

Writeups for Nov. 10 meeting - Edited for Public Consumption

NOTE: This draft was current as of the time that the convening was held; Open Philanthropy Project's thinking and have been updated significantly since that time

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Criminal justice reform

10:15 session

NOTE: This draft was current as of the time that the convening was held; Open Philanthropy Project's thinking and strategies on criminal justice have been updated significantly since that time.

Goal: To address the needs of high-crime/high-incarceration communities by reducing both crime rates and rates of unnecessary incarceration

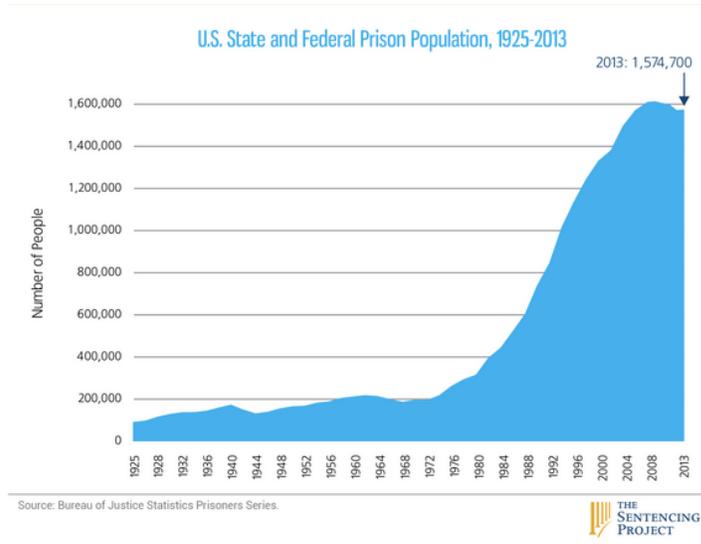
Amount of Work that We Have Done on this Issue So Far: Our investigation into criminal justice, while definitely ongoing and by no means close to being complete, is substantially deeper than any other investigation we have undertaken thus far. We have probably had between conversations with between 100 and 200 experts in the field about this topic.

What Changes We Seek

Mass incarceration is quickly becoming the cause de jour in criminal justice—and for good reason. Between 1972 and 2007, the U.S. incarceration rate nearly quintupled—and by 2012, the U.S. was incarcerating 2.23 million people and had the highest incarceration rate in the world.¹ But while we are extremely concerned about mass incarceration, we also recognize that crime and incarceration are inextricably linked. First, if incarceration goes down substantially, absent any other changes, the crime rate will likely rise somewhat (incarceration does serve an anti-crime function, at the very least by incapacitating people who are likely to reoffend; the fact that incarceration has been overused does not obviate the fact that some incarceration is necessary). Second, crime rates may at some point rise again on their own; crime rates rose in the United States from the 1960s to the 1990s, and there continues to be debate in academic circles about why crime fell dramatically in the 1990s and whether we currently have the tools to prevent large spikes in crime. If crime starts to rise again, it will be hard to continue progress on reducing unnecessary incarceration.

As a result, we seek to work on both crime and unnecessary incarceration, either in tandem or as separate interventions, with the goal of both building the political will to address unnecessary incarceration and discovering new tools to reduce crime, preferably those that do not involve long prison sentences.

¹ National Academy of Sciences, *The Growth of Incarceration in the United States: Exploring Causes and Consequences* (2014), 33.



Why We Are Interested in this Cause

We are especially interested in this area because there seems to be an unusual window of opportunity to achieve real policy change in this space. In particular, as a result of state fiscal crises, there has been increasing interest at the state level at cutting prison spending by reducing incarceration—including in Southern and conservative states. There has also been a wave of conservative interest in what has traditionally been a solely liberal cause, especially from libertarians who object to such massive growth in the size of the government. This has also led to some unusual partnerships on criminal justice efforts, including by Van Jones and Newt Gingrich as well as Cory Booker and Rand Paul.

We are considering potentially spending in the range of \$5 million to \$30 million annually on criminal justice issues, including some 501(c)(4) spending. We would start at the lower end of this range and potentially increase our investment as time goes on.

Other funders in this space (partial list)

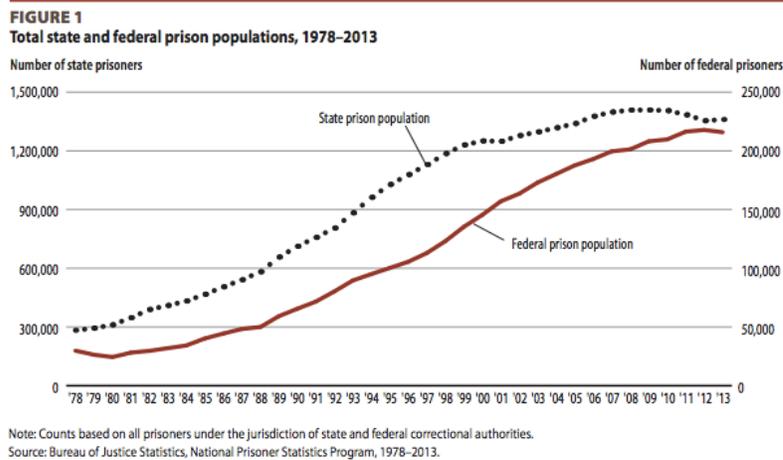
- Open Society Foundations
- MacArthur
- Arnold Foundation
- Ford Foundation
- Public Welfare Foundation
- Pew Charitable Trusts
- Smith Richardson Foundation

Atlantic Philanthropies also used to fund in this space, but is winding down its 501(c)(3) giving.

The primary funder of research on crime is the Federal government, especially the National Institute of Justice at the Department of Justice, which gave \$159,873,645 in funding in FY 2013 (370 awards) and \$233,536,728 in funding in FY 2014 (418 awards), covering a wide range of topics. NIJ research grants can be substantial (several million dollars per project) but applications are complicated and bureaucratic and NIJ research funding priorities shift regularly.

Landscape:

Criminal justice is largely a state and local issue; only about 14 percent of all prisoners in the United States are in Federal prisons.²



This means that the vast majority of campaigns on criminal justice issues will be at the state and local level. We are still surveying the state-based landscape on criminal justice, but note that the capacity of state-based criminal justice groups varies tremendously by state (not surprisingly). Many national groups also have state affiliates.

Examples of national criminal justice groups:

- *ACLU*
- *Sentencing Project*
- *Drug Policy Alliance*
- *Right on Crime*
- *Justice Fellowship*
- *Pew Charitable Trusts*
- *Council on State Governments*
- *Vera Institute*
- *Families Against Mandatory Minimums*

Non-traditional allies are increasingly becoming interested in the criminal justice space as well. For example, the AFL-CIO has spoken out against mass incarceration and is

² E. Ann Carson, U.S. Department of Justice, “Prisoners in 2013,” <http://www.bjs.gov/content/pub/pdf/p13.pdf>

funding some campaign work around the issue as well as member education and the inclusion of formerly incarcerated individuals in community benefits agreements. The national faith-based community organizing group PICO has made reducing mass incarceration one of its key campaigns. The conservative group ALEC, which promotes legislation for state legislators, has also been doing some work to reduce prison size, reversing its previous position of support for mandatory minimums and other bulwarks of mass incarceration.

As already mentioned, state fiscal crises are driving a lot of criminal justice reforms. There is also increased public support for the idea that the United States has too many people in prison,³ although that has not translated into many substantial sentencing reforms. New York, New Jersey, and California have seen the greatest drops in their prison populations.⁴

To understand what kinds of interventions will have the most impact on reducing crime and unnecessary incarceration, it is worthwhile to look at some of the factors maintaining the current levels of incarceration and crime:

- Mass incarceration is the status quo—in most cases, inaction leads to continued high levels of imprisonment, and in some cases actually ensures higher rates.
- Lack of knowledge about how to reduce crime
- Prosecutors, sheriffs, and some victims' groups oppose reform efforts, and their opinions are well respected by the public and policymakers.
- Misaligned incentives:
 - Politicians (including prosecutors), especially Democrats, still do not feel that supporting decarceration is entirely risk free
 - Police officers judged by numbers of arrests, stop/frisks, etc. (also influence of social norms)
- Public attitudes not aligned with realities of what is public safety enhancing (e.g., people don't understand that you age out of crime, or that there are ways to confine people without incarcerating)
- We don't have sufficient intermediate options between prison and letting people live completely independent lives—and as a result, we incarcerate large numbers of people who could probably leave confinement if there were highly supervised alternatives to prison.
- People who leave prison have trouble reintegrating into society, which may make them more likely to commit crimes again
- Segregation means that there's a disjuncture between the political influence of high-crime/high-incarceration communities, and communities that make the political decisions (e.g., Philadelphia and Pittsburgh have most of the crime and

³ Public Opinion Strategies and the Mellman Group, *Public Opinion on Sentencing and Corrections Policy in America* (2012), http://static.prisonpolicy.org/scans/PEW_NationalSurveyResearchPaper_FINAL.pdf.

⁴ Sentencing Project, "Fewer Prisoners, Less Crime: A Tale of Three States," http://sentencingproject.org/doc/publications/inc_Fewer_Prisoners_Less_Crime.pdf.

incarceration in Pennsylvania, but decisions about whom to put in prison are made in large part by officials from the rest of the state).

- The fact that former felons do not vote in many places
- Rates of illegal gun ownership in high crime communities make crime committed much more dangerous
- Racial attitudes affect how White Americans feel about both crime and incarceration
- The current criminal justice system employs large numbers of people, who have significant personal investments in the current system and who have been shaped—however uncomfortably—by it.
- Many interlocking issues associated with poverty

Possible Sketch of a Strategy on this Topic

As already mentioned, it seems like there is an unusual political window of opportunity on criminal justice reform, which is part of what makes this a potentially interesting program area. That said, since this area is changing rather rapidly, it also seems premature to create a single theory of the case for how social change is likely to occur. As a result, we envision putting our money in several different places, and potentially adjusting that as time goes on and the dynamics of the “window” start to clarify a bit further.

As of now, it looks like our strategy on this issue will likely be divided between four general areas:⁵

- A) Efforts to improve the behavior of prosecutors
- B) Broad institution building
- C) Mass incarceration campaigns
- D) Research on crime prevention

While our funding on these issues could easily be spread out across the country, one possibility worth discussing is whether we should “go all in” on a few states to be able to make the most impact and get better feedback about how our grants are working on the ground.

A. Efforts to Improve the Behavior of Prosecutors

When we speak to people about important players in the criminal justice system, they routinely point to prosecutors. First, prosecutors have tremendous amounts of discretion (and some use that discretion better than others): deciding whether to charge someone for

⁵ While many of the other funders are also trying multiple approaches to criminal justice reform at the moment (rather than taking a singular theory of the case), it is worth noting that our approach includes two things that other approaches do not: a focus on prosecutors, and some attention to researching alternatives to incarceration.

a particular offense, what to charge, what to recommend for sentencing, etc. Second, prosecutors are routinely the political force most opposed to sentencing reform (in part because it takes away their discretion); changing that dynamic would go a long way towards increasing the chances for substantial sentencing reform. That said, very little work is currently done on prosecutors and mass incarceration; very few foundations put substantial funding into work on prosecutors, and very little research is done on prosecutorial decisionmaking. Prosecutors are notoriously closed offices, and figuring out how to change their behavior and/or how to work with them more productively is extremely difficult.

Hypotheses about prosecutors:

- *Neither prosecutors nor the public know what it means to be a “good prosecutor;” while some prosecutors’ offices may have their own metrics to judge employee performance, those are not the same as the metrics that people who care about incarceration would likely use. At the same time, the political incentives for prosecutors are often in the direction of charging up, since being a “tough as nails prosecutor” is a well-trodden strategy for upward political mobility.*
- *We still do not know enough about how prosecutors currently behave, which makes it harder to figure out the best interventions*
- *Prosecutors are sufficiently political that scaring them at the polls could impact the way they behave once in office*

Possible types of interventions (note: to identify the best interventions or brainstorm new ideas, we intend to convene a meeting of prosecutors and academics who study prosecutors):

- 1) Research on prosecutors
 - Additional research/data gathering, aimed at figuring out at a more granular level what prosecutors’ offices currently do and why
 - Quantitative work on variation in prosecutorial behavior as well as political science work looking at what sort of prosecutorial behavior leads to upward political mobility. In addition, qualitative/ethnographic work that would allow us to better understand how prosecutors understand their jobs and the institutional processes that lead to prosecutorial stringency.
- 2) Efforts to change the culture of the profession
 - Develop a more concrete vision of what it means to be a good prosecutor
 - Provide training/curricula covering what it means to be a good prosecutor. This could be aimed at people in their first year of being a prosecutor.
 - Score-card for ranking prosecutors
 - Promote/highlight good prosecutors; develop and share best practices (including, potentially, through mechanisms like a trade journal for these new kinds of prosecutors)
- 3) Institutional changes/efforts to change the way prosecutors offices function

- Work with behavioral economists to create nudges to change the incentives for prosecutors. Give prosecutors information about the cumulative impacts of their work, to help them make better ad hoc decisions.
 - Encourage offices to use new metrics of success (which take into account mass incarceration) when determining the promotion prospects of individual prosecutors
- 4) Political efforts
- Campaigns against targeted prosecutors, including testing whether fiscal messages can counter tough on crime messages, especially in conservative jurisdictions
 - Score-card for ranking prosecutorial candidates during elections and prizes for innovative prosecutors

B. Institution Building

In general, the hypotheses on institution building are still preliminary, because we have much more of a sense of the national field than we do of state-level dynamics. Institution-building grants will tend to be general operating support (rather than campaign or program specific-grants), and are intended to build the field of criminal justice reform over time rather than achieve short-term policy objectives.

The following hypotheses shape our current thinking about the kinds of institutions we would likely fund; that said, as mentioned already, this thinking is still definitely evolving. Where the hypotheses do not inherently specify the kinds of organizations we would fund, we list examples of the types of organizations below.

Hypothesis: The criminal justice field could use more national groups with the capacity to organize and mobilize large numbers of people.

Hypothesis: Effective state-based groups in key states could use more capacity, especially field capacity and political capacity

Hypothesis: Effective litigation in strategic locations could help to spur reductions in prison populations

Hypothesis: States need more voices with intimate knowledge of the criminal justice system to counter the testimony of prosecutors

Possible places to invest:

- Policy offices of public defenders

Hypothesis: If business groups could be made more interested in reentry, that could help expand coalitions for CJR

Possible places to invest:

- Chamber-of-commerce-like business affinity groups for criminal justice
- Direct investment with state chambers of commerce to increase their willingness to lobby directly on criminal justice issues, especially connected with state budget questions

Hypothesis: The burgeoning conservative interest and/or faith-based interest in criminal justice needs to be strengthened; in particular, both groups need to coalesce around a fuller policy platform that would actually make a dent in incarceration, and need to develop their mobilization capacity at the state level.

Possible places to invest:

- Creation of a national conservative think tank/center on criminal justice or of state conservative think tanks (particularly covering criminal justice budget issues).
- Invest in the field capacity of conservative faith-based groups
- Fund multiracial evangelical or faith-based organizing groups working on criminal justice issues.

Hypothesis: Public attitudes are not aligned with realities of what is public safety enhancing (e.g., people don't understand that you age out of crime, or that there are ways to confine people without incarcerating); better media and education could affect these attitudes

Possible places to invest:

- Journalism investments
- Effort to put on more TV stories about incarceration (does not currently exist). Since Americans get a lot of their sense of crime and its danger from TV, one could imagine a concerted effort to counter that by creating more TV programming about incarceration.
- Paid media campaigns
- Evangelical groups that seek to tell stories about what they have witnessed in prisons
- Organizations that seek to relay anecdotes about individuals who were diverted from prison (into drug courts or other programs) and thrived.
- Organizations that seek to relay, in a visceral way, the high cost to communities of having so many people in prison, helping to clarify why the current system is not working.
- Organizations that will sponsor public education campaigns on appropriate punishment for sex offenders or violent offenders, since it will be impossible to dramatically reduce prison populations without addressing approaches for more politically unpopular offenders

C. Campaigns to Reduce Mass Incarceration

Given the increasing consensus around the fact that mass incarceration is a problem, one way to address that may be through campaigns to reduce incarceration levels in key states. This could be done either on a national scale or through smaller state-by-state campaigns that we could help fund on an ad hoc basis (for example, investing in California Proposition 47, as Cari and Dustin did earlier this year).

D. Research on Crime Prevention

Given our goal of reducing crime as well as reducing unnecessary incarceration, it seems important to invest in increasing the number of proven interventions to combat crime. The kind of research we fund would likely fall into one of two categories: descriptive/qualitative research intended to help people better understand what drives certain parts of the criminal justice system (e.g., studies of how prosecutors make certain decisions, or studies of how gang members get their guns); or efforts to work directly with government and researchers to develop certain innovations in government and study their effectiveness.

Procedurally, one option is to fund certain criminal justice research centers at universities, giving them general operating support. Another option would be to fund individual academics whose research agendas we support; we would not fund projects on a case-by-case basis. Should we choose to fund individual academics, we would be particularly interested in those who work on alternatives to incarceration and parole/probation reform; police procedures; and gun enforcement, which are parts of the criminal justice system that we think could make a big difference in reducing crime or reducing the severity of crime (see also the description of what makes our approach different, at the start of the strategy section).

Risks and reservations about this cause:

- Prosecutors are a risky space to work—it is clear they are important to criminal justice reform but very unclear whether anything we do could make an impact on their behavior (for example, it is not clear that even guidance from the Attorney General is enough to shift dramatically the behavior of individual Federal prosecutors)
- The government already funds substantial amounts of research on criminal justice (and it has still been hard to make progress on proven interventions to reduce crime); will our additional funding really make an impact here?

Further questions for discussion:

- There is a lot of interest in criminal justice right now from funders, and they all want to work together. To what extent should we be trying to work with other funders, especially on campaigns (which might require a lot of money)?

- How much should we focus on taking advantage of short term political dynamics—e.g., by funding conservative groups? How serious are the drawbacks to being a generally liberal foundation funding some conservative groups?
- What are the merits/drawbacks to focusing the majority of our funding on a few specific states?

Labor Mobility

11:45 session

Goal: To increase the net number of people from low-income countries who are able to move and work legally, temporarily or permanently, in wealthy countries (especially the United States), whether through additional inflows, reduced deportations, or regularization of undocumented migrants. We see it as beneficial for all people to be able to move and work legally and safely, but anticipate that we might focus particularly on lower-skill workers. Note that this cause is similar to general immigration policy, but has somewhat different emphases (see below for more details).

Amount of Work that We Have Done on this Issue So Far: We have probably had 40 conversations on this topic, with both academics and practitioners. We also commissioned a review of the economic literature on the relationship between immigration and current residents' wages.

What Changes We Seek

We would like to see the United States allow substantially more people to move and work here legally. There are benefits to both temporary and permanent migration from our perspective, although obviously the appropriate ratio between people allowed to move permanently versus temporarily is a broader discussion.

We could conceptually support a number of policies to promote this goal, but the degree of “crowdedness” and public support seems to differ widely across them: we see relatively wide support for allowing more higher-skill immigration, strong partisan differences with respect to regularizing the status of people who are here already, and little vocal support anywhere on the political spectrum for allowing more lower-skill people to come.

Most, but not all, of the larger changes we would like to see would require policy changes. On the policy side, the comprehensive immigration reform bill passed by the Senate in 2013 represents many changes we see as beneficial. The CBO projects that it would increase the U.S. population by 16 million people over 20 years, including 2.3 million participants in a new temporary worker program, while increasing annual GDP by more than a trillion dollars. We would conceptually prefer a bill that permitted an even larger increase in population, while also recognizing that much smaller changes would still be meaningful.

There are also many smaller scale efforts that we could support in the absence of comprehensive immigration reform (“CIR”) to enable more people to work in the country legally, ranging from better connecting potential migrants with employers in visa categories that are not already maxed out to naturalizing more legal permanent residents (thereby enabling more family migration) to funding anti-deportation campaigns. We also see value in building support for the concept of labor mobility, which is a longer-run effort.

We believe that gains to moving to other developed countries are probably comparable to gains from moving to the United States, but have primarily focused our exploration on the United States because of our familiarity and, to a lesser extent, the country's historical openness to immigration.

Why We Are Interested in this Cause

Our reading of the academic literature suggests that migrating from low-income to high-income countries massively increases income for migrants—significantly more so than any other development intervention we know of. Economist Michael Clemens of the Center for Global Development has argued that the potential global gains from liberalizing labor mobility restrictions are in the tens of trillions of dollars a year, dwarfing the impact of other barriers to trade.

Although immigration already plays a prominent role in United States political discussions, there are many fewer people interested in migration as an international development issue. We feel that low-skill immigration may represent the greatest potential welfare gains (since low-skill immigrants are generally coming from extremely low-income backgrounds), yet is also probably the aspect of immigration policy that is least emphasized by current philanthropists.

Other funders in this space:

Funders in this space have largely been focused on CIR, and are currently reevaluating their funding in light of the fact that CIR is unlikely to pass in any form before 2017. It is hard to predict how large spending on immigration issues will be over the next few years, but several of the big foundations are currently pondering devoting more money to international migration flows and seeing migration as a global (rather than U.S.) issue. Funders will also be devoting large sums of money to supporting the Obama Administration's administrative relief efforts, should those materialize after the midterm elections.

Large donors include:

- Open Society Foundations
- Ford Foundation
- Carnegie Corporation
- Unbound Philanthropies
- MacArthur Foundation
- Four Freedoms fund (grantmaker collaborative)

A lot of smaller foundations also fund in this space. Atlantic Philanthropies spent substantial amounts of money supporting CIR, but is winding down.

Outside of CIR efforts, there has been less activity. The World Bank has supported several small programs to connect workers from low-income countries to high-income country labor markets.

Landscape

The field of people working on immigration issues has been crowded over the past few years, as people geared up for what was predicted to be the best chance of passing CIR in many years. CIR reached the top of the national political agenda in 2013 and passed in the Senate, but failed in the House of Representatives. Advocates do not generally expect CIR to have a chance of passing again until at least 2017.

President Obama planned to announce administrative immigration relief, but delayed the announcement until after the midterm elections. Outside observers still expect some form of relief to be announced, likely in December, which could affect up to 7 million people.

Many fewer people work on migration as a development issue, and there is no migration office within USAID. Michael Clemens at CGD and Lant Pritchett at the Kennedy School are two of the top American scholars focusing on migration as a development tool. Clemens has been a driving force behind a number of policy initiatives related to migration and development, and the Open Philanthropy Project has funded some of his most recent work.

Current Grantees on this topic

We have made three grants to date in this area:

- [\\$1.2M to the Center for Global Development \(CGD\) for labor mobility research](#): CGD Senior Fellow Michael Clemens is one of the leading advocates for the economic and humanitarian importance of loosening policy barriers to mobility. This grant supports his work for 3 years and will also cover a couple of CGD-convened working groups on aspects of migration and development policy.
- [\\$285K unrestricted to the ImmigrationWorks \(IW\) Foundation](#): IW organizes small business owners to advocate for more lower-skill workers to be allowed to come to the U.S. We see this as a relatively neglected demographic group, and IW was the most prominent group we found working on it, but is still quite small (~\$600K/year).
- [\\$1.5M to the International Organization for Migration \(IOM\) for a pilot project to give Haitians access to U.S. seasonal work visas](#): This grant supports an IOM pilot program to help a small number of Haitians find legal seasonal work in the U.S., with the hope that the program might inaugurate sustainable legal access to U.S. seasonal work for Haitians.

Possible Sketch of a Strategy on this Topic

Interventions on this issue could fall into a number of different categories:

- **Support for longer-term comprehensive immigration reform efforts.** This could include co-funding efforts with other immigration funders to increase grassroots advocacy on immigration issues, build the power of Latinos, or encourage better coverage of immigration issues by the media.
- **Efforts to increase the number of immigrants allowed in as part of CIR.** This could include supporting advocacy efforts or policy efforts by people who want more low-skill immigrants in the country (e.g., businesses that want to hire more low-skill workers). We might also want to fund more policy-oriented research efforts as part of this work—for example, to figure out what it would look like to have a dynamic visa system rather than legislative caps.
- **Efforts to ensure that existing immigration programs are used more effectively as development tools.** This could include projects like funding someone at USAID or the State Department to work on migration-as-development issues or supporting pilot programs that better connect potential migrants from low-income countries with employers in visa categories that are not already maxed out.
- **Efforts to let more people into the country/allow more people to stay even in the absence of CIR:** This category could include encouraging more naturalization by legal permanent residents (which would both increase the number of immigrants voting in the U.S. and also allow more of their family members to migrate to the U.S.) and efforts to convince the Administration to exercise its parole authority more broadly, which would, on a case-by-case basis, let people enter or stay in the U.S. and work here without counting under the visa caps. In the longer run, this category could also potentially include working on bilateral migration agreements with other countries. There are also a number of interventions that would not bring new people into the country, but would allow current undocumented immigrants to stay in the country longer. In general, we are more interested in interventions that let people stay longer in the country than those that regularize someone’s status, since the economic gains to regularization are about ten times lower than the gains from moving.
- **Efforts to increase conceptual support for labor mobility.** This would be a longer term project—perhaps involving supporting more research or journals on this topic. If, however, our ultimate goal is to allow significantly more low-skill immigrants into the country (which requires legislation), one key question is which constituencies would be most moved by the promotion of this idea, and whether we should focus on constituencies that could potentially be a political force.

Risks and reservations about this cause:

- While we generally feel that likelihood of political success is difficult to predict

past the short term, immigration seems to be a particularly charged issue where the obstacles to fundamental change may be extremely strong. Is it realistic to imagine that the United States will ever allow substantial migration from developing countries—and if the U.S. is not likely to do so, is work on this cause still sufficiently valuable?

- Allowing many more low-skill immigrants into the country continues to be quite controversial. A common concern about allowing more lower-skill immigration is that it would reduce income for low-income Americans. As part of our investigation, we commissioned a review of the economic literature on that question, published [here](#), which found limited reason for concern. We hope to conduct a thorough review of other arguments and counterarguments about labor mobility, which we have done some work on but have not yet completed.
- Do we need to be able to articulate a vision of what the ideal “end goal” looks like for this cause? It seems clear to us right now that the United States should allow in more immigrants—but one could imagine many situations where doing so unthoughtfully could create issues. To take an extreme example, there are potentially huge problems to a society in which all of the people from one ethnicity/race are in the country only as temporary workers and do not vote.

Further questions for discussion:

- Does it make sense for us to fund work to support a future CIR bill? On the one hand, it is fundamental to the things we care about; on the other hand, there are a lot of other funders in that space. If we work on labor mobility, to what extent should we make an effort to focus on the aspects of this cause that make us unique?
- How much should we seek to build the conceptual idea of labor mobility? If we work on that, which constituencies should we seek to influence?

Macroeconomic stabilization policy

1:30 session

Goal: As the recent Great Recession shows, business cycle swings can carry enormous humanitarian costs. Our goal in this area would be to reduce those costs through improved macroeconomic policymaking.

Amount of work we have done so far on this issue: We have conducted a fairly wide-ranging but not particularly deep investigation. We've talked to around 30 economists and policy advocates and made two exploratory grants.

What changes we seek, and why

We do not have a full list of the changes that we would like to see in this area, but we see at least three areas of potential interest:

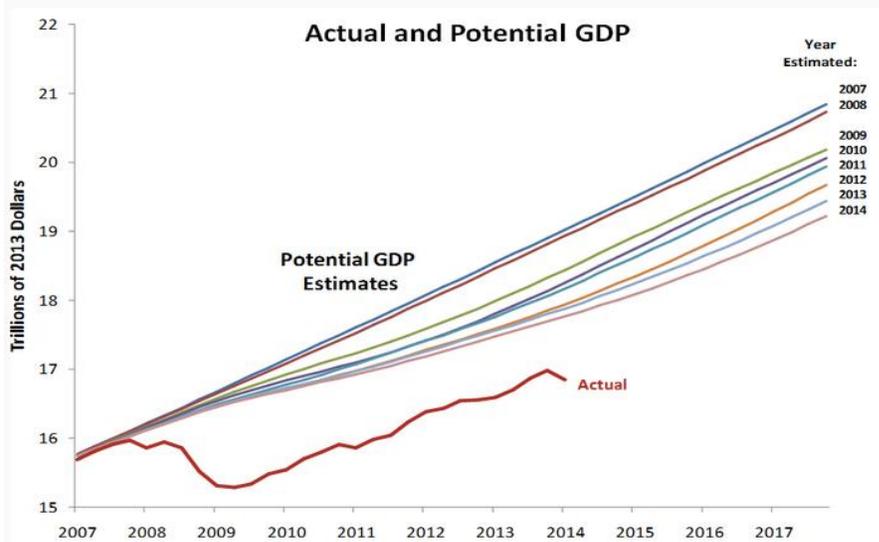
- **Better monetary policy.** This could range from short term efforts to influence the Federal Reserve to keep interest rates low to longer term research or advocacy efforts to promote newer approaches to monetary policy, including price level or nominal gross domestic product (NGDP) targeting or a higher inflation target.
- **Countercyclical fiscal policy.** Economists broadly agree that countercyclical fiscal policy is desirable, but the details of which policies to enact are disputed, and there is little political movement. Plausible policy targets might include protecting or expanding existing automatic stabilizers, promoting a Fed-like body to adjust tax rates across the business cycle, or preparing/maintaining a list of good shovel-ready projects for the next stimulus that might be required.
- **Preventing the next recession.** Improved financial regulation or better “early warning” systems might help prevent future recessions.

Why We Are Interested in this Cause

The Great Recession appears to have cost the global economy more than \$10 trillion of potential output, with particularly negative effects for the millions of people who lost their jobs. While such large recessions are obviously rare, there still seems to be substantial room for improvement in the current recovery, and the business cycle is an ongoing source of volatility across the income spectrum.⁶

⁶ Chart from Larry Summers, Oct 30 2014, <http://www.voxeu.org/article/secular-stagnation-hypothesis>

Figure 1a. Actual and potential GDP in the US



Sources: Congressional Budget Office, Bureau of Economic Analysis

In the long run, greater economic stability would have large positive effects on welfare, particularly for lower and middle-income people.

Although economists, financial market participants, and policymakers already pay considerable attention to the impacts of downturns and efforts to mitigate them, there is relatively little philanthropy or organized advocacy around the topic, and we have found some potential gaps, particularly around policy analysis and advocacy (as opposed to primarily academic and technical research).

Other funders in this space

There are a number of foundations and other funders that support economic research:

- The National Science Foundation spends roughly \$40 million a year on grants for economic research (most of which is not for macroeconomics). Grants are peer reviewed and generally chosen for scientific merit rather than policy impact.
- The Institute for New Economic Thinking (INET), which was initially funded by a \$50 million, 10-year grant from George Soros, aims to support new approaches to economic research.
- The Washington Center for Equitable Growth, which focuses primarily on inequality and growth rather than fiscal or monetary policy, expects to fund around a million dollars a year of research, with significant attention to policy impact.
- The Russell Sage Foundation and the Alfred P. Sloan Foundation have both supported some economic research on the impact and policy implications of the Great Recession.

Funders supporting policy advocacy tend to focus less on the business cycle per se and more on fiscal policy, labor standards, and wider debates about the appropriate size of

government. For instance, foundations that have recently provided major support to both the Economic Policy Institute and the Center on Budget and Policy Priorities (CBPP) include:

- Annie E. Casey Foundation
- Atlantic Philanthropies
- Ford Foundation
- Open Society Foundations
- Rockefeller Foundation
- W.K. Kellogg Foundation

Other fairly large funders who support some work on economic policy, broadly understood, include:

- Peter G. Peterson Foundation, which predominantly focuses on efforts to limit the federal budget deficit.
- Sandler Foundation, which supports the Center for American Progress, CBPP, and the new Washington Center for Equitable Growth
- MacArthur Foundation
- Stoneman Family Foundation

As far as we know, none of these funders have programs explicitly aimed at promoting effective monetary policy or countercyclical fiscal policy, but in practice there may be significant overlap. We see the most current overlap with the Ford, Open Society, Rockefeller, and Sandler Foundations.

Landscape

The three sub-areas discussed above (monetary policy, fiscal policy, and recession prevention) have fairly different landscapes.

Within monetary policy, the key actor is the Federal Reserve, which is relatively technocratic and tends to be run by academic economists. The financial industry pays great attention to the Fed, as do other academic economists and some policy commentators and journalists. When the Fed has taken unconventional measures (such as recent asset purchases), it has come under attack from conservatives and libertarians who are worried that such actions might debase the currency and cause runaway inflation. While such reactions provide some bounds on the Fed's authority, it has relatively wide latitude to set policy as it sees fit, typically in accord with the policy views of mainstream macroeconomists. There's a large community of such economists, who interact with the Fed through a variety of formal and informal channels.

Fiscal policy is driven primarily by Congress, and is both more controversial and more explicitly political than monetary policy. Debates tend to break down along traditional partisan lines, with Democrats and left-leaning economists supporting expansive government responses while Republicans and right-leaning economists oppose them. One idea that seems not to have received as much attention is the notion that regardless of the overall level of government spending, it's generally better to have it be countercyclical

(that is, expanding in recessions and shrinking in expansions), so policies that increase countercyclicality while being budget-neutral over the long run could have potential. Think tanks and interest groups across the spectrum tend to be well-represented in debates about budget issues and the overall size of government, but less engaged in explicitly arguing for countercyclical fiscal policies, including automatic stabilizers (which change in response to the business cycle without requiring ongoing Congressional action).

Financial regulation was a top agenda item right after the recent financial crisis, but has fallen from view somewhat since the passage of the Dodd-Frank financial reform bill. Nonetheless, economists have continued to study the issue, and there seems to be an increasing consensus that reforms have not gone far enough (though the question of what an optimal policy would be has not necessarily been resolved). Most possible reforms would have large (and presumably negative) effects on the bottom lines of banks and other financial institutions, and accordingly would face a long uphill battle in Congress at a time when the issue seems to largely be off the agenda. However, public opinion is still not particularly favorable to big banks, and populist feelings seem to remain strong, so there may be an opportunity to elevate the issue. There are a couple of existing groups advocating for more stringent regulation that we perceive as well-regarded (Americans for Financial Reform, Better Markets).

Current Grantees on this topic

We have made two exploratory grants to date:

- [\\$335K over 28 months to CBPP's Full Employment Project](#). This grant covers a third of CBPP Senior Fellow Jared Bernstein's project to draw attention to the importance of full employment and promote policies that to support it. The project includes CBPP efforts to assess the performance of automatic stabilizers in the Great Recession and generate proposals for improvements, along with suggestions for what might be included in future stimulus bills.
- [\\$100K to the Center for Popular Democracy's Full Employment Campaign](#). CPD's campaign aims to mobilize workers and community advocates to draw attention to the importance of monetary policy in driving economic outcomes and to advocate to the Federal Reserve to hold off on interest rate increases until the employment and wage situation further improves. We're currently considering whether to offer additional support for the campaign.

Though these grants both focus on the “full employment” angle to macroeconomic policy, we don't necessarily see that as more promising than other approaches.

Possible Sketch of a Strategy on this Topic

We can image a few different strategic approaches, and don't have terribly strong views on which we are most likely to pursue.

Improving the communication of economic research to policymakers and the public.

Although economists often disagree about policy decisions, agreement may be more widespread than policymakers and the public are typically led to believe. Better disseminating points of agreements among economists might, in the long run, lead to better economic policy. To support the development and dissemination of points of consensus, a foundation could fund:

- Surveys of economists, perhaps along the lines of the IGM Surveys conducted by the University of Chicago. In addition to conducting the surveys, a funder could support better dissemination and press outreach efforts.
- Research summaries by credibly nonpartisan institutions, perhaps modeled on the Congressional Budget Office.
- Training or support for economists aiming to communicate their work to a policy audience.
- Support for journals, such as the Brookings Papers on Economic Activity, which focus on policy-relevant questions and aim to have some influence.
- Business groups to convey points of economic consensus to a conservative audience.

Support think tanks and advocacy groups promoting full employment and countercyclical fiscal policies as a goal and producing ideas about how to get there.

One version of this approach would be to fund work on these policy goals by left-leaning think tanks and advocacy groups. Potential grantees could include CBPP, Brookings, the Urban Institute, the Economic Policy Institute, Demos, or New America, amongst others. Projects could include work on:

- Automatic stabilizers like extended unemployment insurance or automatic aid to states during recessions.
- Effective and politically plausible stimulus packages for future recessions
- Proposals for a technocratic independent body to guide countercyclical fiscal policy, perhaps by adjusting payroll tax rates.

Our existing support for CBPP's Full Employment Project offers a reasonable example of what we envision future work on this approach might look like, and the Center for Popular Democracy campaign around the Federal Reserve is a specific instance of what “full employment” advocacy might look like.

Another approach to this goal might be to try to move full employment or countercyclical fiscal policy into the political mainstream in the same way that efforts to reduce the budget deficit currently are. This might involve combining support for center-left and center-right think tanks and advocacy groups, perhaps broadly modeled on the example of the Peterson Foundation's work on the budget deficit. It also might involve outreach to business groups to marshal their support for such policies (which could be in their interests). Given the comparatively long history of bipartisan concern with budget deficits, this approach seems relatively unlikely to be successful, but some recent conservative engagement with the need for stimulative monetary policy suggests that there might be room for progress.

Support research or advocacy around financial regulation and recession prevention.

A philanthropist could support:

- Efforts to advocate for higher bank capital ratios, perhaps by supporting work along the lines of Anat Admati's research.
- Policy proposals to change the structure of long-term debt, particularly household mortgages, and encourage more equity-like financing. These proposals might also extend to efforts to remove the current bias in the tax code that favors debt over equity.
- An “early warning” system for recessions at a think tank might focus on monitoring financial indicators and raising alarms when risks are elevated. This might not be necessary if the Federal Reserve and other regulators are already paying sufficient attention to emerging risks, and it also might not be possible to do well, since presumably many financial actors have strong incentives to identify and avoid threats but fail to do so.

Potential grantees might include advocacy groups like Americans for Financial Reform and Better Markets or think tanks like the Peterson Institute for International Economics or Brookings. With many of these issues seeming to be off the agenda for the moment, a foundation might focus on developing policy proposals so that they are ready to be acted on following a future financial crisis.

Overall, we tend to think that recessions have been quite persistent through different economic policy regimes and that accordingly it may be more difficult than typically believed to prevent them in the future, but many of these possibilities nonetheless seem credible and interesting.

Cross-cutting interventions to improve economic research. Improving academic economics might involve one or both of two contrasting approaches: increasing practical and policy-relevant research as opposed to further theoretical work, or supporting methodological advances that will allow further progress on a variety of questions.

Supporting policy-relevant research might involve:

- Summer programs to bring young economists up to speed on the state of a field and encourage them to pursue policy-relevant research in it.
- Increasing the representation of women and people of color in economics. (Historically, members of these under-represented groups tend to be more likely to take on policy roles over the course of their careers.)
- Supporting awards for policy-relevant macroeconomic research or public communication.
- A fellowship or sabbatical program to fund a semester or year of paid leave for young macroeconomists interested in policy-relevant research.
- Funding economics PhD graduate programs to train more students.
- Supporting a conference or an edited volume on a topic of particular interest.
- Conventional research grants, similar to those provided by the National Science Foundation to economists, though we presume these are already allocated

relatively appropriately.

Funding novel methodological approaches might include supporting:

- Novel data collection efforts, whether qualitative (such as Alan Blinder or Truman Bewley's research on wage stickiness), quantitative (such as the Billion Prices Project), or historical (such as digitizing archival bankruptcy records).
- Experiments with macroeconomic phenomena within large-scale multi-player online games.
- Agent-based models to develop a more sophisticated understanding of the impact of various economic policies.
- Incorporating approaches from other disciplines, such as economic history, into macroeconomic research.

Because these interventions are relatively long-term and speculative, it's challenging to assess their value and to compare their merits.

Risks and reservations about this cause:

- Relative to other policy areas, the disagreements between smart people who have similar values seems unusually strong on macroeconomic policy. Accordingly, it is difficult for us to be as confident that we're on the right side of these disputes as we are in some other policy areas.
- Due to the large amount of research that is already being done in these areas, it may be particularly challenging to determine which further research would be productive and to assess that research's impact on the overall dialogue. This issue already receives a great deal of attention from academics and technocrats. We believe there are gaps, particularly where it comes to more directly policy-relevant work and advocacy, but it is hard to determine how important these gaps are.

Further questions for discussion

- Our three-pronged goal (better monetary policy, countercyclical fiscal policy, recession prevention) and the range of strategies considered seem too large for a single program area. How should we narrow our focus to something more manageable?
- Which strategies described above seem most promising? Are there others we should we consider?
- How likely does the prospect of bipartisan agreement around the ideas of full employment or countercyclical fiscal policy as goals seem?
- Does it make sense to separate support for countercyclical or full employment policies from broader support for a progressive economic agenda? Is there a good reason most discussions about macroeconomic policy tend to take place in the context of debates over the size of government?

Alcohol policy
2:15 session

Goal: To reduce the negative impacts of excessive alcohol consumption and raise revenue by increasing alcohol excise taxes.

Amount of Work that We Have Done on this Issue So Far: We've had between 5 and 10 conversations with academics and practitioners.

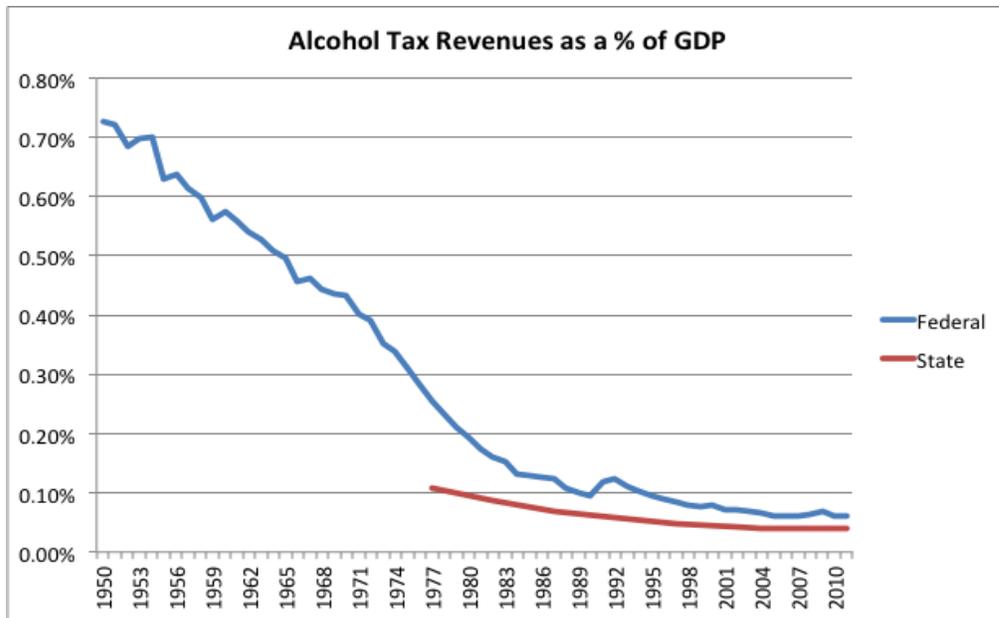
What Changes We Seek

According to the Centers for Disease Control and Prevention (CDC), excessive alcohol consumption (roughly defined as any underage drinking, more than 5 drinks in one session, or more than an average of two drinks a day) accounts for ~88,000 deaths and more than \$200 billion of social costs in the United States each year. The mortality estimate is about a fifth of the CDC's estimate for mortality due to smoking, but the social cost estimates are nearly equivalent. Alcohol accounts for more deaths and (estimated) social costs than any illegal drugs, and plays a role in a large proportion of violent crime.

According to the CDC, there are a variety of evidence-based approaches to reducing excessive alcohol consumption, including increasing alcohol taxation, maintaining limits on days or hours of sale, and reducing outlet density. We have focused the most on tax policies, which have the benefits of raising revenue and being relatively easy to enforce.

Both state and federal governments have alcohol excise taxes, and both have been substantially eroded over the past 60 years:⁷

⁷ Open Philanthropy Project calculation based on Tax Policy Center data, available at <http://www.taxpolicycenter.org/taxfacts/displayafact.cfm?Docid=399> and <http://www.taxpolicycenter.org/taxfacts/displayafact.cfm?Docid=561>



Federal excise taxes on alcohol were last increased in 1991, and have declined significantly in real terms since then. The CDC's estimate of the social cost of excessive alcohol consumption amounts to more than a dollar a drink, while current state and federal taxes are below ten cents a drink (and proposals for increases are usually for less than an additional dime a drink). Although alcohol taxes are regressive, excessive drinking accounts for a large portion of total consumption, so price increases would likely be reasonably well-targeted.

Why We Are Interested in this Cause

We became interested in this topic because Mark Kleiman, one of our criminal justice reform grantees, told us that excessive alcohol consumption was the most important public policy issue he knew of with no major advocate and that increasing alcohol taxes would likely reduce crime rates. While we have not further investigated the connection between alcohol and crime, our initial investigation of this area has confirmed that there appear to be substantial social costs to excessive alcohol consumption and that there is little engagement around alcohol taxes by foundations or advocates. This is particularly notable in light of the helpful example set by the Robert Wood Johnson Foundation's successful efforts to drive public policy to reduce smoking and the success of a couple of recent state-level campaigns for increased alcohol taxes.

Other funders in this space

We are not aware of any U.S. foundations with a program in this area, though some foundations support some related work (e.g. RWJF might fund some alcohol policy work in its general program on health policy research). The CDC funds a few million dollars a year of research and advocacy on alcohol policy, and there are a number of other federal funding streams for research on alcohol disorders and community efforts to fight drug and alcohol abuse.

Landscape

We have no particular reason to believe that the time is particularly ripe for reform in this area, but with alcohol excise taxes gradually diminishing in real terms at both the state and federal levels, it's possible to envision work at either level.

One example of a prospect to exploit the current political environment is a successful 2011 campaign to raise alcohol taxes in Maryland. The campaign was carried out by a coalition of public health and progressive advocacy groups around the combined goal of reducing excessive alcohol consumption and increasing revenue. Many of the coalition members had previously mobilized together to support the Affordable Care Act.

The alcohol industry is the biggest source of opposition to increases in alcohol taxes, and is perceived as influential in many state legislatures. In addition, some Republicans and libertarians are likely to oppose any tax increases.

Existing substance abuse treatment and health access groups may prove to be powerful allies, but we have not investigated this closely. Our understanding is that Mothers Against Drunk Driving tends to focus on technological solutions to drunk driving, and does not have an official position on alcohol taxes.

We do not have much sense of where public opinion stands, though our understanding is the polling in Maryland found fairly strong support for a dime a drink tax increase.

Possible Sketch of a Strategy on this Topic

Because our investigation to date in this area remains fairly preliminary, we are well short of a detailed strategy. Nonetheless, we see this as an interesting area and a potential candidate for a relatively small engagement, perhaps with one or a few grantees.

The model foremost in our mind for a strategy in this area is to focus on supporting state campaigns to raise alcohol taxes, likely through a national intermediary that could subgrant to the campaigns and provide research, communications, and policy support. This is a model that the Robert Wood Johnson Foundation has used for campaigns on a number of health policy issues (including some former work on drugs and alcohol).

State campaigns carry the attractive prospect of building momentum for a national increase in alcohol taxes while themselves appearing plausibly quite cost-effective in terms of improved health outcomes and revenue. They also avoid much of the gridlock that seems reasonably likely to hold up Congressional policymaking for the foreseeable future, though it may be possible that alcohol tax increases would be sufficiently minor not to become a major partisan issue.

In addition to or instead of a national intermediary that supports state campaigns, we could also support:

- Groups pursuing a litigation strategy, similar to early efforts against tobacco

companies. We think this is unlikely to be comparably successful to the tobacco efforts, but it could plausibly be useful. Our understanding is that part of the benefits of the early tobacco litigation was that the discovery process yielded a trove of documents containing statements that were damning in public, whether or not they represented criminal activity. The change in public attitudes engendered by the documents may have played a role in the increased willingness to raise tobacco taxes.

- National advocacy organizations that might play more of an “inside game” at the federal level. This could include budget-focused organizations (e.g. the Center on Budget and Policy Priorities, which advocated increasing alcohol taxes as a partial financing mechanism for the ACA) or health-focused ones (e.g. the Center for Science in the Public Interest, which had a program on alcohol policy until funding dried up a few years ago).
- Other mechanisms for coordinating state efforts. This could include nonpartisan efforts to connect and inform state policymakers (e.g. work by the National Conference of State Legislatures or the National Governors Association), or more explicit advocacy networks separate from an intermediary (e.g. the new U.S. Alcohol Policy Alliance).
- More research on potential harms of alcohol consumption and policy analysis around politically plausible ways to reduce those harms. This may include supporting more work on the role of alcohol in crime.

Overall, we see this area as being primarily attractive based on the potential of state campaigns, though we still know relatively little about where conditions might be appropriate for such campaigns or how likely they might be to succeed.

Risks and reservations about this cause:

We have not yet systematically reviewed the evidence for a number of important parts of our case:

- The CDC's estimates of mortality and social costs due to excessive alcohol consumption are high, and seem reasonably likely to be overstated, perhaps by a wide margin.
- Although the intuitive case that raising alcohol taxes will reduce consumption is strong, estimates in the academic literature for the elasticity (which measures the magnitude of that relationship) vary pretty widely, so it is not clear whether we should expect the main impact of tax increases to be increased revenue or reduced social costs.

Even if raising alcohol taxes does reduce consumption, there's a question about whether it does so by reducing the consumption that accounts for social costs (i.e. consumption by excessive drinkers) or (other) non-problematic consumption. Our impression is that excessive drinkers account for most alcohol consumption and are likely to be deterred by higher prices, but we have not investigated this question closely.

Further questions for discussion

- Are there better approaches to the policy goal of raising alcohol taxes than running state campaigns?
- How should we think about the “momentum” dynamics around state policy wins? Do state tax increases raise the probability of a federal win?
- What other interest groups or advocates might overlap, positively or negatively, with efforts in this area?

Land use reform

2:15 session

Goal: To liberalize land use regulations so that more housing can be built in high-demand regions, improving affordability, opportunity, and growth.

Amount of Work that We Have Done on this Issue So Far: We've had between 5 and 10 conversations with academics and practitioners.

What Changes We Seek

Zoning and other land use restrictions in some of America's most productive and desirable regions inflate housing costs, contributing to the dislocation of some longtime residents and keeping potential migrants out, which reduces long-run growth. We would like to see zoning and other land use regulations loosened so that they better incorporate the genuine social costs and benefits of additional construction (as opposed to creating artificial scarcity to benefit current owners and occupants).

Even if some groups of citizens were more strongly mobilized in favor of land use reform, local authorities interested in liberalizing zoning restrictions are likely to face opposition from residents opposed to allowing additional construction (which could lower their house prices). One potential approach to address the expectation of widespread local opposition might be to push for zoning decisions to be made at a higher administrative level (e.g. region or state), which tends to lead to less restrictive policies. Another approach might be to encourage municipalities to set “zoning budgets” that better align the political incentives of those who advocate for specific additional developments and ensure that downzonings are offset by upzonings in accord with an overall plan.

According to research by UC Berkeley economist Enrico Moretti, a handful of regions-- Boston, New York, the San Francisco Bay Area, Seattle, and Washington, D.C.--account for the majority of the negative impact of restrictive zoning on growth, so it may be appropriate to focus attention on those regions.

Why We Are Interested in this Cause

Importance: Overly restrictive land use regulations drive up housing prices and prevent construction of housing that people would like to live in and developers would like to build (that is, they cause deadweight loss). We don't know of any national estimates of the total amount of these costs, but extrapolation from Harvard economist [Ed Glaeser's regional estimates](#) suggests that they might amount to around \$25 billion/year in deadweight loss. To the extent that these price distortions prevent people from moving to more productive regions (which is plausible but disputed), there may be substantially larger second order effects on economic growth. The work by Enrico Moretti cited above estimates that these costs to growth could add up to more than a trillion dollars a year, though there are reasons to doubt his model (which assumes that the majority of the U.S.

population would eventually move to the five highly productive regions). In addition to slowing growth, tight housing supply regulations likely reduce economic mobility: the five regions that Moretti identifies as most importantly constrained by land use regulations are five of the top six metro areas for economic mobility according to research by Raj Chetty and Emmanuel Saez.⁸

Lack of other funders: In spite of what we perceive as the importance of this cause, there's a notable lack of advocates or funders. We became interested in this area because of writing by a number of journalists and academics, but don't see much in terms of organized efforts to address this issue. That said, the issue seems to offer a somewhat unusual degree of potential for transpartisan agreement: thinkers and writers from the both the left ([Larry Summers](#), [Matt Yglesias](#)), right ([Ed Glaeser and Joseph Gyourko](#), [Reihan Salam](#)), and center ([the Financial Times](#)) have written in support of reform (though of course these individuals all share a certain technocratic bent).

Other funders in this space

A number of foundations fund work on housing policy, especially for low-income residents, and urban spaces, but there seems to be relatively little attention to land use regulation in particular:

- The Ford Foundation has a ~\$25 million/year Metropolitan Opportunity program that primarily focuses on affordable housing, transit-oriented development and smart growth. In 2014, roughly \$7M has gone to a strategy around “land use innovation” that includes a number of efforts to support affordable housing measures in “thriving” regions and redevelopment efforts in struggling ones.
- The MacArthur Foundation has a ~\$14 million/year housing program that generally focuses on affordable housing and the impact of housing on education, health, and economic opportunities.
- The Knight Foundation has supported some work around affordable housing, smart growth, and “lean urbanism” in some of the cities in which they work.

In addition, the Rockefeller Foundation does some related work on resilience and public transportation, the Surdna Foundation does some related work on green building in cities, and numerous other foundations are involved to some extent, particularly in their local communities.

It appears to us that none of these efforts have a significant component that is focused on liberalizing regulation and increasing overall supply of housing and construction.

Landscape

In most cases, states have the authority to regulate land use decisions but delegate it to

⁸ We've collapsed the San Jose and San Francisco metro areas throughout this discussion. The other top-6 high-opportunity metro area is Salt Lake City. <http://www.equality-of-opportunity.org/>

local authorities, which have varied arrangements for policymaking and varied arrays of interest groups.

Two large groups of progressives could conceptually be allies for efforts to liberalize land use, though their typical current stances on the issue range between neglect, ambivalence, and outright hostility:

- Affordable housing advocates. In cities like San Francisco, the gap between housing that is affordable for low-income families and the cost of new construction (not to mention market rents) is sufficiently wide that a small amount of new construction is relatively unlikely to relieve cost pressures on low-income renters, who are the main focus of affordable housing advocacy. However, in some other regions, the benefits to low-income renters may be sufficient to attract support from affordable housing advocates, and it might be possible to draw advocates' support for policies that will improve middle class affordability. Making this case could be an uphill battle.
- Environmental advocates. Dense, walkable, transit-oriented development, which is more likely with looser zoning restrictions, reduces environmental footprints. There are numerous groups that promote “smart growth” under that rubric (including Smart Growth America, the Council for New Urbanism, and several national and local advocacy groups), though they have a wider agenda and do not necessarily support straightforward loosening of restrictions. On the other side, some environmental advocacy groups can be quite concerned with narrow local interests (such as protecting parks, trees, or preventing the short term costs of development) and accordingly oppose changes that would carry net global environmental benefits.

There are a number of other groups that could conceptually support efforts to promote urban densification, though we know little about the extent to which they do:

- Real estate developers vocally support development, though their efforts tend to be fairly narrowly focused on individual major developments that yield large profits, as opposed to broad-based upzonings.
- Construction and building trades workers who would benefit from job growth in new construction.
- Public employee unions which would benefit from increased tax bases and city revenue growth.

Opposition tends to be locally intense but not necessarily nationally or regionally organized. Many conservatives ideologically favor suburban development (which depends to some extent on land use controls), but there appears to be some nascent “reform conservative” and libertarian support for land use liberalization, including the likes of Reihan Salam and R Street.

There are also a number of relatively nonpartisan research institutions that work on housing and land use policy nationally, including Harvard's Joint Center for Housing Studies and NYU's Furman Center.

Another force to consider are a few specific local institutions:

- Urban planning and good government groups. The San Francisco Bay Area has SPUR, a broadly “smart growth” coalition of businesses, citizens, and civic leaders. New York has two similar groups, the Municipal Art Society and Regional Plan Association.
- City or neighborhood urbanist and transportation blogs, which seem to have a sizeable readership in many of these regions, but relatively little support or capacity. DC has Greater Greater Washington; many cities have Streetsblogs. New York has a fairly new one – New York Yimby (short for Yes In My Back Yard).

Possible Sketch of a Strategy on this Topic

Given the paucity of existing organized work in this area, we've struggled to develop plausible strategies that we could readily support.

A few preliminary ideas we've considered include:

- Seeding a number of local advocacy groups to build coalitions around removing land use restrictions. However, we're wary of this strategy because it seems likely that there's a reason (other than a lack of foundation funding) that has prevented such groups from emerging before, and we're not sure how to source them.
- Supporting pilots or advocacy around micro- and inlaw units. Although these wouldn't solve the fundamental land use problem, they're on the political agenda in San Francisco, Seattle and New York to some extent, and it's plausible that demonstrations could have an impact.
- Conferences to bring together economists, lawyers, planners, and architects who have shared interests in these topics, aiming to build a research or advocacy agenda. In particular, there may be room for practical research around the political economy of reform proposals and which interest groups might be mobilized.
- Providing support to “smart growth” groups to focus more on the benefits of relaxing land use restrictions.
- Supporting offshoots of the affordable housing or environmental movements aimed at marshaling their support for looser land use regulations.
- Funding think tank activities by libertarians and “reform conservatives” to try to grow support for liberalizing land use policies on the right.
- Funding a campaign to move zoning power to the state level in one state or implement a “zoning budget” in one city. Given the current level of research on and support for these policies, this seems premature, and, again, we're not sure how we would source qualified leaders who were devoted to running such campaigns.

Overall, we remain quite uncertain about how to approach strategy in an area that has some serious intellectual attention but little movement on the ground.

Risks and reservations about this cause:

- As noted above, there aren't obvious grantees at this time, and it isn't entirely clear how to build the field.
- It's not clear whether efforts to liberalize existing land use regulations have a plausible chance of succeeding. Doing so is likely to involve large changes in the value of some households' most important assets (positively in some cases, negatively in others), and opposition may be too strong to overcome. (On the other hand, the overuse of land use restrictions has been getting worse over time, and merely keeping things at the current level of stringency may be quite valuable.)
- The channels through which land use restrictions affect economic growth are not necessarily well-identified, and the estimates that we've seen and based our views on could be significantly overstated. The role of land use policy, as opposed to other forces, in driving up prices is also not perfectly clear although the evidence is somewhat stronger.
- Working on this issue in San Francisco could bring more backlash and personal attacks than working on most other causes would.

Further questions for discussion:

- What other interest groups come to mind as potentially being engaged in this issue?
- Which strategic approaches—whether the ones we've considered above or other suggestions—are most likely to lead to long-term progress?
- How should we source potential grantees in a field where there seems to be a real lack of organized effort around the policy goals we see as important?
- How should we think about the value of national advocacy organizations in the context of an issue where most of the benefits to changes are drawn from a few specific regions?

Income security

3:15 session

Goal: Increasing the level and predictability of income and social insurance for low- and middle-income Americans.

Amount of Work that We Have Done on this Issue So Far: We have had around 20 conversations with academics and advocates.

What changes we seek, and why

This area is substantially larger than many of the others we have considered in terms of foundation and interest group engagement and the number of sub-issues. As a result, we have not explored potential changes in the same level of depth. Some of the changes that we could envision supporting include:

- Stronger enforcement of labor law to reduce wage theft
- Increasing state or national minimum wages
- Universal early childhood education or other forms of subsidized childcare
- State paid family leave policies
- Mandatory provision of paid sick days
- Apprenticeship programs to facilitate job training
- Expanding the Earned Income Tax Credit for single people and eliminating the marriage penalty
- Experiments with changing disability insurance to provide greater support for work
- Longer-term efforts to address the possibility that technological progress requires substantial changes to the welfare state, e.g., by further developing universal basic income or capital taxation policy ideas.

This is not meant to be an exhaustive list; there are certainly other policy changes we would support in this area.

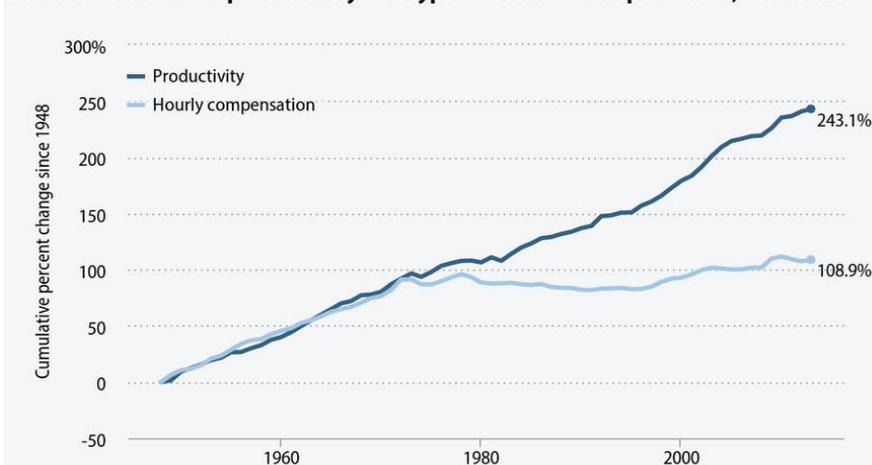
Broadly speaking, we support these policies because we believe that the welfare-maximizing level of taxes and transfers is substantially higher than current levels, and we see them as some of the more politically palatable ways of increasing incomes for lower-income Americans.

Why We Are Interested in this Cause

Between 1948 and the mid 1970s, productivity and wages rose in lockstep, but since then wages have risen far more slowly:⁹

⁹ Chart is from the Economic Policy Institute, based on BLS and BEA data: <http://www.epi.org/chart/bp382-figure-disconnect-productivity-typical/>

Disconnect between productivity and typical worker's compensation,* 1948–2013



Other ways of measuring income, including those that adjust for family size, include healthcare and other benefits, and incorporate taxes and transfers, find less extreme results but generally support the notion that median income growth has not kept up with economic or productivity growth (or the incomes of the top 1%). Based on CBO figures for 2010, which find less of an increase in inequality than other metrics, returning the top 1% of the income distribution to its 1979 proportion of after-tax income would involve redistributing around half a trillion dollars of annual income.¹⁰ Of course, income for the top 1% has risen partly for non-policy reasons (e.g. skill biased technological change, assortative mating), and there is no reason to believe that any of the policies described above would return it to 1979 levels. On the other hand, there's no particular reason to believe that 1979 levels represent optimal redistributive effort.

In the past, we've guessed that this area would be relatively crowded because of its partisan salience and accordingly thought that there would be few opportunities for more funding to make much of a difference. However, conversations about potential state-level action around some of these issues have led us to believe that there may remain substantial room for investment in reasonably “winnable” issue campaigns, leading us to reconsider our overall assessment.

Other funders in this space

The Grantmakers Income Security Taskforce, a funder affinity group, includes the following foundations, amongst others, on its steering committee:

- Annie E. Casey Foundation. AECF's [financials](#) indicate that it spent ~\$18M on

¹⁰ Based on <http://www.cbo.gov/sites/default/files/44604-AverageTaxRates.pdf>. The CBO estimates that average after-tax income for the top 1% of households in 2010 was just over \$1M/year, and that it had tripled since 2010, while after-tax income for the rest of the distribution had risen by about 50%. With about 1.2M households in the top 1%, a reduction to 150% of 1979 levels (in line with the rest of the income distribution) would involve redistributing roughly \$600 billion/year.

“poverty” in 2013.

- Charles Stewart Mott Foundation. In 2012, Mott [made](#) \$3M in grants for income security, \$5M in grants for training, and \$5M in grants for building community organizing infrastructure.
- Ford Foundation. So far in 2014, Ford has spent about \$20M on [these issues](#) in the United States.
- Stoneman Family Foundation. In 2012, Stoneman [made](#) a total of around \$6M in grants, not all on these issues.
- W.K. Kellogg Foundation. In 2013, Kellogg [made](#) around \$5M of grants in this area.

All of these foundations have supported the Center on Budget and Policy Priorities, which we see as a fairly good representative of the kinds of policies we might support in this area, as have the MacArthur, Open Society, Public Welfare, Rockefeller, and Sandler Foundations and Atlantic Philanthropies.

We're far from having conducted an exhaustive tally, but we would guess that foundations make more than \$100 million/year of grants to support advocacy (broadly defined) in this area.

Landscape

Although much of income security policy is national in scope, and therefore difficult to envision making progress on in the near term, there may be opportunities for substantive state and local policy initiatives in some areas, such as wage theft, minimum wages, paid family and sick leave, apprenticeships, and early childhood education. Many people we've spoken to believe there is currently an “inequality moment” that could be capitalized on in proposing such policies.

While we're still particularly early in this investigation of this area and can't speak with much confidence, some of the major national groups promoting one or more of these policies seem relatively small:

- National Employment Law Project, which plays a major role in minimum wage and wage theft discussions, [spent](#) ~\$6M in 2012, including ~\$1M it regranted.
- Family Values @ Work, a coalition working on paid family leave and paid sick days, had [expenses](#) of ~\$3M in 2012, including ~\$2M it regranted.
- National Partnership for Women and Families, another organization that works on paid family leave and paid sick days, amongst other issues, [spent](#) around \$7M in 2012.

Of course, these organizations work as part of broader coalitions that have more funding, but it nonetheless seems to us that they could plausibly benefit from greater scale. We've heard a few claims that similar dynamics apply to state issue campaigns, which aren't necessarily being run everywhere they could plausibly win.

Many of the policies we might consider in this area are reasonably popular with the

public, but face opposition from conservatives, libertarians, and business interests. Our impression is that historically, expansions of the welfare state that help people in a variety of income brackets in the U.S. have been typically controversial when they were enacted but became popular relatively quickly once they were in place, preventing roll-back.

Possible Sketch of a Strategy on this Topic

We can imagine a few different strategic approaches to working on these issues, focused on funding:

- **State issue campaigns funded through national intermediaries.** These could be attractive for many of the issues within this area (e.g. minimum wage, wage theft, paid sick days, paid family leave) when it appears that a given campaign could cost-effectively increase income security. Winning state campaigns may also help advance the national case for a given policy change.
- **Cross-cutting state or local advocacy infrastructure.** This could include general or issue-specific support for think tank networks (like CBPP's State Priorities Partnership or EPI's Economic Analysis and Research Network), groups for legislators (like the State Innovation Exchange, the newly formed liberal alternative to ALEC), or community organizing groups or organizations providing support to community organizing groups (like the Center for Popular Democracy or Center for Community Change). It could also include efforts to mobilize newer constituencies, such as small business owners (like the Main Street Alliance), to support some of these policies.
- **Cross-cutting national infrastructure.** Broadly speaking, the national infrastructure supporting income security issues seems to be better-funded than the state advocacy infrastructure, but we could imagine finding specific issue or organizational gaps in which additional funding might prove particularly useful.
- **Long-run efforts to address the possibility that technological prospects undermines existing income security arrangements.** This approach is quite different from the others in that it does not necessarily aim to affect policy in the next ten years, but to address the possibility that there are opportunities for research or preparation now that would be helpful if it becomes apparent in the future that technological progress is fundamentally changing the economy. The most obvious question is how the welfare state, which is currently largely reliant on work support, might respond to widespread technological disemployment (e.g. by replacing the EITC with a basic income), but another important question would be on the revenue side, since most federal revenue current comes from income and payroll taxes. It is not clear to us what could be done to address these questions, but we could imagine supporting academic research to help raise their profile.

Risks and reservations about this cause:

- Although many of the other issues we've considered have a partisan valence, this

issue, the size of the welfare state, seems to be virtually the core dispute of contemporary American politics. Taking a strong stance here seems like might undermine our ability to have an impact in other areas.

- It is unexpected to us that there would be room for additional advocacy funding to make a big difference in this area—given its partisan salience—and accordingly we wonder whether we've missed a large portion of the advocacy infrastructure pushing these policies forward.
- Technological disemployment has been predicted regularly for decades without having occurred. There may be good reasons to believe this time could be different, but seems unreasonable to be confident that it will be, and it's very difficult to know whether any activities undertaken to prepare for such a risk would still be useful if it fails to come to pass.

Further questions for discussion

- How do state issue campaigns help advance national policies? Does it seem plausible that there are states where minimum wage or paid sick days campaigns would win but aren't being run?
- What other strategic approaches should we consider? How should we assess the ones discussed above?
- Are there activities that might be helpful today to increase the likelihood that, if technological progress does lead to widespread job loss, we respond reasonably well?
- Many income security policies fail to pass primarily because low-income people are not a strong political force. If we want to work on this cause, to what extent should we be looking to build the power of low-income people as a political force, independent of specific policy campaigns? What would be the drawbacks to doing so?

Breadth vs. depth in choosing focus areas

4:00 session

Below is a blog post that we ran in October, laying out our current tentative thinking on trading off breadth vs. depth in philanthropy.

Expert philanthropy vs. broad philanthropy

OCTOBER 16, 2014

By Holden

It seems to me that the most common model in philanthropy – seen at nearly every major staffed foundation – is to have staff who specialize in a particular [cause](#) (for example, specializing in [criminal justice](#) policy). Often, such staff have a very strong background in the cause before they come to the foundation, and they generally seem to focus their time exclusively on one cause – to the point of becoming (if they weren't already) an expert in it.

I think this model makes a great deal of sense, partly for reasons we've [discussed previously](#). Getting to know the people, organizations, literature, challenges, etc. most relevant to a particular cause is a significant investment – a “fixed cost” that can then make one more knowledgeable about all giving opportunities within that cause. Furthermore, evaluating and following a single giving opportunity can be a great deal of work. Now that the [Open Philanthropy Project](#) has made some early grants, it is hitting home just how many questions we could – and, it feels, should – ask about each. If we want to follow each grant to the best of our abilities, we'll need to allocate a lot of staff time to each; having staff specialize in causes is likely the only way to do so efficiently.

Yet I'm not convinced that this model is the right one for us. Depth comes at the price of breadth. With our limited [management capacity](#), following each grant to the best of our abilities shouldn't be assumed to be the right approach. I've been asking myself the question of whether there's a way to be involved in many more causes at a much lower level of depth, looking for the most outstanding giving opportunities to come along in the whole broad set of causes. I've been thinking about this question recently mostly in the context of [policy](#), which will be the focus of this post.

Having a “low-depth” involvement in a given issue could take a number of forms – for example:

- One might make a concerted effort to identify a small number of “big bets” related to an issue, and focus effort on following these “big bets.”
- One might make a concerted effort to identify a small number of “gaps” – aspects of an issue that get very little attention and have very few people

working on them – and focus grantmaking activity on these “gaps.” This approach could be consistent with making a relatively large number of grants in the hopes that some grantee gains traction.

- One might focus on identifying a trusted advisor in an issue space, and make a small number of grants as recommended by the advisor (this is largely the approach behind our [grants so far on labor mobility](#)).
- One might co-fund the work of another major funder, join a collaboration of major funders, or support the work of a large and established organization, and gain more familiarity with the issue over time by following this partner’s work.
- One might aim for a very basic level of understanding of an issue – in particular, which way we would like to see policy change relative to the status quo, and whom we feel aligned enough with to take their advice. With this understanding in hand for multiple issues, one might then be well-positioned to support: (a) “cross-issue” organizations and projects that are likely to have a small impact on many issues; (b) campaigns aiming to take advantage of short-term “windows of opportunity” that arise for various issues.

I can see a few arguments in favor of trying one or more of these, all of which make it possible to take some form of a “breadth” oriented approach (more causes, at with a lower degree of depth and expertise, than the standard cause-specialist approach would involve).

First and most importantly, **we will never know as much about grantees’ work as they do**, and it arguably makes more sense to think of grantees as the relevant experts. The best funder might be the one who picks qualified grantees in an important cause, supports them and otherwise stays out of their way. With this frame in mind, focusing on in-house expertise is arguably inefficient (in the sense that our expertise would become somewhat redundant with grantees’) and possibly even counterproductive (in the sense that it could lead us to be overly “active” with grantees, pushing them toward our theory of the case). Of course, picking qualified grantees is a serious challenge, and one that is likely harder without deep context. But the question is how much additional benefit deep context provides. Even without expertise, it is possible to get some signals of grantee quality – general reputation, past accomplishments, etc. – and even with expertise, there will be a great deal of uncertainty. In a high-risk model of the world, where perhaps 10% of one’s grants will account for 90% of one’s impact, it may be better pick “potentially outstanding” grantees from a relatively broad space of possibilities than to limit oneself to a narrower space, while having more precise and reliable ways of distinguishing between marginally better or worse giving opportunities.

Expertise would also be an advantage for following a grant, learning from it and continuing to help grantees as they progress. However, it seems quite possible to me that the best grantees tend to be self-driven and improvisatory, such that following them closely wouldn't add value to what they're doing, and would largely serve to assuage our own anxiety without doing much to increase our impact.

Secondly, **the best giving opportunities may sometimes cut across multiple causes** and be hard to assess if we've engaged seriously with only a small number of causes. This issue seems particularly important to me in the area of [U.S. policy](#), where the idea of strengthening the network of people who share our values – or the platform representing those values – could be very important. If we focus exclusively on a small number of policy areas, and give little attention to others, we could end up lacking the knowledge and networks to perform well on this goal, and we could be ill-positioned to evaluate the ramifications of a giving opportunity for the full set of issues we care about. (An argument for pursuing both breadth- and depth-oriented strategies simultaneously is that the depth-oriented work may surface opportunities that are relevant to a large number of issues, and the breadth-oriented work could then be helpful in assessing such opportunities.)

Finally, it seems to us that **there are some issue areas where the giving opportunities are quite limited** – particularly issues that we think of as [green fields](#), as well as neglected sub-areas of other issues. Devoting a full staff member to such an issue would pose particular risks in terms of inefficiency, and it might be better to fund the few available opportunities while waiting for more to emerge.

I think the cases of [Ed Scott](#) and the [Sandler Foundation](#) represent interesting examples of what a philanthropist can accomplish despite not specializing exclusively in a particular cause, and despite not building out a staff of domain experts.

- [Ruth Levine of the Hewlett Foundation writes](#) that Ed Scott has “built at least four excellent organizations from the ground up” – including the Center for Global Development, which we have [supported and think positively of](#). She adds that “Far more than many others seem to be able to do, he lets go – and as he does, the organizations he supports go further and faster than if he were holding on tight.”
- We know less about the Sandler Foundation, but it seems to have played a founding role in [several prominent organizations](#) and to be well-respected by many, despite not having staff who specialize in a particular cause over the long run. It does do deep cause investigations in sequence, in order to identify promising grantees, but staff work on new cause investigations even while maintaining their funding of previous causes and organizations; this approach

therefore seems distinct from the traditional foundation model and can be thought of as one approach to the kind of “broad” work outlined here. One of its core principles is that of looking for excellence in organizations and in leadership, and entrusting those it supports with long-term, flexible support (rather than continuously revisiting and revising the terms of grants).

In both cases, from what we can tell (and we are considering trying to learn more via [case studies](#)), a funder helped create organizations that shared a broad set of values but weren’t focused on a particular policy issue; the funder did not appear to become or hire a domain expert, and may have been *more* effective by being *less* hands-on than is the norm among major foundations. My point isn’t that these funders should be emulated in every way (I know relatively little about them), but that the “cause-focused, domain expert” model of grantmaking is not the only viable one.

I’m not yet sure of exactly what it would look like for us to try a breadth-emphasizing model, and I know that we don’t want this to be the *only* model we try. The depth-emphasizing model has much to recommend it. I can anticipate that, in some ways, a breadth-emphasizing model could be both genuinely risky and psychologically challenging, as we’d have a lower level of knowledge about our grants than many foundations have of theirs. But I think the potential benefits are big, and I think this idea is worth experimenting with.