EMPLOYMENT

September 2019

![Bar chart for September 2019 showing hours of paid work and childcare for employed and nonworking single mothers.]

January 2020

![Bar chart for January 2020 showing hours of paid work and childcare for employed and nonworking single mothers.]

Notes: bar graphs visualize parents’ weekly hours spent on paid employment and childcare in September of 2019 by mothers’ employment and partnership statuses. Thus, nonworking mothers in the bottom row have zero average hours in paid work. For the same reason, single mothers shown in the right column are missing bars corresponding to weekly time spent on employment and childcare by spouses and live-in partners to the mostly-mothers who comprise my panel.
Figure A.5.3  Regular Non-Relative Care, by PreK Group

Nonrelative Caretaker  Family Day Care

<table>
<thead>
<tr>
<th>Group</th>
<th>Nonrelative Caretaker</th>
<th>Family Day Care</th>
</tr>
</thead>
<tbody>
<tr>
<td>PreK 9/19 No PreK 1/20</td>
<td>14%</td>
<td>8%</td>
</tr>
<tr>
<td>PreK 9/19 No PreK 1/20</td>
<td>8%</td>
<td>13%</td>
</tr>
<tr>
<td>PreK 9/19 No PreK 1/20</td>
<td>8%</td>
<td>13%</td>
</tr>
<tr>
<td>PreK 9/19 No PreK 1/20</td>
<td>11%</td>
<td>13%</td>
</tr>
<tr>
<td>PreK 9/19 No PreK 1/20</td>
<td>15%</td>
<td>13%</td>
</tr>
<tr>
<td>PreK 9/19 No PreK 1/20</td>
<td>13%</td>
<td>8%</td>
</tr>
</tbody>
</table>


Childcare attitudes

Figure A.5.4 Beliefs About Public Provision of Childcare

Prefer Publicly Provided Childcare for Children Up to 5-Years-Old

Colored Bars indicate support in September 2019 & black dashes show support in January 2020

Figure A.5.5 Beliefs About Public Financing of Childcare

Primary Responsibility for Covering Childcare Costs
WORKS CITED


McCabe, Katherine T. 2016a. "Attitude Responsiveness and Partisan Bias: Direct Experience with the Affordable Care Act."


OECD. 2018. *For Good Measure*.


