## Summary of Research Areas and Papers Discussed

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| **Area 2) Behavioral Market Design** |

| **Area 3) Economic Applications of Happiness Data** |
| Can Marginal Rates of Substitution be Inferred from Happiness Data? Evidence from Residency Choices | Daniel Benjamin, Ori Heffetz, and Miles Kimball | American Economic Review (2014) |
Overview

I conduct research in behavioral economics. Across several research lines—spanning public finance, market design, and basic research on economic applications of happiness data—my work is focused on improving the integration of suboptimal behavior into economic welfare analysis.

The classic economic approach to welfare and policy analysis models humans as idealized decision-makers. While this assumption is at times motivated by ideology, more often it is better understood as a simplification that was necessary to make progress on difficult questions. Economists are in the business of evaluating policies, and in order to forecast the consequences of a policy change the analyst must have a tractable and predictive model of behavior in hand. Assuming that humans are very smart yields a model that is reasonably predictive and exceptionally tractable—as a profession, we’ve made much progress on understanding the properties of markets populated by people like these. Given the advances of behavioral economics, one might conclude that analyses can be made more predictive by accommodating people’s mistakes. But because mistakes can be so varied, and can arise from so many disparate models of imperfect optimization, accounting for these behaviors introduces a host of new problems for tractable welfare analysis. Their addition necessarily adds parameters of interest to our models, and identifying the welfare-relevant model parameters in limited field data is often daunting.

My work aims to make progress in this domain. The approaches I use to do so take two forms. First, and most commonly, I use lab-in-field approaches to generate tests that precisely identify the welfare-relevant model components in manners impossible without experimental elicitation. Second, and often concurrently, I use theory-driven approaches to determine the measurable “behavioral sufficient statistics” that must be captured to understand a psychology’s welfare consequences. Using these approaches, I have made progress in understanding topics such as the welfare costs of misreacting to taxes or misunderstanding matching mechanisms. Across these settings, my work often highlights a common theme that correcting behavioral biases has welfare consequences that accrue not only to the individual (the typical focus of analysis of “nudges”), but that also affect broader social welfare through biases’ interaction with the distributive goals of the market.

In the sections that follow, I describe my work in these areas in depth.

Research in Behavioral Public Finance

My main line of recent research aims to incorporate ideas from judgment and decision making into serious and quantitative analysis of tax policy. This is a promising setting for the development of behavioral welfare economics for two reasons. First, it is a setting where clearly important financial decisions are made based on limited understanding of the incentives in place. The U.S. tax code is tremendously complex, and full understanding of it is plausibly outside of the abilities of most citizens. Such an environment is a natural setting to expect heuristics and biases to take hold, and to potentially be of importance to final market outcomes. Second, this is a setting with a long history of serious, quantitative welfare analysis, providing me with well-developed and well-vetted baseline models to
adopt and modify when incorporating new psychological ideas. Contrasting my findings against existing results provides a clear indication of when incorporating psychology is essential for policy analysis (and when it is not).

To illustrate this type of approach, I summarize my results from the three major papers I have written on the topic, and then briefly describe my ongoing research in this area.

My paper “Measuring Schmeduling” (joint with Dmitry Taubinsky) was motivated by a desire to accommodate poor understanding of the tax code in models assessing the social welfare consequences of a given tax schedule. Tax policy is often analyzed through the lens of a social planner who values redistribution, but who also understands that taxing income in order to redistribute distorts labor supply decisions. In such models, this distortion occurs by workers making tradeoffs between consumption (which they like) and the work they must do to fund that consumption (which they don’t like). These models typically impose the assumption that workers make this tradeoff fully optimally, and thus that their decisions are informed by a perfect understanding of the tax consequences of the different amounts of labor they could supply. To the extent that individuals misunderstand or heuristically approximate their tax schedule, the behavioral assumptions of this component of the model may fail. In order to do standard policy analysis in such a world, one way to proceed is to generate an empirical understanding of the heuristics and misunderstandings the populace holds, and then rewrite standard analyses of the welfare associated with a tax schedule assuming that individuals choose their labor supply optimally contingent on their measured imperfect understanding of the income tax.

In this paper, we undertake that exact exercise. We conduct a large-scale and quasi-representative lab-in-field experiment that measures key features of the tax schedule as taxpayers perceive it. Using this data, we estimate the prevalence of the much discussed “Schmeduling” heuristics of Liebman and Zeckhauser (2004). In our experiment, we find that approximately 43% of subjects adopt what Liebman and Zeckhauser term the “ironing” heuristic: they act as if their tax schedule is linear, applying the average tax rate as if it were their marginal tax rate. We find no evidence of any presence of Liebman and Zeckhauser’s other heuristic of interest (“spotlighting”). We also characterize the average misperceptions held by the remainder of the populace, helping inform the features of misperceptions that “schmeduling” overlooks. Overall, we find that a simple two-type model allowing for ironers and correct forecasters does a remarkably good job of forecasting the relevant features of aggregate tax perceptions.

With this empirically informed two-type model in hand, we then theoretically analyze the consequences of this population of ironers in standard models like those discussed above. We find that the ironing heuristic’s presence may be viewed as socially useful. Because ironers underestimate the marginal taxes on their labor, the social planner is able to raise needed revenue with lower distortion to labor supply (as has been previously observed by, e.g., de Bartolome 1995 and Liebman and Zeckhauser 2004). Furthermore, when applied to a convex, progressive tax schedule, this heuristic operates in a manner that would generate minimal mistakes (and thus minimal additional tax revenue) from poor filers who rely on it, and relatively large mistakes (and thus larger amounts of additional tax revenue)
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from comparatively rich filers who rely on it. Through this channel, which we believe was not previously noted or appreciated, ironing serves as a force for achieving additional redistribution. To assess these forces quantitatively, we conduct several simulations building on the approach of Saez (2001). For example, to help illustrate how the social planner views these heuristics, we calculate how he would value a costless technology that immediately taught the 43% of the population who uses the ironing heuristic to instead optimize against the true schedule. Our preferred estimates suggest that the social planner would willingly pay 2.3% of government revenue to stop this technology from being deployed. As another example, to help illustrate how this bias accentuates the value of a progressive tax system, we calculate how accounting for its presence would change the social planner’s willingness to pay to avoid going to a revenue-equivalent linear tax. Our preferred estimates suggest that, with ironers present, costs are 14% higher. Put simply, we find quantitative effects with magnitudes worthy of attention, illustrating the value of being able to accommodate these misperceptions in a relatively tractable model.

In earlier work, “Quantifying Loss-Averse Tax Manipulation,” I build a sufficient-statistics framework for measuring the impact of a commonly studied bias—loss aversion—on attempts to avoid or evade income taxes. To illustrate the behavior of interest, consider a citizen in the process of filling out his tax return shortly before tax day. Throughout the year, this taxpayer earned taxable income, took actions that might be tax advantaged, and made tax payments based on a forecast of the tax liability that would ultimately be owed. When finalizing his tax return, these activities must be precisely documented and reported to the Internal Revenue Service (IRS), and the “balance due”—the difference between the total taxes owed and the tax payments already made—must be settled. If the balance due is positive, the tax filer must pay that amount to the IRS, and thus incurs a literal loss. If the balance due is negative, the tax filer collects a refund, and thus incurs a literal gain. If we suspect Prospect Theory (Kahneman and Tversky 1979) is relevant to tax decisions, an immediate implication is that this gain/loss framing might induce loss aversion. When comparing two similar taxpayers, one who owes the IRS a payment and one who is owed a refund, this model predicts that the taxpayer owing a payment should be more likely to take costly actions that reduce their overall tax liability. For example, the filer could pursue additional credits or deductions, increase evasion, or work harder to ensure that all tax reductions due are claimed.

While the logic discussed above has been long appreciated, and has been tested in a large battery of lab experiments, field evidence testing this story is rare (for a notable exception, see Engstrom et al. 2015). As a consequence, measures of the overall quantitative effect of loss aversion in this setting were previously entirely absent. In my paper, I build a framework that achieves both of these difficult goals. I consider a model where taxpayers report their balance due after manipulating it according to loss-averse preferences. I show that this model generates a strong predictions of bunching near the reference point and a uniform shift of the reported balance due across the entire loss domain. Looking for these qualitative features provides a nice reduced-form test of the presence of loss-averse manipulation. Furthermore, while I show that model primitives like the coefficient of loss aversion are unidentified in
the data available in administrative tax records, I also show that a sufficient statistic for policy impact—the extra reduction in taxes reported due when facing a loss rather than a gain—is structurally identified from the features described above. Implementing this approach in a panel of U.S. tax returns, I estimate that the typical tax filer would reduce their tax bill by $34 more if they faced a loss rather than a gain. These estimates can be used to inform the debate about the wisdom of aggregate overwithholding, and ultimately establish that loss-averse behavioral response is of comparable quantitative importance to other things discussed in that debate.

Turning from income taxation to commodity taxation, my paper “Attention Variation and Welfare” (with Dmitry Taubinsky) considers the implications of heterogeneous biases when assessing the deadweight loss generated by sales taxes. This work builds on the tax salience framework pioneered by Chetty, Looney, and Kroft (2009; henceforth CLK). In that paper, the authors consider the possibility that consumers might ignore sales taxes collected at the register, which in the U.S. is typically not included in the sticker price seen on goods. CLK show that, in a representative agent model where all consumers underreact to sales taxes to the same degree, one can generalize the canonical “Harberger Triangle” formula for calculating deadweight loss to account for this underreaction. Conveniently, the underweighting parameter serves as an additional measurable sufficient statistic for policy analysis, and can be inferred from a comparison of “tax-inclusive price sticker” and “tax-exclusive price sticker” demand curves.

In the CLK framework, the presence of bias is unambiguously a good thing: the greater the degree of underreaction, the lower the deadweight loss. In our paper, Dmitry and I show that this conclusion changes considerably if one allows for the possibility of differences in the bias across people, as would arise if some people attend to taxes and some don’t. Conceptually, the presence of differences in the bias across individuals introduces a new problem not present in the homogenous-bias case: stark misallocation of products across people. When underreaction to the tax is homogeneous, individuals who buy the product value it more than individuals who do not, and thus the market preserves the efficient sorting that is obtained with fully optimizing consumers. However, when consumers vary in their mis-reaction, purchasing decisions depend on both their valuation of the good and on their propensity to ignore the tax, thus breaking the efficient sorting property. This means that using a non-salient tax instrument is no longer unambiguously good, since one must balance the benefits of average underreaction that CLK highlighted against the costs of varying mis-reaction that we highlight.

In our paper, we show that CLK’s sufficient statistics formulas for deadweight loss can be generalized to account for varying mistakes. In the generalization, it now becomes necessary to know not only the mean underreaction of consumers, but also the variance of an appropriately defined underreaction parameter. We deploy a large-scale online experiment to measure this parameter, and find evidence of substantial differences both across individuals and across the economic stakes of the mistake. In our analyses, accounting for this variation results in deadweight loss estimates that are at least 200% higher than those that would be inferred by CLK’s analysis, illustrating that accounting for...
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varying mistakes is extremely important quantitatively.

These three papers constitute my primary contributions to the public finance literature thus far. Across them, a common theme is that even quite complex additional heuristics and biases can be incorporated into the sufficient statistics framework for policy evaluation that is currently in place. Measuring the additional “behavioral” sufficient statistics at times requires additional experimental data collection, but even with this extra complication the end result is that accommodation of behavioral biases can be done without entirely giving up on tractability. In my review paper, “Taxing Humans: Pitfalls of the Mechanism Design Approach and Potential Resolutions” (with Dmitry Taubinsky), we summarize much of the literature on tax misunderstanding, illustrate the ways in which these issues complicate tax policy analysis, and make the case for the sufficient statistics approach used across our projects. In ongoing projects, I continue this style of research, addressing questions such as the importance of price effects versus more behavioral considerations when assessing the dissuasive power of a sin tax (see “Price Isn’t Everything: Behavioral Response around Changes in Sin Taxes” with Kyle Rozema), or the implications of psychological models of information avoidance as a rationalizing theory of poor understanding of tax incentives (in “Tax Information Avoidance,” a work in progress with David Hagmann and Jeffrey Yang).

Analysis of Behavior in Matching Markets

Distilled to its essence, much of the public finance research summarized above can be described abstractly as a case where a policy designer aims to achieve some goal (say, raising revenue to redistribute), but is concerned about how optimizing consumers will change their behavior in response to the policy used to achieve that goal. The behavioral twist is assuming that consumers don’t respond fully optimally, and asking how their suboptimal response changes one’s assessment of a potential policy. In my second line of research, I study an exactly analogous line of reasoning in a very different policy domain: the design of systems to assign students to schools.

A substantial literature in economics has explored mechanism design in two-sided matching markets. The defining characteristic of these markets is the need to accommodate the preferences of the two groups being matched—for example, respecting students’ preferences over schools and schools’ preferences over students. Compared to the one-sided markets more commonly studied, these settings pose unique challenges to reaching desirable outcomes. Difficulty in coordinating on the timing of decisions often leads to “market unraveling” (Roth and Xing, 1994). Furthermore, decentralized approaches often result in unstable matches, which have been empirically shown to be detrimental to the success of these markets. These problems can be avoided by employing a stable matching mechanism to assign a binding match based on preferences reported to a neutral intermediary at an agreed-upon time. However, the use of such a mechanism introduces the new challenge of managing the strategic incentives involved with preference reporting. In common mechanisms, like the much-studied Boston mechanism, careful strategic choices of ones “favorite” picks can be extremely
important, and sincerely reporting preferences (as the designer might wish students to do) can lead to very bad outcomes.

Existing research in student-to-school matching has been reasonably concerned about issues of strategic sophistication. If students are very good game theorists, it may not matter if the matching mechanism used has a very complex optimal reporting strategy—these smart students will respond optimally, information about their preferences will be extracted as well as possible, and students will be matched to schools in a reasonably desirable way. However, one might worry that students are not sufficiently good game theorists to figure out the optimal strategy in such environments, in which case frictions arise. Furthermore, if there are cross-group differences in understanding of the mechanism, undesirable disparate impacts occur. A common worry with school-choice mechanisms is that affluent students will get good and credible guidance on school ranking strategy from their involved parents, whereas low-income students may not. In such a case, a consequence of the complexity of the matching system is suboptimal behavior that, on net, harms disadvantaged students and makes affluent students better off. That is rarely our goal as policy designers.

Motivated by concerns like these, the market design community has focused a great deal of attention on the use of strategy-proof matching mechanisms. If we are worried that some students are bad at understanding complex optimal strategies, then perhaps we should design algorithms that have the simplest optimal strategy possible: telling the truth. In such a case, common logic suggests, the sincere students will not be punished, all students can be expected to pursue the optimal strategy, and we may no longer worry about suboptimal behavior.

My contribution to this literature thus far has been to demonstrate that meaningful concerns about suboptimal behavior still exist even in strategy-proof mechanisms, and to explore some of the consequences of the persistence of these mistakes.

My interest in this area began when I ran a large-scale survey among graduating medical students for a project that was not fundamentally related to matching theory (described in the next section). This setting is of note, since the system designed to match medical students to medical residencies is one of the leading success stories of strategy-proof market design. This large labor market is organized by a modified version of the Gale-Shapley algorithm, and is carefully designed to elicit sincere preferences over matches from the students (Roth and Peranson 1999). In my preparations for running the survey, I learned in talking to medical students that many seemed to believe the mechanism was manipulable, and believed that they and others acted on this belief. In my work “Suboptimal Behavior in Strategy-Proof Mechanisms: Evidence from the Residency Match,” I formally document these beliefs in a survey among nearly 600 medical students participating in the match. This paper summarizes my initial finding that approximately 17% of students self-report submitting a preference ordering different than their “true preferences.” I also dig into the causes of that divergence, and makes some attempts to validate these unincentivized responses. In a follow-up experiment that I think is significantly more impressive, “An Experimental Investigation of Preference Misrepresentation in the Residency Match” (joint with Sam Skowronek), we recruit over 1,700 medical students to take part in a simple, incentivized experiment
nearly immediately after the deadline for their match submission list. In that experiment, which transparently applies the same mechanism they just used for their “real” match, we find stark evidence of costly and intentional preference misrepresentation. In the experimental task, 23% of students fail to pursue the simple strategy of telling the truth. In short, it appears that a good fraction of participants in an exemplar strategy-proof market don’t act like they understand strategy-proofness, suggesting that worries about the harms of suboptimal response remain even in these markets. I additionally explore the predictors of who makes mistakes in this domain: across projects, I find evidence that mistakes are more common among worse students and are more common when strategic position is weaker (among other predictors). These associations are useful for assessing who is harmed and who benefits from the complexity of the matching mechanism.

In my accompanying (short) theoretical piece “Mistaken Play in the Deferred Acceptance Algorithm: Implications for Positive Assortative Matching” I demonstrate the challenges of welfare analysis in this environment. In contrast to the natural intuition that mistakes in this setting must be socially undesirable, I demonstrate that under some conditions the presence of mistakes can effectively provide additional information to the matching mechanism that facilitates the successful sorting of the best students to the best schools. Depending on the degree to which optimal reporting in the mechanism predicts the students’ potential for academic performance, the welfare effects of the presence of mistakes can range from dramatically negative to dramatically positive. Given the evidence in my studies (convergent with several other recent studies in the literature), there does appear to be a positive association between student quality and optimal behavior, suggesting the potential for mistakes to be helpful if positive assortative matching is the goal. As in the tax settings above, we again see that individual’s difficulty in understanding incentives meaningfully interacts with the distributional goals the market is designed to achieve.

**Economic Applications of Subjective Wellbeing Data**

My earliest papers, written in graduate school and during my postdoc, were focused on assessing economic applications of happiness data. While this work is very different in style to the projects described above, it is motivated by the same fundamental desire to accommodate suboptimal behavior in economic welfare or policy analyses.

When assessing welfare, economists have a strong preference for the “revealed preference” approach. Put simply, rather than imposing our own guesses of what makes individuals best off, we instead prefer to learn how individuals’ welfare is determined by assuming that their decisions maximize it. Despite this preference, economists regularly ask questions that choice data cannot answer in an entirely satisfying way. Such situations can arise when considering questions involving the valuation of public or non-market goods; when approaching a problem where data constraints make the collection or analysis of choice data infeasible; or when considering an environment where mistakes, heuristics, or biases cloud the relationship between choices and preferences.
When facing problems like these, economists are increasingly turning to subjective well-being (SWB) data as a tool to assess preferences or welfare. In these approaches, large-scale surveys are deployed to a population, collecting self-reports of individuals’ evaluation of their quality of life, their life satisfaction, or measures of transitory positive affect. The increased acceptance of these measures is perhaps best represented in the adoption of the Sarkozy report (Stiglitz, Sen, and Fitoussi, 2009), in which a large panel of economists—including several Nobel prize winners—advocated for the principled inclusion of large-scale SWB surveys into GDP-like measures of national progress. Advocacy of this sort has led to national governments as diverse as those of the United Kingdom, Bahrain, and New Zealand to undertake serious attempts to achieve that goal. Given this boom of both interest and data availability, data of these sorts have rapidly proliferated in economic applications, ranging from pricing pollution (Levinson, 2012), to studying the impact of health on the marginal utility of consumption (Finkelstein, Luttmer, and Notowidigdo, 2013), to formally testing of models of economic growth (Aghion, Akcigit, Deaton, and Roulet, 2016).

When we integrate a new tool into our toolkit, it is essential to understand how this tool performs as compared to standard methods. Unfortunately, despite substantial literatures inferring preferences from both choices and SWB, at the time I entered this literature (2009) we knew remarkably little about the relationship between these two approaches. To what extent do choices and SWB “reveal” the same construct, and yield the same conclusions about preferences or welfare? If the answers they provide are different, what are the differences? Which features of decisions might be over- or under-weighted as a function of the data used to infer preferences? Understanding questions such as these is essential for evaluating the wisdom of using SWB data for welfare or policy analysis. These questions motivated the line of research I pursued with Daniel Benjamin, Ori Heffetz, and Miles Kimball.

Our first paper in this research agenda—“What Do You Think Would Make You Happier? What Do You Think You Would Choose?”—directly assesses the alignment of choice-rationalizing utility and forecasts of SWB in a survey setting. We pose a variety of hypothetical decision scenarios to three respondent populations: a convenience sample of 1,066 adults, a representative sample of 1,000 adult Americans, and 633 students. Each scenario has two alternatives, directly trading off attributes of the decision problem. For example, one scenario describes a choice between a job that pays less but allows more sleep versus a job with higher pay and less sleep. We ask respondents which alternative they think they would choose. We also ask them under which alternative they anticipate greater SWB, using three measures crafted after common SWB questions found in large-scale social surveys.

This scenario-study approach has much in common with a large psychological literature on “preference reversals.” In such prior work, demonstrations that an individual would choose a non-happiness maximizing option were often used as demonstrations of failures in the decision-making process—for example, taking a failure to maximize happiness as evidence of a faulty forecast of the hedonic consequences of an action. In contrast to this typical design and interpretation, our studies were conducted to shut down these channels and to test if subjects were deliberately attempting to maximize the construct measured by standard SWB questions. A finding that subjects do not attempt to maximize
SWB constitutes a direct demonstration that these large-scale SWB surveys measure something conceptually distinct from economic utility. Furthermore, our design allows us to assess the features of the environment that predict the magnitude of deviation, and thus predict situations in which the policy recommendations of these two approaches will or will not diverge.

We have two main results. First, we find that overall, respondents’ SWB predictions are a powerful predictor of their choices. SWB and choice coincide 83 percent of the time in our data. As a tool for forecasting binary choices, SWB data performs reasonably well. However, this high rate of concordance masks significant heterogeneity. We find that the strength of this relationship varies across choice situations, subject populations, survey methods, questionnaire structure variations, and measures of SWB, with coincidence ranging from well below 50 percent to above 95 percent.

Our second main result is that discrepancies between choice-rationalizing utility and SWB rankings are more than random noise, but instead capture predictable and systematic differences in subjects’ understanding of these constructs. In addition to eliciting participants’ choices and predicted SWB, in some surveys we also elicit their predictions regarding particular aspects of life other than their own SWB. Aspects such as sense of purpose, control over one’s own life, family happiness, and social status systematically contribute to explaining choice, controlling for own SWB.

We interpret these results as supporting a conceptualization of SWB not as an approximation to utility itself, but rather a measure of an abstract commodity that individuals consciously trade against other goods and goals. Such an interpretation weakens the argument for the use of this measure as an approximation to overall welfare.

In a later project—“Can Marginal Rates of Substitution be Inferred from Happiness Data? Evidence from Residency Choices”—we sought to make two contributions beyond our initial work in this domain. First, we sought to move beyond the unincentivized scenario-study paradigm in order to confirm and expand on these findings in an economically important and incentivized real-world choice. Second, we sought to move beyond studying the alignment of choice- and SWB-rankings, and to more directly study the parameters of preferences one would infer from these rankings.

To do so, we examined the preferences of 561 medical students as they went through the process of graduating from medical school and moving into a medical residency (It was my experience with this study that generated my interest in the market design research described above). We surveyed students shortly after they submitted their preferences over residencies to the virtually incentive-compatible matching mechanism used to determine assignments. We additionally elicited students’ forecasts of their SWB at their top 4 most preferred residencies, and a variety of attributes relevant for a residency utility function. These data permit the estimation of random utility models, forecasting students’ preference ordering of residencies as a function of residency attributes. We conduct this exercise using both choices and SWB forecasts as proxies for preferences, and compare the rates of tradeoff between attributes that are generated by each approach. The most common application of SWB data in the economics literature is to infer tradeoffs in this way—by directly studying the impact of our initial results for this estimation exercise, we more tightly link our slightly philosophical point on the cleanest
interpretation of SWB data to the practical issues surrounding SWB-data’s usage.

As in our earlier work, we find substantial correlation between choice and SWB rankings, and thus replicate our findings that SWB data are practically useful for the raw forecasting of choices. However, despite this correlation, we find that all considered SWB measures predict meaningfully different tradeoffs than choice (and that this difference is not driven by misunderstandings of the mechanism documented above). Choice-based and SWB-based rates of tradeoff often differ by as much as a factor of two, demonstrating that each serves as a poor approximation for the other in this setting. If our goal with one data source is to approximate a welfare notion captured by the other, these results suggest a degree of error that may be viewed as unacceptable.

Summary

Across this body of work, I am making progress on important task of integrating psychology into the large-scale economic evaluation of policy. Particularly in my work in tax settings, I am documenting that judgment and decision-making concepts can have quantitatively important impact in the field, and can interact with Econ-101 concepts in important ways. I interpret my work as a meaningful piece of an argument currently being made in the literature: that accommodating heuristics and biases in formal policy analysis is quantitatively important, empirically tractable, and essential to our understanding of some of the key distributional channels that our policies are meant to influence.

References (excluding my own works, summarized on page 1)