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OF MARKETS AND MARRIAGES:
A MULTIDISCIPLINARY, MULTIBOOK REVIEW ESSAY OF RECENT WORK ON THE CAUSES OF U.S. POVERTY

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I have nothing to disclose. The views expressed herein are those of the author and do not necessarily reflect the views of the National Bureau of Economic Research.

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ABSTRACT

I discuss recent books offering differing explanations for persistent U.S. poverty. Desmond (2023) argues that aid to low-income Americans is captured by more powerful market actors. I contextualize this concern as about incidence and consider both policies for changing incidence (by changing outside options) and supplemental critiques focused on reducing deadweight loss as well as reapportioning surplus. Kearney (2023) argues that declining marriage means American children grow up in less-resourced families. I suggest this decline may reflect shortfalls in “soft” skills needed to make marriage efficient in an era when men are increasingly a source of family economic instability while women’s outside options improve. I consider policies to improve soft skills and policies supporting family economic stability, which could reduce spousal-skill requirements. I conclude by encouraging engagement with the emerging policy feedback literature, which explores why popular, evidence-based policies like those discussed have not so far been adopted in the U.S.

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Of Markets and Marriages: A Multidisciplinary, Multibook Review Essay of Recent Work on the Causes of U.S. Poverty

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I. Introduction

It is a perennial question: how is it that in perhaps the richest country in the history of the world, such a large number of people struggle to simply acquire the basic necessities of life? Clearly merely increasing the total quantity of goods and services produced in the market does not address this core problem facing society.

A variety of new books in social science fields both inside and outside of economics turn our attention in novel ways to nodes of interaction other than consumer markets: the employer-employee relationship, the relationship between mothers and fathers, and the relationship between voters and policymakers. All of these are relationships that the tools of economics are well-suited to study. Indeed, the arguments in these books raise a number of interesting hypotheses that could usefully be tested in economics.

The most highly publicized of these books are Matthew Desmond's *Poverty, by America* and Melissa Kearney's *The Two-Parent Privilege*, which offer instructively different perspectives on the persistence of poverty in the United States. Each calls attention to an important issue by documenting a striking trend: Desmond illustrates that market poverty has been stagnant over the past several decades, while Kearney shows that child poverty has fallen for single-parent and married-parent families but has moved little overall as family composition has shifted toward unmarried arrangements. Each also lays out a set of sharp, testable arguments that provide numerous starting points for researchers looking for empirical traction. Both provide well-evidenced policy implications made accessible and compelling to the general reader.

Each also relies rhetorically on identifying a "culprit" for persistent poverty in a way that, however, ultimately undercuts their proposals for fighting it. In the case of Kearney's book, the framing around the culprit—namely, the putative modern norm that marriage is unimportant for childrearing—is at odds in important ways with the substance of the evidence and recommendations she presents as read by an economist. In the case of Desmond, the culprit is the reader, whose comfortable life depends on the immiseration of those less fortunate—an argument that leads toward strict zero-sum logic and resistance to recognizing even fairly straightforward possibilities for mutual gains.

In addition to discussing these two focal books, in this essay I review a variety of related work as I explore the plausibility of the authors' arguments in the context of existing research, frame their hypotheses within economic theory, and propose ways economists might empirically test their implied causal claims. Despite the critiques I offer, both books are eminently worth closely reading, as are the other works I cite below.

II. Research that focuses on work: Desmond, *Poverty, by America*

Matthew Desmond, a sociologist at Princeton and Pulitzer-prize winner, has written what is likely the highest-profile book on poverty to be published in many years. As the title suggests, his is a sweeping argument that covers a lot of ground. But the central concern is fairly straightforward: that in the United States exploitation of the poor population in the housing, banking, and especially labor markets is a key source of profit, and that, therefore, everyone who buys cheap goods and services, purchases real estate, or invests their savings benefits from, and incentivizes, the existence of poverty.

Desmond documents that, instead of creating a market distribution of income that eliminates poverty through high wages, the U.S. mostly tries to remedy poverty after the fact, through redistribution. He makes some highly effective rhetorical points about the problems with post-market remedies. For example, he points out that the U.S. has the second largest social safety net in the world, if we count toward the total what we spend on tax exclusions and tax expenditures—which, after all, we frame as helping a broad swath of Americans, although they are actually tilted toward the affluent. His discussion includes an excellent lay explanation of tax expenditures and their regressivity; economists, who often struggle to explain this phenomenon clearly, could take note. And he has a rousing discussion of how even funds dedicated by legislation for redistribution get redirected away from poor Americans by politicians, leveraging the story of how TANF, the block-grant replacement for the old AFDC cash welfare program, has turned into a slush fund for states, with the result that only 22% of it now goes to cash for low-income families.

Redistributive policy's failure, he argues, is clear from the fact that the U.S. poverty rate has not fallen in decades—a splashy claim that centered the book's excerpt in the *New York Times*. Here his measure is, however, mismatched to his claim, because the poverty rate he uses, the Census Bureau's Official Poverty Measure (OPM), is, independent of its other flaws, a measure only of market income that by design does not capture redistribution.[1]

Desmond believes redistributive policy fails because, even when government spending lands in the pockets of the poor, it is quickly extracted. The Earned Income Tax Credit, he worries, allows employers to lower wages. Housing vouchers push up rents, and so did, he claims, the expanded 2021 Child Tax Credit (CTC).[2] Banks charge exorbitant and unpredictable fees for overdrafts, pushing low-income clients instead toward an inferior and predictably expensive segment of the financial services market that they also fund but keep their hands clean of, namely check cashing and payday loan companies. He notes on p. 73, "If the poor had a costless way to access their own

[1] The "best" way to measure poverty is up for debate. The Census now provides a Supplemental Poverty Measure (SPM) that accounts for near-cash transfers, and a backward-looking reconstruction of that measure implies that poverty has fallen in recent decades by 40% thanks to redistribution (Wimer et al. 2016). Meyer and Sullivan (2012) and Fitzgerald and Moffitt (2022) each consider a consumption-based measure of poverty, of which Edin and Schaefer (2022) are skeptical. However, the OPM is an historical artifact--a basic food budget from 1955 multiplied by three and adjusted for inflation—and for that reason has mostly fallen out of favor among researchers.

[2] The book provides no evidence for this claim, nor am I aware of any. Given that the expansion was temporary, the claim appears implausible theoretically; given that rents were rising rapidly over this period for other reasons, it appears unverifiable empirically.

money, over a billion dollars would have remained in their pockets during the pandemic-induced recession ."[3]

He cites in some cases (such as the CTC) only anecdotes while in others he cites evidence by economists for these phenomena. But he consistently elides the difference between non-zero incidence of these programs on unintended beneficiaries and full incidence. For example, Rothstein and Zipperer (2020), whom he cites to support his unqualified claim that the EITC drives down wages, in fact write with significant qualifications:

> Although simple theories of the labor market imply that EITC expansions will lower wages, the empirical evidence is quite limited and dated. What evidence there is suggests that wage effects are important but that the EITC is nevertheless quite effective at raising the incomes of its intended beneficiaries: Even with the implied wage effects, the EITC makes recipients much better off.

It is also unclear what his concerns about incidence have to do with his critique of redistribution as an approach to anti-poverty policy. After all, if landlords raise rent in response to the CTC, why wouldn't they do so in response to an increase in tenants' wages? In fact, in a separate discussion Desmond points out that they do: Agarwal et al. (2019) find that an increased minimum wage allows landlords to raise rents (although, again, the research but not Desmond's summation focuses on the fact that the wage hike nonetheless greatly benefited renters, reducing their probability of default; research on the effects of the CTC expansion is similarly positive).

Later in the book Desmond does backtrack in his criticisms of redistributive anti-poverty policies to some extent, admitting that these programs do benefit the people who receive them and voicing support for their existence. But his language in those sections is nowhere near as vivid and memorable as his earlier excoriation. Given how close the typical American is to tipping into cynicism about the government's ability to do anything but waste money, the book's uneven tone raises the concern that it may lead voters who care about fighting poverty to underestimate the effects of eliminating programs like SNAP, the EITC, and housing subsidies—because they walk away from reading it with the misimpression that such programs don't really do that much anyway.

Understanding "exploitation" as a question of incidence

Nonetheless, Desmond's book contains important ideas worth engagement by the economics community. For one thing, what Desmond calls "the exploitation of the poor" I think can usefully be reframed for an economics audience as the following quite insightful concern: that a lack of outside options for low-income Americans in many markets means that they tend to get only a small share of the surplus from exchange and therefore a smaller-than-hoped-for incidence from transfers intended to boost their resources. While the book's concern that this is due to the structure

[3] Sometimes the book's critique is a little scattershot. For example, along with bankers and landlords, he writes with frustration about disability lawyers who take a cut of their clients' back benefits. But those payments are strictly capped (Hotfelter and Chaikin 2023), and amount to less than 0.7% of DI spending. SSDI rights lawyers earn on average $44,160 per year (Ziprecruiter.com 2024) advocating for the disabled, making them an odd villain in the fight against poverty.

of antipoverty programs is plausibly misplaced, as suggested by the inconsistency of his own examples, framing the problem this way suggests several interesting implications for economists.

First, when we evaluate the effects of a policy change (be it a change in redistributive programs, in a labor market regulation such as the minimum wage, or whatever else) on the well-being of the poor population, economists prefer, for good reasons, to focus on quasi-experimental shocks. But this approach may cause us to miss general equilibrium effects in which interventions that target poor people get only gradually captured by other side of market, especially where prices may be initially sticky. Event study approaches that keep this possibility in mind, and test for it formally where possible, could enhance our understanding of policy dynamics, the duration of policy impacts, and long-run relative to short-run incidence.

Moreover, it would be useful for economists to explore the circumstances under which poor households capture more or less surplus from policy changes. How much do rents rise in response to a minimum wage increase in times and places where housing supply is more or less constricted, or where income or racial segregation more or less reduces the number of neighborhoods in the low-income population's choice set? How much of the EITC is captured by employers in times and places where unemployment is high or low, or as monopsony power varies? How much of pandemic stimulus checks were captured by the financial industry in states with and without regulation on check cashing or usury laws?

On p. 138, where Desmond writes, "We need to ensure that aid directed at poor people stays in their pockets, instead of being captured," it sounds a lot like he is proposing that we legislate incidence. While that makes the task seem impossible, policy can in fact change outside options with the intent of changing elasticities. The implication of an economic version of Desmond's argument, then, is that anti-poverty policies should be paired with policies that affect elasticities by increasing the outside options of the targeted population. For example, housing subsidies could be expanded at the same time that supply-side policies stimulating new construction are enacted. Capping credit card late fees (as the Consumer Finance Protection Bureau recently did) might most effectively be paired with the creation of an alternative credit source, such as postal banking, so that financial institutions start working to retain their customers instead of working to come up with a replacement form of surplus extraction. Evaluations of program effects in times and places where policies were adopted in bundles could be very informative about optimal program design. Similarly, cross-national studies of the other structures in place in countries where transfers appear to be less subject to capture than in the US could generate new hypotheses about the conditions under which poor populations gain market power.

Desmond also cites his own research documenting that in many parts of the country, landlords in high-poverty areas earn higher profits than those in rich areas, because costs are much lower in poor neighborhoods but rents are only slightly lower. This results, he argues, from poor people's scant outside options for neighborhoods, due to discrimination in high-income areas. While this suggests that improving outside options through anti-discrimination enforcement would be helpful, he also documents that rents sometimes rise even where vacancies are high—which suggests another, complementary explanation: that high fixed costs of moving for poor renters and a lack of liquidity lower their ability to take advantage of outside options.[4] Testing for this possibility, for example by examining whether rent increases in high-vacancy areas were less common when pandemic stimulus payments relaxed liquidity constraints—could be informative.

One shortcoming of an economic interpretation of Desmond's exploitation narrative, however, is that it is zero-sum. It requires that, for every loss a poor person suffers, some rich person must profit. This framework, like Desmond's narrative, leaves little room for deadweight loss, but in fact anti-poverty policy is rife with it. For example, in describing the difficulty of successfully enrolling in an anti-poverty program, Desmond focuses on lawyers' fees (an empirically small issue, but one that reflects a diversion of funds from the intended beneficiaries to a higher-income group) rather than the paperwork-filling, rules-parsing, documentation-finding, and other "administrative burdens" of program enrollment that the lawyer helps reduce—a much large issue, but one of deadweight loss (Parolin et al. 2023) and therefore providing no obvious villain to blame. While mostly absent from Desmond's accounting, administrative burden appears to be quite a large part of why anti-poverty programs aren't as effective as they could be (Moynihan et al. 2015)—because they deter many eligible people from actually receiving program benefits and lead many more to receive benefits only inconsistently. To the extent that administrative burden has a nefarious cause (rather than being the result of haphazard design and neglect), research suggests that the cause is not profit but rather racism: administrative burden is created in order to separate the deserving from the undeserving, and the American frame of deservingness is highly racialized (Ray et al. 2023, Barnes 2023). But Desmond struggles similarly to have a comprehensive engagement with the issue of racism in the book, because, again, he focuses only on its (large) role in exploitation and not, unfortunately, on how much deadweight loss it creates (also large). Administrative burden is a growing area of research in social science around poverty, however, and could greatly benefit from increased engagement by economists.

Desmond's focus on who wins when poor people lose means he has plenty to say, by contrast, about landlords—but that discussion is mostly a reprise of what was included in his previous book, *Evicted* (2017). And while banks do come in for some harsh words, Desmond does not tie financial industry practices clearly to benefits for high-income Americans, as he does with how homeowners benefit from segregation or how consumers benefit from corporations' poor labor practices.[5] Instead, in terms of new villains in this book, his greatest ire is for corporations that pay poverty wages and create working conditions that immiserate employees, about which he writes compellingly.

The impoverishing labor market

The conditions Desmond eloquently describes have previously been documented in a variety of other work. Some are clearly profit-maximizing responses to weak regulation, such as wage theft

[4] A model of the landlord-tenant relationship more akin to our frameworks for employer-worker relationships, or of tenant farmers in a development context—one that takes seriously the costs of search and threat of homelessness—could be helpful here.

[5] In a full accounting of privileged Americans' gains at the expense of low-income Americans, he could have discussed, for example, the regressivity created by flat credit card swipe fees that support cards' rewards programs for the affluent (Schuh et al. 2010) and drain public coffers (Klein 2023).

(Fine et al. 2021), safety violations (Grittner and Johnson 2020), and labor-law violations (Marinescu et al 2021). Others are straightforward efforts to increase monopsony power, such as franchisee no-poaching agreements (Krueger and Ashenfelter 2022) and requirements that workers sign non-compete agreements for work that does not involve trade secrets (Johnson et al. 2023). Still other practices, such as outsourcing non-core firm tasks and hiring independent contractors instead of employees, allow firms to shift earnings and employment risk to workers, avoid within-firm wage compression pressures, and minimize obligations to provide benefits (Weil 2019; Hacker 2006). In these cases, practices that cause losses to low-wage workers provide gains to firms.

Yet other practices that make workers worse off are perplexing, and equally so whether analyzed within our standard models of profit maximization or in Desmond's zero-sum framework. Desmond doesn't question why employers treat workers badly in ways that do not increase profits, but it is a natural question for economists to ponder—and investigate. Research that aids our understanding of the motives behind these firm practices would be useful in determining whether and how to regulate them.

For example, unstable scheduling has become ubiquitous in many customer-facing occupations, including retail and hospitality; workers cannot predict from week to week what days and times they will work and get paid for (Lambert et al. 2014, Lambert et al. 2019). Schedule instability creates a great deal of harm for workers, jeopardizing their health, ability to pay their bills, and family responsibilities (Schneider and Harknett 2019, Ananat and Gassman-Pines 2021, Williams et al. 2022).

This loss to workers, however, does not appear to come at a benefit to employers: Kesavan et al. (2022) show that typical scheduling practices shift more than 100% of demand volatility onto workers, inducing variation where there need be none. In a randomized trial they conducted with a large, typical retail firm, The Gap Inc., the authors found that simple changes in scheduling practices led to stabler schedules, which reduced turnover, improved productivity, and increased sales (Williams et al 2018). While The Gap adopted the experimental practices company-wide after the intervention, other companies have not followed suit and in fact have fought the adoption of state and local regulations requiring it. This resistance remains despite the fact that in places where the practice has been regulated through "fair workweek" regulations (Seattle, WA; Emeryville, CA; and others), it has improved worker outcomes while having no effect on employment or hours (Ananat et al. 2022; Harknett et al. 2021). Research into firms' motivations in objecting to these laws, and to others such as paid family leave insurance (Bartel et al. 2023) that have been shown to benefit employees and not to harm firms, could be very helpful both in designing policy and in modeling firm behavior.

"Fair workweek" regulations typically include a provision that employers offer additional hours to existing workers before hiring new ones. In my own ongoing work, employees report that their employers are less likely to follow this requirement than other parts of the statute—including workers who are well under the hours thresholds for receiving employment benefits. Both theoretical and empirical work understanding, from the firm side, why employers would go through search and training costs to add workers when existing workers want more hours would be helpful. For example, are firms keeping a surplus of workers at low hours as a strategic reserve, in order to minimize the chances of insufficient labor supply if they need last-minute help?

Attempting to infer firm rationales for these choices, of course, assumes that the practices in question are purposeful firm policy. But may they instead be, or under certain circumstances be, instead the result of firms deprioritizing supervision of low-level managers, who may use the resulting discretion to abuse employees because they view exercising power in petty ways as one of the (few) perks of their job? This is certainly the working model in popular ethnographies of low-wage work such as Ehrenreich (2001), and it gets some validation in more scholarly work as well (Edin and Schaefer 2015, Wood 2020). For example, Edin and Schaefer tell the story of "Rae," who had been employed at Walmart for years and had earned Employee of the Month but was fired after missing one shift because a relative had left her car with no gas. (Walmart, in fact, adopted a non-discretionary points-based termination policy in 2019, suggesting that they had found such manager discretion to be inefficient.[6]) Work exploring the traits of frontline managers, like work on the psychological characteristics of those who apply for law enforcement jobs (Arrigo and Clausen 2003), could model selection into this occupation and measure the value-added of managers with different traits, which might help us better understand the frequency of and reasons behind apparent manager cruelty.[7]

Alternatively, are managers manipulating working conditions to reward favored workers and make "examples" out of errant employees in order to "divide and conquer," reducing worker solidarity? Could this also be a reason for unstable scheduling—to prevent workers from working together consistently and therefore bonding (Shepherd 2021)? Are firms attempting to prevent the development of social networks among employees in order to avoid labor organizing, even at the cost of lower worker productivity, higher turnover, and lower short-run profits? Identifying which of these or other explanations predominate in generating unpleasant working conditions in low-wage employment may require innovative data collection, perhaps along the lines of the ethnographic efforts (e.g. Orr 1996; Levy and Murnane 1996) that informed Autor, Levy, and Murnane's (2003) groundbreaking work.

Regardless of their cause, the results of these practices are the volatile earnings and physical and mental exhaustion that characterize low-income workers' lives. Mullainathan and Shafir (2013), of course, have demonstrated how such scarcity harms decisionmaking and can generate poverty traps. In a more mechanical way, low and volatile earnings can create, as Desmond documents, the trap of dependence on high-interest loans (Amorim and Schneider 2022).

[6] The points system, now popular with firms including Walmart and Amazon, is currently under fire for violating medical and family leave law and the Americans with Disabilities Act.

[7] As service work has become a larger part of the job landscape for non-college workers, some groups engage in this work at increasing rates while others, particularly non-college white men, are increasingly dropping out of the labor force rather accept this type of employment (Kahn et al. 2022). Given the subordinate behavior some managers appear to require, leveraging an Akerlof and Kranton (2010) identity-economics framework might be useful in accounting for these different reactions to the same pressures. One implication of the possibility that white men's reluctance to work these jobs is due to their sometimes humiliating nature is that improving working conditions in these jobs might improve non-college white men's employment rates, forestalling a number of negative social and political as well as economic trends.

Economists don't usually apply a poverty trap framework (Mookherjee and Ray 2002) in the U.S. context, but perhaps they should. The predictions of such a framework, in which a combination of borrowing constraints and a required threshold level of investment in earnings capacity prevents individuals from getting out of poverty, are consistent with the surprisingly positive effects of recent transfer programs on labor supply in this group. While, in canonical models, non-labor income transfers reduce labor supply (or, at most, leave it unaffected), Cook and East (2023) have recently found that quasi-random access to federal nutrition assistance leads to increased earnings among workers in the medium term. Ananat et al. (2024) find suggestive positive effects on labor supply of less-educated single mothers from the expanded 2021 CTC. Pierce et al. (2024) find that mortgage subsidies provided at the onset of unemployment lead to long-term increases in employment and earnings, while Hungerman et al. (2024) find the same for emergency cash transfers to those at risk of homelessness. Future work could helpfully identify the populations for whom and circumstances in which cash transfers in the U.S. context not only do not depress earnings but in fact lead to productive labor market investments.

Policy implications of the Desmond diagnosis

Once Desmond has fully documented the many injuries that firms exploiting the impoverished have caused, he then calls for a revitalized poor people's movement to claw back the gains made by monied interests in the last decades—a movement that will then lead, in his vision, to a number of new regulations and programs. His recommendation for bringing this movement about is for rich consumers, homeowners, and 401(k) holders to change their demand curves, individually rejecting the lowest prices and highest returns on their investments. It is a surprisingly individualistic solution—notably, he is still placing the blame for poverty on individuals, just rich ones instead of poor. And it is not clear whether his goal is to get companies to change their behavior without worker involvement, based on having lost business from Desmond's readers, or to weaken employer power through lost business enough that workers gain leverage to make demands. Either way, it is at odds with what worker organizations themselves have publicly requested (Sanchez 2021). If he wanted to focus on individual actions that readers could take immediately that worker organizations have endorsed, he might have instead encouraged readers to donate to those organizations' strike funds (which directly improves strikers' outside option), but he argues that charity, however laudable, is no substitute for the moral redemption of personal divestment.

If he wants to get rich people more directly involved—and he does encourage readers to join organizations—he could have defined the boundaries of the movement he encourages as one in which readers are not on the outside looking in. He criticizes the reader for being invested in a stock market that punishes firms for raising wages—yet he could instead have made a solidarity argument, that a more robust government that offers a stronger pension system, long-term care insurance, and cheaper college would give the reader less reason to fear dips in their investments; he could have argued that readers should join a cross-class movement against the erosion of public goods and increasing precarity of American life. In place of such an argument, the book instead commits to a narrative framework in which the reader's interests are inherently in conflict with poor Americans'.

Even within his framework, he could have advocated for some sort of coordinating mechanisms to enhance the effectiveness of the individual actions he encourages. For example, a system to identify products made with fair labor practices using a symbol—like the ones for organic or fair-trade certification—would reduce the costs to individuals of identifying ethical companies. He could have encouraged readers to put their investments in ESG funds and called for strict standards for labor practices within such funds. Economists, of course, generally try to address public goods such as poverty alleviation through government action in order to avoid free-riding, but failing that we typically try to identify coordination mechanisms for individuals.

Desmond ends the book by pointing out the recent revival of the labor movement, which he hopes will lead to an inequality-reducing redistribution of market resources. Unfortunately, the book does not include an analysis of the causes of this revival—a logical question given the context of the book's description of worker disempowerment. Fortunately, other sociologists have written recent books on the topic. Interestingly, they argue that it is not reduced demand for exploitative companies' products but instead increased demand during the pandemic, along with improved outside options for workers provided by enhanced redistributive policy, that has led to this revival.

Jamie McCallum, in *Essential: How the Pandemic Transformed the Long Fight for Worker Justice* (2022), argues that the energy to unionize workplaces like Amazon and Starbucks (the unionization fight at which has come a long way since McCallum's book was published, with Starbucks agreeing to negotiate across stores in 2024) came from the combination of untenable stress being placed on these workers during the pandemic and the worker shortage in low-wage jobs, which provided workers with new leverage. Economist Aaron Sojourner has documented the remarkably strong position of US workers since the pandemic, with quits to firings, which he calls the Labor Leverage Ratio, at a record high (Sojourner and Devito 2022) and the Black employment-to-population ratio catching up with the white ratio for the first time since measurement began (Sojourner and Wilson 2022). This labor shortage, in turn, McCallum argues, was created by the mass displacement that occurred at the beginning of the pandemic, along with declining immigration and lower labor-force participation among older workers (Jacobs and Newman 2023) that tightened the labor market, after which many workers previously employed in low-wage service jobs were able to re-match to better jobs and industries.

Alexandrea Ravenelle, in *Side Hustle Safety Net: How Vulnerable Workers Survive Precarious Times* (2023), argues that a significant driver of workers' ability to transition to these better jobs was the more generous government supports available during the pandemic. She followed a number of displaced service workers and found that those who were able to access Unemployment Insurance (UI) and its pandemic supplement in a timely manner were able to invest in themselves, getting new training or starting new businesses; these workers by and large did not return to their old work. Those who were not able to access payments,[8] by contrast, scraped by on the gig and warehouse work that expanded in the pandemic, and took their old jobs back when they could. Consistent with Ravenelle's claims, Sojourner and Wilson note that, overall, the "Great Resignation" has really been the "Great Upgrade," with workers finding better jobs rather than

[8] Ravenelle uses a convenience sample, so her work is not able to speak to what share of low-wage service workers were unable to access pandemic-era UI in a timely manner. Ananat et al. (2022) followed a representative sample of similar, UI-eligible workers in Philadelphia, however, and found that only half were able to do so, due to heavy administrative burdens in applying.

leaving the labor market. It would be useful for economists to leverage pandemic variation in UI to test whether Ravenelle's story holds at scale, i.e. whether more generous UI led recipients to better jobs and partially accounts for the Great Upgrade. If so, it would call into question decades-old economics research finding that larger UI payments do not improve eventual match quality, or perhaps call into question that research's applicability to subgroups of workers most likely to face severe credit constraints.

That a few months of enhanced UI was enough to help a sample of workers transition to better jobs is another data point consistent with credit constraints among the U.S. low-income population creating poverty traps. That the worker shortage created by some service workers moving to other industries was enough for the remaining workers to gain a better bargaining position and begin to improve their working conditions is another point in favor of Desmond's implicit argument that exploitative conditions have resulted from poor Americans' lack of choices and can be remedied by government intervention. That these new choices may have been created in part by a classic redistributive program, UI, that Desmond dismisses as a "stanchion of poverty" is another indication that redistribution itself is not the problem and instead may in fact lead to a better market distribution of outcomes.

With these possible implications to build from, we can consider what types of policy interventions would provide the variation to test them. Research leveraging quasi-experimental variation induced by existing or future versions of interventions along the lines of those described below to further explore the validity of these implications could be informative:

- Housing interventions that potentially expand renters' outside options:
    - Desmond advocates dedicated federal funding for areas that expand affordable housing or adopt inclusionary zoning.
    - Desmond advocates expanding the 502 Direct Loan homeownership program, which provides down payment assistance to low-income renters, giving them the option of owning instead.
- Labor regulations that potentially limit employer power:
    - Ravenelle proposes regulations to restrict firms' ability to rely on independent contractors, such as charging a surtax to firms that exceed a certain ratio of independent contractors to employees, or requiring reclassification if independent contractors are necessary to the core business, and creating good-cause-style rules for gig worker deactivation.
    - Paid family leave insurance and paid sick leave programs reduce firms' ability to enact harsh attendance policies, while Fair Workweek laws reduce employers' ability to control employees' time without paying for it.
    - Desmond proposes reforming the National Labor Relations Act to allow sectoral bargaining and to increase punishments for firms' violations of the Act.
    - Rothstein and Zipperer propose enacting EITC expansions and minimum wage increases together so that employers cannot capture EITC benefits through lower wages.
- Redistributive policies that potentially expand workers' outside options:

- Medicaid expansion de-links health insurance from employment, plausibly making workers more mobile and increasing their bargaining power.[9]
- Converting transfers that include work requirements, such as the current CTC, into transfers that are independent of work, such as the 2021 expanded CTC, plausibly decreases employer power.

III. Research that focuses on family: Kearney, *The Two-Parent Privilege*

Where Desmond's book is sweeping, Kearney's is quite focused. Leveraging an impressive variety of data, she thoroughly documents the disparities in outcomes between children growing up in married-parent and single-parent households.[10] Those growing up in married households consistently hold the advantage, and boys, in particular, raised by single parents grow up to have less education and worse economic prospects. Men with worse economic prospects, in turn, make less appealing marriage partners, and so Kearney argues that this creates a vicious circle: worsening economic prospects for less educated men leads to declining marriage, which leads to more boys raised by single parents, which renews the cycle of poverty.

The approach Kearney recommends to address U.S. poverty—that parents make the best of whatever their current economic situation by marrying and raising children together rather than maintaining separate households—is appealingly simple, deploying as it does the most classic of economic concepts: economies of scale and specialization. When parents marry, she points out, children nearly always have access to, first, more disposable income thanks to household economies of scale, even when each parent's initial resources are meager; this argument echoes Thomas and Sawhill's (2002) microsimulation paper, in which single moms whom the authors "matched" in the CPS to demographically similar single men were lifted out of poverty two-thirds of the time by their randomly-assigned husband's earnings. Second, she argues that children access more parental time and energy thanks to returns to specialization in two-parent households, as each parent takes on household tasks according to their comparative advantage and develops complementary skills, so that the whole family gains from higher household productivity. All else equal, then, based on these premises children will do better within two-parent households, just as the correlational research shows that in fact they do: they get more education, earn more, have less criminal justice involvement, and are more likely to have marital births themselves. The volume of research she cites is enormous; it is also, however, almost entirely correlational, by the standards of economics.[11]

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[9] Medicaid expansion somehow comes under fire by Desmond as subsidizing low-wage jobs by letting employers off the hook for providing health care. But rising employer-provided health care costs contribute to the wage stagnation he decries, and having insurance even if you lose your job expands your outside options.

[10] Kearney makes clear that she believes her argument for marriage holds equally for same- and different-sex couples; nonetheless, the extant research and therefore the evidence she marshals are heavily concentrated on heterosexual couples. Many critics' concerns about her argument also stem from difficulties they identify in the straight marriage market. Evidence from Hancock, Lafortune, and Low (2024) further supports the claim that straight marriage involves a unique set of challenges. Given the mode's numerical superiority and its differences from other marriage markets, this review will also focus on straight marriage.

[11] The book cites two studies considered causal by economic standards. One, Gruber (1994), shows negative effects on child outcomes from easier divorce, but gets a rather strong counterweight in Stevenson and Wolfers (2006), as Kearney discusses. The other, Ananat and Michaels (2008), the book mischaracterizes (p. 54) by citing our finding of

Kearney argues that despite the lack of causal research, the sheer volume of correlational research is so large that the relationship must be causal (p. 56 and elsewhere). This claim is inconsistent with the principles of modern applied microeconomics. More importantly, it is also a claim that, if provisionally accepted, immediately raises a very interesting economic question: *why* don't parents whose children would be better off if they got married get married? What is the underlying theory for such big bills being left on the sidewalk?

Norms-based explanations for declining parental marriage

The book doesn't engage explicitly with this question, however, so the reader is left to infer Kearney's implicit answer from her other arguments. In particular, her belief that a large-scale advertising campaign promoting marriage could help reverse the downward trend in the share of births to married parents suggests that she thinks people make their family formation decisions relatively casually, based on fluctuations in norms that tell them whether marriage is worthwhile, rather than based on careful optimization around one of the most significant choices in their and their children's lives. If these norms have changed, and have steered them causally into sub-optimal choices, then an education campaign to get Americans to re-embrace marriage norms can set things right.

Kearney's belief that an education campaign can turn things around stems, as she explains, from her finding with Levine (2015) that MTV's *16 and Pregnant* led to lower birth rates among teens. Leaving aside critiques of that paper's identifying assumptions (Jaeger, Joyce, and Kaestner 2016), a provocative conceptual critique (Cohen 2024) of the move to generalize to marriage from teen childbearing focuses on the fact that the effect Kearney and Levine found amplified a decades-long decline in early childbearing in both the US and other rich countries, rather than *reversing* a cross-country long-term trend—as a marriage campaign would have to. Cohen argues that, in fact, every documented causal popular media effect on societal trends has been an acceleration toward modern norms, never back toward tradition. He, like Kearney, cites telenovela storyline effects on birthrates and divorce (La Ferrara, Chong, and Duryea 2012), while he also adds internet access effects on Chinese age at marriage (Zhang et al. 2023), and movie theater and bus stop effects on Nepalese age at marriage (Yabiku 2004) to the list of modernizing amplifications. He argues that the one-way nature of these impacts is no coincidence: because it is inherently modern, pop culture messaging cannot drive reversion to tradition, an argument that echoes foundational media theory (i.e., "the medium is the message," McLuhan, 1964).

Consistent with this hypothesis, the media campaigns that were attempted in many of the federal government's Health Marriage Initiative sites failed to increase marriage, as Kearney acknowledges. Cohen's argument that Kearney's proposed remedy for falling marriage can't work is empirically testable, however, both abroad and in the US. For example, the US has a

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a fatter lower tail of the divorced than married income distribution but omitting mention of the fatter upper tail, so that we in fact find zero average causal effect of divorce on the resources of women with children.
McClanahan et al. (2012) review studies that attempt to identify causal effects of single parenthood. With the exception of Gruber (1994), these studies use propensity scores, sibling fixed effects, lagged dependent variables, or other approaches generally considered correlational within economics.

very popular marriage-promoting network, the Hallmark Channel, along with shows such as *19 Kids and Counting* (which has been consistently much more popular (Google Trends 2024) than *16 and Pregnant*). Kearney and Levine's strategy could be productively deployed to test the effects of either type of programming on local fertility and marriage.

Another challenge to Kearney's diagnosis that norms are the problem is that survey data suggest that Americans' embrace of marriage norms is remarkably strong and steady. Among adult Americans who haven't been married, 81% say they hope to—a share that has actually risen slightly over the last 10 years (Gallup 2020). Kearney cites different data from the same survey, on the share of Americans who report that it is very important to them (the respondent) that a hypothetical couple who has a child legally marry. But the Gallup authors caution against interpreting this question in the way Kearney does, noting, "While people may think it is less important for couples to marry regardless of the situation, it is still a choice most people make, or hope to make, for themselves…Thus, their evolving attitudes about marriage may reflect *increasing acceptance for how others lead their lives* rather than a profound shift in their own lifestyle preferences [emphasis added]." If this is what Kearney hopes to roll back, then it will involve not trying to get Americans to value marriage again but rather trying to get them to stigmatize unmarried families again—a notably different goal.

Moreover, the group Kearney is most concerned about due to their low and falling marriage rate and substantially higher risk of poverty, namely less-educated Americans, are significantly *more* likely than highly-educated Americans to endorse her preferred measure of pro-marriage norms. Non-white Americans, who have lower marriage rates than white Americans and about whom she also notes concern, also endorse the measure at higher rates. Similarly, the well-known Future of Families (formerly Fragile Families) study, which focused on the group for which Kearney is most concerned, found that most unmarried parents report at their child's birth that they want to marry each other and think they will, but that most don't end up achieving this goal (McLanahan, 2011)—suggesting that despite pro-marriage norms, something else is getting in the way.

Inconsistency with survey reports is not, however, the main reason an economist would typically find implausible a claim that millions of people are gravely erring in their biggest life decisions. Rather, skepticism would arise because our entire discipline is built on the idea that people generally try their best to optimize their own and their children's well-being. Yes, people can and do of course make mistakes, and norms might nudge marginal families one way or the other. But can a large and growing share of Americans really be systematically making an individual choice that is as contrary to their own stated goals and as harmful to their children as a causal interpretation of the correlations suggests? Or, instead, is a simple model of marriage as vehicle for childrearing perhaps leaving out something that makes these missing marriages, particularly for the less educated, less of a home run than they appear?

Kearney doesn't engage with marriage as a relationship with its own dynamics, taking the view that such issues are beyond the purview of economics (p. 43). But economists in fact left behind the unitary household model she implicitly draws upon some years ago, and engaging seriously with her argument—as I very much encourage readers to do—requires that, when we model the family as a child-producing firm, we consider its internal economic logic.

The changing economics of marriage

In particular, the specialization Kearney describes as benefiting children requires trade if it is to be more efficient than autarky, and gains from trade evaporate in the face of high negotiation costs. That negotiation costs are high enough between most pairs of individuals—strangers or even friends—to negate the ostensible gains from co-residence is made clear by the fact that living with roommates is a life stage most people try to move through as quickly as possible.

Roommate situations are hard because they are matches of putative equals that generate both a lot of shared surplus and a lot of negative externalities, without clearly assigned property rights. Negotiating over the division of that surplus is a complex and costly ad-hoc task that involves establishing Nash bargaining coefficients and then applying them to countless components of the household production bundle, while addressing externalities requires coming up with equally countless household rules to approximate Coase. This work frequently swamps the gains from economies of scale and household specialization.

That was not the case for traditional marriage with children. Mothers' historically poor outside options, and the difficulty of obtaining a divorce in many communities, reduced negotiation over the division of surplus in many marriages to something close to a simple dictator game with a variably-altruistic dictator. Meanwhile, tightly prescribed gender roles allowed couples to skip most negotiation about who did what tasks and which externalities each would bear. In other words, to participate in a traditional marriage was to work a routine job (Autor, Levy, and Murnane 2003) paid with a mostly-set portion of the firm's proceeds—the kind of middle-skill job most people could succeed at. These traditional marriages date much more recently than one might think: Kearney cites marriage in 1980 as the baseline to which she compares today's less productive family arrangements. The average married mother of minor children in 1980 had wed by 1970, a year in which she didn't have access to legal abortion, clear access to birth control in the absence of a husband, access to credit in the absence of a husband or father's support, or legal protection in the labor market from sexual harassment.

Thanks to a host of legal reforms and a shrinking gender-wage gap, women have much greater outside options and thus greater, but likely more variable, bargaining power in marriage today. This would suggest that marriages in the US today may less closely follow the historic, institutionalized model than the ad-hoc roommate model. Notably, such an arrangement requires a lot more skill and effort to successfully negotiate.

That offers a partial, testable explanation for the growing education gap in marriage Kearney documents: that those with less education are, on average, less well-equipped to succeed in the non-routine cognitive tasks of negotiating a modern marriage, because college both selects for and teaches communication skills and tolerance of ambiguity, each of which lowers negotiation costs. Tolerance of ambiguity may also reduce the psychological burden of participating in a relationship that, in order to generate and divide surplus in such a way as to keep both partners participating at a time of rising women's bargaining power, has to deviate from traditional gender roles. An investigation of the causal effects of education on marriage over time, leveraging experimental or quasi-experimental variation in education as well as women's outside options, could be informative here.

Men on average have fewer soft skills than women, which may help explain why women more often file for divorce (Rosenfeld 2018), and why Kearney's popular press critics have often offered, as an explanation for the decline in marriage, the argument that "a good man is increasingly hard to find." As Anna Louie Sussman wrote in a November 25, 2023, op-ed in the *New York Times*, quoting gender researcher Daniel Cox of the American Enterprise Institute, "Navigating interpersonal relationships in a time of evolving gender norms and expectations 'requires a level of emotional sensitivity that I think some men probably just lack, or they don't have the experience.'" Or, as Christine Emba, writing on July 10, 2023 in the *Washington Post*, describes the increasing skill requirements of modern marriage: "Guys should learn to be more sensitive, quiet and socially apt, seemingly overnight. It's the equivalent of "learn to code!" as a solution for those struggling to adjust to a new economy."

That men and women are frequently unable to renegotiate traditional gender roles with each other, and therefore leave significant marital surplus on the table, is made clear in Hancock, Lafortune, and Low (2024). They show that men's and women's relative labor supply and home production fail to adjust to relative wages in ways that would make marriages with relatively low-earning husbands efficient, and argue this stickiness helps explain high divorce rates among women who outearn their husbands. Using variation by country of origin for immigrants, the authors show that ethnic groups in which men traditionally engage in more household production in the sending country are better able to respond to U.S. variation in relative wages by gender, resulting in higher in-group marriage rates among these ethnicities. These findings suggest that one way to increase marriage may in fact be by changing a norm—namely the norm against men's housework. Kearney (p. 84) asserts that changing norms around housework is less likely than changing norms around the desirability of marriage. But Hara and Rodriguez-Planas (2023) find that requiring high school home economics classes for boys in Japan increased household equity in adulthood, suggesting these norms may be malleable. Future research along these lines could help us understand the relative prospects for changing different norms.

Taking seriously the possibility that marriage has become more soft-skill-intensive, meanwhile, may help researchers gain traction in explaining several phenomena that Kearney attributes to the putative decline in marriage norms. First is why the marital birth share has fallen not only for less-educated men, whose wages have fallen, but also (albeit less dramatically) for college-educated men, despite their rising earnings: can individual or occupation-level variation in communication skills help account for differences in college men's marriage rates? Second is her finding that when the fracking boom recreated high-wage jobs for non-college men, the marital birth share didn't rebound. While a high-wage job might in the past have made a given non-college man an attractive hire for the routine job of a traditional husband, would him getting that same job today be enough to convince a potential mate that he could successfully handle the non-routine cognitive labor of a modern marriage?

Acknowledging the increased challenges of sustaining a marriage in the modern economy might also help explain one fact Kearney reports and another notable one she does not: first, that the United States is unique among rich countries in its high share of single parenthood (OECD 2020); second, the unusually high likelihood that, in the U.S., a child born to a married couple will see his parents break up before he turns 18 (Wilcox and DeRose, 2017). The child of a married, high-education US couple, in fact, faces nearly the same chance of family breakup as the child of an unmarried, low-education Norwegian couple—both figures are around one in five. By contrast, fewer than half that share of children born to married, high-education Norwegian parents experience breakup. While Kearney makes the important point that a comparison of our rate with countries that are much more generous toward families with kids shows that our social safety net's existence is not the cause of our high rate of single parenthood (Brady and Burroway 2012), the book does not take the logical next step in thought experimentation. Namely, do these comparative statics suggest testing the hypothesis that other countries' more generous safety nets may lead them to *better* sustain stable two-parent households?

After all, the US has the highest rate of poverty among families with children of our peer countries, and we know that lower resources drive a cognitive burden from scarcity (Mullainathan and Shafir 2013), which reduces the cognitive bandwidth available to engage in productive negotiation. As discussed above, the structure of the US labor market leads to much greater fluctuations in earnings, while the structure of our safety net leads to larger deviations in post-transfer income when earnings fluctuate (Ananat, Gassman-Pines, and Truskinovsky 2019). Instability is stressful and therefore cognitive-bandwidth-reducing in itself. Moreover, frequent changes in what each partner brings from the labor market to the household implies a frequent need for re-negotiation of roles and surplus-sharing, putting further pressure on couples' negotiation skills (and making a short-term high-wage fracking job less marriage-enhancing than it might at first appear). And unbuffered shocks to a man's economic contributions—which have been more common as our economy has shifted—require more gender-role renegotiation in order to maintain surplus than shocks to a woman's.

In high-education families in the US, but to a much greater extent in all families regardless of education in other rich countries, a devastating loss of one partner's economic contributions through unemployment or disability is much less likely than for a non-college US family. It is a safer bet for a US potential mother considering the net present value of a marriage that a college-educated man will maintain his health insurance and earning prospects than that a non-college man will. But in other rich countries—thanks to more generous unemployment and disability insurance, along with family-finance-stabilizing child allowances and universal health insurance—even if a man does experience a health or employment shock, he is unlikely to create a net economic drag on a household. This in turn means he has to contribute less non-economically in order to be of net benefit to the household, and the couple has to engage in less renegotiation to maintain shared surplus from his presence in the event of a shock.

The combination of lower scarcity, lower instability, and the improbability of an individual becoming a family financial drain may help explain why, even though they don't get formally married that often, European parents more often than American ones raise children in long-term, stable cohabiting relationships. It's also worth noting that US military families—which, as sociologist Brad Wilcox (2024) points out in promoting his new marriage-promotion book, have sustained high marriage rates and low education-marriage gaps—experience guaranteed affordable health care, housing, and child care, along with disability protections, that much more closely resemble Europe than the rest of the US.

This is pure conjecture, but it does fit the relative rates of family breakup by education and nationality better than a comparison of groups' marriage-norm endorsements does. Additional research on how family support policies in the wake of economic shocks affect mental health, relationship quality, negotiation costs, and breakup across education levels could greatly enrich economists' understanding of the economics of marriage and the policies that could promote Kearney's goal of stable family lives for children.

College-educated Americans also face at least two more advantages over their less-educated, less-marrying counterparts that Kearney doesn't cover. First, the absolute income surplus generated by combining households among the college-educated is often great enough to simply outsource high-tension tasks like toilet cleaning, meal prep, and routine childcare (Cortes and Tessada 2011), eliding some of the negotiation over gender roles that might otherwise arise.[12] Lafortune and Low (2023), second, argue that legal marriage, in particular, better facilitates household specialization within high-education than low-education families thanks to the accessibility to college graduates of both homeownership and jobs with convex returns to hours. That is, the ability to securitize marital assets through home ownership, to which both partners can lay claim in the event of breakup, is of benefit only for couples that can afford to purchase a home. Securitization, in turn, lowers the risk to a partner who specializes in home production, which allows the other partner the ability to invest in a high-earning job with convex returns to hours—but only for those with the skills to access such jobs. Lafortune and Low provide empirical evidence for the hypothesis that education-marriage gaps result partly from legal marriage's unique value to high-education couples in the modern economy by using local variation in home prices. Further quasi-experimental tests of local economies' contribution to the education-marriage gap (which have mostly focused on Wilson's (1987) marriageable male hypotheses and have mostly focused on the US) could use variation induced by home prices or the cost of outsourced home production to advance our understanding of the education gradient in the returns to modern marriage.

While Kearney acknowledges the education gradient in the value of marriage—focusing on college graduates' greater earnings—she doubts that the missing marriages among less-educated parents would have, in the counterfactual, had negative net returns. She writes on p. 172, "Could it really be the case that so many children have fathers who would not be positive contributors to the family if they were part of their household? If that is even close to the reality for men in America today, then we really do have a terrible crisis." She then, however, sets aside this possibility of crisis and returns to her thesis, that the actual explanation is that parents seem erroneously "to believe that the options of cohabitating or living apart are more or less equally good." Her dismissal appears to result from a belief that negative non-economic contributions to a household are "rare or extenuating circumstances" that are too infrequent to drive the macro pattern that 37% of U.S. children lack two married parents at home (p. 172).

But consider: 1 in 6 Americans abuse drugs or alcohol (US Department of Health and Human Services 2023); 1 in 10 Americans have untreated mental illness (Reinert et al. 2021); 1 in 11 Americans have a personality disorder (Lenzenweger et al. 2007); 1 in 9 men report being addicted to pornography (Grubbs et al. 2019); more than 1 in 5 men cheat on their spouse (Djamba and

[12] In other rich countries, meanwhile, subsidized childcare reduces at least one source of negotiation over gendered tasks.

Kimuna 2020). These are not rare problems. Yet they are the kinds of problems that many prospective parents who now have the option of not marrying might reasonably decide would mean their kid is better off with a single parent than with a second parent who brings such an issue into the house—even one who also raised family income.

Kearney clarifies repeatedly that she is not supporting abusive marriages (p. 68 and elsewhere), but again seems to think this exception numerically small enough to be irrelevant to understanding marriage patterns. Yet 7% of women experience sexual or physical violence or stalking by an intimate partner each year, and another 7% experience psychological aggression, i.e. emotional abuse (Leemis et al. 2022). Eight percent of men think it's okay to share intimate pictures of a partner online without permission, and 12% of men think a woman is obliged to have sex with her partner even if she doesn't want to—with even higher rates among younger men (Ipsos 2022). Again, these are not rare problems.

While there are no comprehensive data to identify what share of people have at least one of these "deal-breaker" traits of violence, infidelity, addiction, or untreated mental illness, given the rates of each it is not implausible that the proportion with at least one is large enough to generate a negative average return to marriage among missing marriages. Along these lines, Stevenson and Wolfers (2006) show, as Kearney acknowledges, that no-fault divorce reduced femicide and women's suicide. And, over the same period that single parenthood rose so dramatically—between 1980 and 2010—child abuse fell dramatically (Wexler, 2021). Improved data collection and further research on this subject that follows Sawhill and Thomas (2003) but examines the share of simulated matches affected by dealbreaker behaviors in addition to simulating matches' income could be highly informative about this counterfactual.

Policy implications of Kearney's diagnosis

It is worth considering, if in fact many parents are choosing not to marry each other because at least one partner anticipates negative returns, what a prescription of strengthening marriage norms would accomplish. Here again international comparisons can be informative, because there is a set of societies in which women's education and labor market opportunities have greatly increased but norms against nonmarital childbearing have held strong. What we observe in Asian countries such as China, Japan, and South Korea is that the same pressures on the marriage contract have occurred—as women have stronger outside options, they become unwilling to marry men who have severe behavior problems or who refuse to reconsider gender roles in order to share with their wife more of the surplus from the marriage (Sussman, forthcoming). The difference is that in these countries, due to the continuing stigma against nonmarital fertility, women who reject marriage also, then, do not have children. The result is a collapsing birthrate: South Korean women now bear on average just over half a child; the U.N. predicts that China's population may fall by as much as two-thirds from its peak by 2100, and in Japan communities are disappearing. The Asian example suggests that, in a society where women's economic progress puts the terms of marriage in flux, an insistence on marital childbearing may not move us back toward a world flush with married parents but rather toward a future of few parents at all.[13] The progressive European

[13]Kearney lauds Asian-Americans for having the highest percent of children with two married parents of any U.S. racial group, hypothesizing (p. 82) a "social or cultural explanation." The book does not note the flip side of this statistic, which is that Asian-Americans have the lowest birth rates of any U.S. racial group, at 1.39 births per

example suggests, by contrast, that shoring up economic support to reduce strain and conflict on couples who are trying to maintain their relationships might lead to children being raised stably by two parents.

Alternatively, we could take seriously the need Kearney emphasizes to have multiple adults' worth of time and money for childrearing, but consider whether those resources need to come from two parents versus combinations of adults whose relationship patterns aren't in such radical flux. Work by Cross (2020) finds that the two-parent family structure is not as predictive of outcomes for Black children as for white children; Black children's outcomes may depend less on marriage because of a stronger norm of extended-family involvement in childrearing within communities of color. Further, Kearney notes, children who live with a single mom and grandparent(s)—who after all also have multiple adults in the home, with the attendant economies of scale and returns to specialization—get just as much adult time and attention as kids of married parents. Additional work on the outcomes of children in multigenerational households could provide new perspective on the childrearing production function.

We could also consider whether the time and attention children require needs to be supplied at home. Hilger (2023), in *The Parent Trap: How to Stop Overloading Parents and Fix Our Inequality Crisis*, is, like Kearney, concerned by the differences in outcomes between children of highly educated and less educated parents, but focuses on understanding the specific production processes that build skills in children and who can best provide them. Rather than demanding that less-educated parents act more like educated ones so as to produce more skilled children and reduce intergenerational poverty and inequality, he argues that this demand is what generates that poverty and inequality. After assessing the evidence on decades of parenting interventions, he posits that the level of resources needed for children to flourish in the modern economy simply cannot be provided directly by parents (whether one or two) except among the most highly-resourced (and even for them it is exhausting). Expecting parents to do so, he concludes, guarantees that most children won't get what they need. He proposes that the government should instead be getting children access to more specialized adult time—through provision of child care, tutoring, school psychologists, camps, and other interventions that have been shown to improve children's outcomes and more than pay for themselves—and interprets the existing evidence to suggest that focusing on parent marriage instead is just another way to set parents up for overwork and failure.

While Kearney is much more optimistic than Hilger about the rewards of increasing marriage, she is very pessimistic about the prospects of actually doing it. In addition to citing her own work with Wilson (2018) finding that fracking-induced positive shocks to non-college men's earnings didn't increase the marital birth share, she also cites the failure of the Building Strong Families experiment. That program randomly assigned counseling to low-income unmarried couples with an infant or a current pregnancy. It led to *fewer* marriages (Wood et al. 2014) in the treatment than in the control group, due to men's "highly conflicted or disengaged relationships with their children's mothers" (Kearney p.143), which the program helped the couples realize would not lead to a good marriage but was unable to help them improve.

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woman (Osterman et al. 2021). In general, the US pattern, as with the emerging pattern in countries with high female education and labor market opportunity overall, is that the lower the nonmarital birth share in a demographic group, the lower the overall birthrate.

Given these discouraging results, Kearney proposes a well-thought-out set of increased supports for both one- and two-parent families as an alternative to increasing marriage that delivers benefits more directly to children (coalescing in the end around many of the same programs that Hilger and Desmond support). But these conclusions are all in the text of the book, not the cover art, blurbs, book flap, or media coverage. The presentation of the book can feel disconcerting, with the body full of caveats, acknowledgements of complexity, and alternative approaches to fighting poverty but the marketing selling a simple narrative that arguably belies that substance. Since, in the modern version of judging a book by its cover, many more people read posts and listen to podcasts than peruse the actual text, casual consumers may walk away with a misapprehension of Kearney's nuanced conclusions.

To do justice to that nuance, I want to take the opportunity here to highlight policies that warrant further research attention as part of serious engagement with the possible causes of declining marriage and the efficacy of potential remedies. Where such policies have been or might in the future be implemented, researchers could constructively leverage randomized experiments or quasi-experimental variation to get at more causal rather than correlational relationships; such research, in turn, could greatly advance our understanding of the causes and effects of marriage. Policies that would be informative to evaluate include:

- Those that potentially reduce sources of conflict within parent relationships:
    - Much of US safety-net policy provides resources to single parents only when they legally pursue the noncustodial parent for child support (in much of Europe, by contrast, children get many resources from the state unconditionally). Research can investigate whether reforms that treat noncustodial parents as parents rather than wallets improve coparenting relationships such as those described in the Building Strong Families study.
    - Research can investigate whether state reforms that make 50/50 custody arrangements the default in the absence of "dealbreaker" behavior, and/or allow noncustodial parents to provide child care instead of pay to meet their parental obligations, reduce conflict-reinforcing "gatekeeping" behavior by custodial parents and increase children's access to both parents outside the marital context.
    - Research can investigate whether the level of economic support provided to families, as discussed above, affect parent conflict. Can existing variation in economic supports within the US provide evidence on how the design of support affects relationship quality or duration?
    - Some existing programs have been shown to increase men's college graduation rates. [14] Research can investigate whether such interventions also increase men's earnings, and if so whether they in turn decrease conflict (Hancock, Lafortune, and Low 2023) by making marriages efficient without the need for renegotiation.
- Those that potentially improve relationship skills:

[14] For example, the City University of New York's randomly assigned Accelerate, Complete, Engage program, a gender-neutral program, raised men's bachelor's degree attainment by 15.7 percentage points and women's by 9.8 points, increasing college graduation and narrowing the college gender gap (Scuello and Strumbos 2024).

- Male teachers may provide at-scale role models for boys to develop soft skills. Compelling research has shown beneficial effects of same-race teachers (Gershenson et al. 2022) and of teachers who have high value-add for noncognitive skills (Jackson 2018). Estimates of the effects of male teachers on marriage, specifically for boys and particularly for boys without resident fathers, could be informative.
- Richard Reeves (2022) has proposed scholarships for men to study for careers in healthcare and education in order to destigmatize those growing fields for as sources of male employment—but, given that those fields require and teach soft skills, it would be interesting to see how any such scholarships (presumably funded by a private entity) affect marriage relative to other training that had an equal effect on earnings.
- A variety of existing policies have been shown to increase men's and women's college graduation rates. It would be informative to test whether such policies also improve communication skills and tolerance of ambiguity, and if so whether they facilitate negotiation and increase relationship stability.

A few caveats are called for here. First, Kearney describes (p. 138) a vicious cycle in which harsh parenting of some men toward their sons leads boys to grow up to be inadequate partners. This cycle can clearly not be interrupted in the current generation by encouraging marriage to these inadequate partners; rather, Kearney's diagnosis implies that the cycle would have to be interrupted by improving current child environments other than through marriage—e.g. through increased family supports for single parents and educational policies to build soft skills, so that current boys grow up to be adequate partners. The clear but unstated implication of her argument is that policies directly encouraging marriage now, given the shortcomings of the current pool in generating marriages with surplus, would in many cases be counterproductive under Kearney's assessment.

Second, although Kearney never recommends it, the book also does not reject the popular idea of subsidizing marriage, for example through the tax code. This omission stands in contrast to her clear rejection of other proposals she considers harmful, including homophobic policies and policies that withhold support from single-parent families. But, given that Kearney explains that at the moment we have no idea how to increase marriage, subsidies for marriage are likely to be almost entirely inframarginal transfers. While economists often think of inframarginal subsidies as wasteful but otherwise harmless, note that subsidizing an inelastic trait associated with high income *increases* inequality in children's living circumstances, and therefore might worsen the very problem such a strategy was intended to solve. In fact, Cross (forthcoming) suggests that many of the benefits associated with two-parent families among white Americans (recall the weaker association for Black families) are actually driven by the policy preferences shown to these families, which are delivered in ways that disproportionately exclude non-white populations from benefiting. Tax law professor Dorothy Brown, meanwhile, argues (2021, *The Whiteness of Wealth*) that the U.S. proclivity toward using economic policy to incentivize or reward socially desired behaviors contributes significantly to the racial wealth gap. The possibility that our society's favoritism toward families with behaviors—such as holding a large mortgage—mostly attainable only by families that are white, highly-educated, and married may in fact drive the success of such families' children is worthy of further exploration by economists.

IV. Research that focuses on the political process

Desmond concludes his book by exhorting the reader to demand a new set of policies to fight poverty. Kearney similarly encourages readers to support a substantial list of programs to increase resources for families. It is common in books about poverty for authors to commend to their readership a number of evidence-based anti-poverty policies (and of course I have speculated about potential policies implied by their diagnoses here), presumably with the belief that increasing awareness of these demonstrated high-return policies will enhance their likelihood of adoption. The challenge facing this well-intentioned strategy is that these policies are generally already quite popular with voters (Leisman 2019). That they have few political prospects is thanks not to a lack of knowledge or popular support but to the empirical regularity that, over a wide range, there is zero correlation between public opinion and policy adoption in the US (Gilens and Page, 2014).

Neither of these books, however, engage in why that is, or consider the possibility that political inequality could itself be the taproot of US poverty—that is, that we are uniquely unable to adopt the kinds of anti-poverty policies common in other rich democracies specifically because of political pathologies peculiar to the U.S. To understand this possibility, it is helpful to turn to books that explore the challenges in enacting anti-poverty policy in the emergently dominant scholarly framework for studying US politics, known as policy feedback. The implications of this framework have been little explored in economics, and the field is ripe for new contributions.

McGhee (2022, *The Sum of Us: What Racism Costs Everyone and How We Can Prosper Together)* summarizes political science research showing that racialized frameworks of "deservingness" among voters are easily activated by politicians who oppose redistribution, and that these frameworks, when activated, cause (mostly in experiments, but also in a handful of quasi-experiments) voters to oppose policies they otherwise support—including the popular anti-poverty policies Desmond and Kearney recommend. These political manipulations, which build on zero-sum narratives of economic well-being held by many white Americans, may help prevent state (e.g. Medicaid expansion) and federal (e.g. paid family leave insurance) policy adoption that would economically benefit the majority of Americans of all racial groups. Economists could usefully become active participants in this literature, identifying a richer set of real-world quasi-experimental events to exploit, applying modern econometric methods to unpack the phenomenon, and exploring its effects and limits.

At the local level, books by sociologists Schaefer, Edin, and Nelson (2023, *The Injustice of Place: Uncovering the Legacy of Poverty in America*) and political scientist Jamila Michener (2018, *Fragmented Democracy: Medicaid, Federalism, and Unequal Politics)*, build on the field of rural sociology developed a generation ago by Cynthia Duncan (1999) to focus on how, in many parts of the country, politics and government administration (including the administration of anti-poverty policies) at the local level function both to preserve race and class hierarchy and to prevent low-income people and Black people from translating their political preferences into policy. So far, this work has relied on descriptive statistics combined with qualitative interviews, leaving any number of causal claims about the factors affecting local political processes and outcomes to be helpfully assessed using economists' approaches to causal inference.

Both of these processes may help explain why popular, evidence-based anti-poverty policies do not get enacted. They may also help explain why the policies we do have fall into one of two categories both decried as ultimately ineffective by Desmond. The first is those that focus on "deservingness" of the poor and therefore have high administrative burden and high marginal tax rates to prevent "cheaters" from getting them; these programs protect the existing social order but can lead to poverty traps. The second are those that provide allegedly universal support but do so through the tax system—because taxpayers are "deserving"—with the result that most of the benefits go to the well-off. This taxonomy of enactable programs, in turn, could explain why we overwhelmingly do not enact the types of policies the writers discussed here propose.

Hilger, taking a novel tack, draws on the policy feedback literature in the last chapter of his book; a discussion comes after his wish list of policy proposals but before his final exhortation to the reader. Specifically, he warns readers not to fall for the "myths" of unaffordability, zero-sum social gains, or government failure that policy feedback scholars have identified, and he briefly but clearly summarizes the explicitly racist history driving them. It is an interesting approach, combining the two heretofore separate types of social science evidence on the causes of U.S. poverty into a single narrative. I hope to see more endeavors toward such an unblinkered economics in the future.

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