Lecture Note 5: Segregation, Market Outcomes, and Individual Impacts

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1 Segregation, market outcomes, and individual impacts

Racial segregation is a salient feature of the economic geography of many nations, the United States among them. In the US, it is a particularly pronounced feature of urban areas, where many central cities are highly 'ghettoized' The 1999 paper by Cutler, Glaeser and Vigdor documents the rise and (partial) decline of the American ghetto. There are many important economic questions about segregation: 1) how do we measure it; 2) what causes it; 3) to what degree is it efficient or inefficient (that is, the benevolent social planner would or would not do it differently); 4) do ghettos harm residents by lessening their human capital acquisition, hampering economic opportunities (e.g., 'spatial mismatch'), or reducing their political efficacy

As always, we will only scratch the surface of these questions, while introducing you to the questions and the tools of the topic as well as highlighting some key results and pointing you towards further reading. The two main questions we will focus on are (1) what causes segregation and (2) how harmful is it for economic agents to grow up in segregated (or poor or 'bad') neighborhoods.

2 Measurement of segregation

There are many measures. Two that are commonly used are the Index of Dissimilarity and the Index of Isolation. The dissimilarity index is:

$$D = \frac{1}{2}\sum_{i=1}^{N}\left|\frac{black_i}{black_{total}} - \frac{white_i}{white_{total}}\right|,$$

where $black_i$ is the number of blacks in area i and $black_{total}$ is the total number of blacks in the city. This index ranges from zero to one and answers the question "What share of the black or white population would need to change areas for the races to be evenly distributed within a city?" Values between 0.3 and 0.6 are considered moderate, and above 0.6 is considered high.

This index doesn't tell us much about the degree of racial contact in a city. The isolation index, by contrast, attempts to measure the exposure of blacks to whites. The index starts with the percentage black of the area occupied by the average black

$$E = \sum_{i=1}^{N}\frac{black_i}{black_{total}} \times \frac{black_i}{persons_i},$$

where $persons_i$ is the total population of tract i. To subtract off the mechanical effect of black population share, one can further calculate

$$E' = E - \frac{black_{total}}{persons_{total}}$$

When there are few blacks, it's impossible for them to be completely isolated (given that the tract must be full). The minimum of E' is

$$\min(E') = \min\left(\frac{black_{total}}{persons_i^{\min}}, 1\right) - \frac{black_{total}}{persons_{total}},$$

where $persons_i^{\min}$ is the size of the *minimum* population area (e.g., tract). Rescaling E' by this minium, we get the isolation index:

$$I = \frac{\sum_{i=1}^{N}\left(\frac{black_i}{black_{total}} \times \frac{black_i}{persons_i}\right) - \frac{black_{total}}{persons_{total}}}{\min\left(\frac{black_{total}}{persons_i^{\min}}, 1\right) - \frac{black_{total}}{persons_{total}}}.$$

Cutler and Glaeser refer to an area as a ghetto if the dissmilarity index exceeds 0.6 and the isolation index exceeds 0.3.

Massey and Denton propose many other measures of segregation. And Echenique and Fryer propose a more sophisticated (though far harder to caculate) measure. One can only hope that conclusions about segregation don't turn too heavily on which measure is chosen.

3 Causes of Segregation

The modern analysis of segregation begins with Thomas Schelling, whose work on neighborhood 'tipping' is discussed (albeit briefly) in his Nobel prize citation in 2005 (along with work on committment, credibility, focal points and uncertainty in games of conflict, and also work on self-control). You should read Schelling's 1971 paper in the *Journal of Mathematical Sociology* on "Dynamic Models of Segregation" to get a sense both of how prescient a thinker Schelling was (and still is) and to appreciate how much the field of Economics has benefited from the formalization of theory that has ensued in the subsequent decades (the distance in formal argument between Roy 1951 and Schelling 1971 is quite minor). I highly recommend Schelling's 1978 book, *Micromotives and Macrobehavior*. This is a rich and readable set of applications and extensions of the ideas exposted in the 1971 behavior.

The 'tipping' model is probably already part of your standard toolkit of informal economic arguments. But it's still enlightening to work through the Schelling 1971 paper which is nuanced and generally counterintuitive beyond its most basic level.

There are several models presented in Schelling 1971 (all informal). I'll discuss two briefly.

3.1 Schelling Bounded-Neighborhood model

There is a single neighborhood and everyone is either inside or outside of it. The neighborhood does not have a capacity constraint—as many people as want to live there can do so. Each person will choose to live there unless the percentage of residents of the opposite color (white, black) exceeds his tolerance. That is individuals do not need to have a preference about whom they live with, but if they do have a preference, it must take the form of a maximum fraction of the other group they will abide (this can also be rephrased as a minimum number of their own group they require to be comfortable). Assume that people can leave and return from the neighborhood freely (this helps with the dynamics) and that the least tolerant always leave first. Finally, there is no anticipatory behavior.

One observation to note right away is that the upper bounds of tolerance must sum to at least 100 for any mixed equilibrium of blacks and whites to be stable. For example, if the most tolerant black can tolerate 50 percent whites and the most tolerant white can tolerate only 49 percent blacks, then there is no stable equilibrium:

- If the neighborhood were 50 percent white, blacks would (by definition) comprise the other 50 percent. This would be intolerable to the most tolerant white, and so whites would move out.

- If the neighborhood were 51 percent white and 49 percent black, this would be intolerable to the most tolerant black, and so blacks would move out.

- If the neighborhood were 49 percent white, whites would move out, etc.

Schelling asks what equilibria are feasible in this setting. To answer this, we need a distribution of tolerance. Figure 18 gives one example of a linear PDF. There are 100 whites, the most tolerant of whom will accept a ratio of 2 blacks to 1 white, and the least tolerant of whom will not tolerate any blacks. $T_i^w = 2 - i/50$. The 50th percentile white will tolerate a ratio of 1 to 1. There are 50 blacks, and they have the same tolerance schedule, ranging from 2 to 0. $T_i^B = 2 - i/25$.

The lower panel of Figure 18 plots these tolerance curves in white-black population space. These diagrams take a bit of getting used to. The first plotted point (excluding 0,0) says that there is 1 white that can tolerate up to 2 blacks. The point $(25, 37.5)$ says that there are up to 25 whites who can tolerate 37.5 blacks (a ratio of 1.5 to 1). Note that 24 of these whites are inframarginal—they can tolerate additional blacks. But when the ratio exceeds 1.5, there are fewer than 25 whites who will stick around. Similarly, the black curve says that there are up to 10 blacks who can tolerate 15 whites, and up to 25 blacks who can tolerate 25 whites, etc. (These curves are neither PDFs or CDFs.)

What is the equilibrium in this figure? Imagine we are inside the elipse made by the intersection of the two curves, for example at point $(25, 25)$. Both blacks and whites are happy at this point. But the problem is that there are as many as 50 whites who are happy at a ratio of 1 to 1, whereas there are only 25 blacks who are happy at this ratio. This means that more whites will move in (up to 25 more). But now the white/black ratio is 2 to 1, and only 1 black can tolerate that ratio. So, blacks move out—and that attracts increasingly intolerant whites. Eventually, we are at 100 whites and 0 blacks. A similar thought exercise shows that there is no stable integrated equilibrium in this diagram. If we start inside the elipse, we always converge to all white. If we start above the elipse but under the black curve, we end up at an all black equilbrium. If we start below the elipse, we end up at an all white equilibrium.

Notice by the way that tolerance is the enemy of equilibrium in this diagram. It's exactly the 'friendly' whites who don't mind a mixed 50-50 B/W neighborhood who are driving out the blacks with their enthusiasm to move into the neighborhood.

Figure 19 gives another nice example. The tolerance schedule is $T_i = 5 - i/20$. Thus, the median white will tolerate a ratio of 2.5 blacks per white resident. There are 100 whites and 100 blacks. There is a stable equilibrium in this setup at 80 blacks and 80 whites. This is because there are no more than 80 whites willing to tolerate a ratio of $1:1$ and no more than 80 blacks willing to tolerate a ratio of $1:1$. Thus, there is no further inward or outward movement of either group at $(80, 80)$. Notice that if whites were more tolerant in this example, this would erase the mixed equilibrium. And if the last 20 whites were much more *intolerant*, this would have no effect at all. Moreover, notice that the median white and the median black have tolerances of 2.5 each and yet the equilibrium is nowhere close to these preferences.

There are many other nice examples in the paper. But the model is a bit odd in that there is no capacity constraint in the neighborhood, and one suspects that some of the odd results stem from the 'killing us with kindness' feature of the model (whereby tolerant whites or blacks flood the neighborhood, thereby chasing out the other group). Also, these models are in a way too robust in that they find their stable equilibria very readily (it just depends on the assumed starting point) and tend to stay there (this also follows from the unlimited capacity feature). Thus, there is no notion of instability in these models. The tipping model is the more enduring contribution.

3.2 Schelling Tipping model

The tipping model captures the observation that a neighborhood can apparently be at a stable racial mix and yet a small perturbation can cause an unraveling that leads to full segregation. In this model, neighborhoods have a capacity constraint. This eliminates the somewhat unappealing feature that greater tolerance leads to greater odds of a segregated equilibrium. The lecture slides parse some of the figures given in the paper. Another important feature of the model is that the equilibrium does not appear to particularly maximize anyone's preferences or well-being.

What is distinctive about many of these examples is that they exhibit locally stable ranges of integration. But a small perturbation to the neighborhood mix may produce a movement towards a fully segregated equlibrium (and even one with vacant properties, as shown in Figure 32). Thus, these models exhibit 'tipping.' In general, greater tolerance in these examples does not lead to greater chances of tipping. However, having a relatively larger population of one group does lead to a greater chance of tipping because there will tend to be sufficiently many tolerant members of the larger group that they will tend to eventually flood the neighborhood, causing the other group to flee.

There is a real ‘tail-wags-dog’ aspect to the equilibria in that the preferences of a single individual can lead an entire neighborhood to unravel. In this sense, preferences act more like constraints on the problem rather than maximands of the invisible hand. Not coincidentally, preferences are written in this model as *willingness to tolerate* given neighbors rather than *desire* to have given neighbors. If we reinterpreted these preferences as individuals’ demand for segregation, the equilibrium would have the property that the resident with the highest demand for segregation is indifferent between the current neighborhood and the outside option. More tolerant neighbors would earn greater surplus. In this seeting, there are externalities in all direction. The most prejudiced resident who remains in the neighborhood exerts a positive externality on his co-ethnic neighbors by keeping the degree of neighborhood segregation above their reservation values.

The tipping model has two key limitations. First, there are no prices. This makes it a bit less surprising that housing markets don’t function well here. Second, there is no forward looking behavior. One suspects that part of the tipping phenomenon (if it exists) is likely to be driven by *fear* of one group or the other that the neighborhood may tip—which makes them want to exit before property values fall. (Another shortcoming from a contemporary perspective is that nothing is formally proven here, so it’s hard to draw general lessons from this model.)

The Card, Mas and Rothstein (2008) paper introduces a version of the Schelling model that incorporates prices and provides evidence on the relevance of the tipping phenomenon. The working paper by David Dorn (2009) extends CMR (2008) to incorporate forward looking behavior, and this gives nice (and empirically relevant) predictions on the differential reaction of homeowners versus home-renters to changes in neighborhood racial composition.

3.3 Evidence on tipping: Card, Mas and Rothstein

The tipping phenomenon is widely believed-in but rarely or perhaps never demonstrated. For example, a 2005 paper by Easterly concluded that the tipping model is not supported for US cities. Perhaps inspired by Schelling’s 2005 Nobel prize, Card, Mas and Rothstein took another look at the data and found reasonably striking evidence (see Figure I for a compelling example).[1]

3.3.1 Model

The CMR model is simple and useful in a number of respects. Most importantly, it explicitly includes prices in the real estate market. It is silent on the possible role played by forward-looking expectations.

Consider a neighborhood with a homogenous housing stock of measure one and two groups of potential buyers: whites (w) and minorities (m). Let $b^g(n^g, m)$ denote the inverse demand functions for the two groups for homes in the neighborhood given minority share m (and $g \in \{w, m\}$). So there are n^g families from group g who are willing to pay at least $b^g(n^g, m)$ to live there.

By construction, these inverse demand functions are weakly downward sloping in n:

$$\begin{aligned} \partial b^w / \partial n^w &\leq 0, \\ \partial b^b / \partial n^b &\leq 0. \end{aligned}$$

The partial derivatives, $\partial b^w / \partial m$ and $\partial b^m / \partial m$, reflect the social interaction effects on the inverse demand functions. A key assumption is that for minority shares above some threshold, whites are willing to pay *less* than they would otherwise. For example, $\partial b^w / \partial m < 0$ for $m \geq 10$ percent.

At an integrated equilibrium with minority share $m \in (0, 1)$, the mth highest minority bidder has the same willingness to pay as the $(1 - m)$th highest white bidder:

$$b^m(m, m) = b^w(1 - m, m).$$

Differentiating the white bid function with respect to the neighborhood minority share:

$$\begin{aligned} \partial b^w(1 - m, m) / \partial m &= \partial b^w / \partial(1 - m) + \partial b^w / \partial m. \\ &= -\partial b^w / \partial n^w + \partial b^w / \partial m. \end{aligned}$$

Assuming that $\partial b^w / \partial m \approx 0$ when m is low, and that $\partial^2 b^w / \partial m^2 < 0$, the white demand curve expressed in minority share space will initially be upward sloping and then will turn down when

[1] A key difference with Easterly is that they studied tipping at the Census tract level rather than the MSA level. In fact, they use MSA fixed effects in all models to take out the MSA mean.

$-\partial b^w/\partial n^w < -\partial b^w/\partial m$.[Note: the *upward slope* of the demand curve stems from the fact that at the lefthand extreme of the figure, all whites are housed in the neighborhood, so that the marginal white buyer has low valuation of the neighborhood. As we move rightward, fewer whites are housed, so the marginal white buyer has greater willingness to pay—until the social interaction effects dominate.]

Consult Figure II. Observe that point A is a stable mixed equilibrium. Small perturbations to A will re-equilibrate: a slight increase in the minority share will raise white relative to minority demand (because the whites with the highest demands want to get back into the market); a slight decline in minority share will raise relative minority demands for the same reason.

By contrast, point B is unstable. If the minority share increases at all, white demand falls off rapidly whereas black demand trends down smoothly. Thus, the neighborhood tips. The reverse also appears true. If the minority share falls a bit, white demand rises faster than minority demand, and we end up back at point A.

Figure III shows how tipping could occur as minority demand rises secularly (perhaps due to rising minority population nationally). If we are initially at an all W equilibrium, the first few increments to the minority share do not induce tipping. But as soon as the B demand curve exceeds the maximum of the W demand curve, tipping follows. Note by the way that at the point of tangency between the maximum of the W demand curve and the B demand curve, tipping can only occur in one direction: towards all minorities. If instead we perturbed the model such that slightly more whites moved in, B demand would rise faster than W demand, leading us back to this point of tangency.

An interesting feature of this model is that prices do *not* need to fall discontinously as a neighborhood tips. This is because B demand takes over smoothly from W demand at the discontinuity. Of course, if $B's$ believed the neighborhood was about to tip as the tipping point was reached, prices might fall off much faster. The model does not deal with the question of expectations and anticipatory behavior. One suspects that these expectations would *reinforce* the tipping process. However, they might also make it less discreet (i.e., the tipping point could be smoother). But that's just a conjecture.

3.3.2 Identification

Identification of this model is subtle (and perhaps not entirely kosher). The objective is to perform a regression discontinuity analysis at an *unknown* discontinuity. So, in reality, the exercise is to estimate whether or not a discontinuity exists at an *estimated* location.

Assume that there is a tipping point at m^*. Let $m_{t-1} \in [0, m^* - r]$, where r is the maximum feasible relative demand shock. At m_{t-1}, there is no demand shock that will cause the neighborhood to tip *in the next period*. Thus:

$$E[\Delta m_t | m_{t-1}] = g(m_{t-1}),$$

where $g(\cdot)$ is a continuous function.

By contrast, for $m_{t-1} > m^*$:

$$E[\Delta m_t | m_{t-1}] = h(m_{t-1}) > 0.$$

There is also an intermediate range $m_{t-1} \in [m^* - r, m^*]$ where tipping is possible but uncertain (depending on the size of the minority shock). If this range is small, we can write:

$$E[\Delta m_t | m_{t-1}] \approx \mathbf{1}[m_{t-1} < m^*]\, g(m_{t-1}) + \mathbf{1}[m_{t-1} \geq m^*]\, h(m_{t-1}).$$

If

$$\lim_{e \to 0^+} [h(m^* + \varepsilon) - g(m^* - e)] > 0,$$

then there is a discontinuity at m^*.

Thus, the exercise is to identify candidate locations for m^* and test for a discontinuity at these points.

3.3.3 Empirical specification

The main dependent variable is the change in the share of white population over a ten year period expressed as a share of initial population

$$D_{wic,t} = (W_{ic,t} - W_{ic,t-10}) / P_{ic,t-10},$$

where i is a neighborhood and c is a city.

The main explanatory variable is the initial minority share:

$$m_{ic,t-10} = M_{ic,t-10}/P_{ic,t-10}.$$

A key assumption is that there is a city-specific tipping point $m^*_{c,t-10}$, so the model can be identified by within-city, cross-neighborhood variation in initial minority shares. Let

$$\delta_{ic,t-10} = m_{ic,t-10} - m^*_{c,t-10.}$$

The base specification is:

$$D_{wic,t} = p\left(\delta_{ic,t-10}\right) + d\mathbf{1}\left[d_{ic,t-10} > 0\right] + \tau_c + X_{ic,t-10}\beta + \varepsilon_{ic,t}.$$

Here, $p(\cdot)$ is a fourth-order polynomial in δ, and τ_c are city fixed effects. The model is estimated separately by decade.

A key issue is identifying $m^*_{c,t-10}$. The paper uses two methods. The first is to assume the existence of a structural break and look for the point that best fits this assumption. Specifically, write the equation above by city as:

$$D_{wic,t} = \alpha_c + d_c\mathbf{1}\left[m_{ic,t-10} > m^*_{c,t-10}\right] + \varepsilon_{ic,t}$$

This equation is only estimated for $0 \leq m_{ic,t-10} \leq 60$, and the statistical approach selects the value of $m^*_{c,t-10}$ on the interval $[0, 50\%]$ that maximizes the R-squared of the equation. This estimator is consistent if the identifying equation is correctly specified. However, the paper reports that this approach is not terribly robust.

The second method is trickier. The model implies that the city specific tipping point is a fixed point of the process—the minority share at which the white population share remains unchanged (relative to the city specific growth rate). If this point exists, the following will be true:

$$E\left[D_{wic,t}|c, m_{ic,t-10} = m^* - \varepsilon\right] > E\left[D_{wic,t}|c\right] > E\left[D_{wic,t}|c, m_{ic,t-10} = m^* + \varepsilon\right],$$

for $\varepsilon > 0$.

To identify the point, the authors smooth the data to obtain a continous approximation $R\left(m_{t-10}\right)$ to $E\left[D_{wic,t}|c, m_{ic,t-10}\right] - E\left[D_{iwc,t}|c\right]$. They use a fourth order polynomial to approximate the function on neighborhoods where $m_{ic,t-10} < 60\%$. They calculate the roots of the function for each city (roots are equal to the inflexion points because $\partial R/\partial m_{ic,t-10} = 0$ at these points). Further details are given in the paper.

An important feature of their approach is to use a 'leave out' sample for hypothesis testing. Two-thirds of the data are used to estimate $m^*_{c,t-10}$ for each city. The other one-third is used for hypothesis testing. One has to admire the craft here. It's not entirely clear, however, that one can consistently estimate a regression discontinuity without knowing with certainty exactly where that discontinuity is located

3.3.4 Results

The main data used are the Neighborhood Change Database (NCDB).

- First key results are in Table III. These models estimate the RD at the candidate tipping points. Standard errors should be okay because the tipping points are estimated from a separate subsample (2/3rds) of the data.

- Table IV shows that in neighborhoods that are supply constrained (because they are highly developed), tipping takes the form of white outflows and minority inflows of equal and opposite size. In neighborhoods that are not full, it is mostly that whites exit while the minority population doesn't really move.

- Table VII shows that tipping primarily occurs in neighborhoods that are distant from areas that have already tipped. That suggests that expectations *are* relevant. If nearby neighborhoods have already tipped, then anticipatory behavior may eliminate the sharp behavioral discontinuity.

- There is not much evidence of a discontinuity in rents or housing values at the candidate discontinuity. This is potentially consistent with the model in which minority willingness to pay is not discontinuously falling at the tipping point.

- Table VIII shows evidence of tipping in the racial composition of elementary schools. It's not entirely clear what drives this discontinuity—changes in school composition or changes in the neighborhood composition (obviously these are not independent).

- Table IX shows that the tipping point is lower where racist attitudes are more prevalent (this strategy also used by Charles and Guryan). It's also lower where whites are wealthier.

3.3.5 Conclusion

This is a terrific piece of social science. I suspect that this paper will open a research agenda with two fronts: identifying tipping in other settings, and using RD-tipping designs to examine the effect of segregation (or tipping itself) on other outcomes of interest.

3.4 Other causes of segregation

The 2007 NBER working paper by Elizabeth Ananat presents an innovative analysis of the causes and consequences of segregation, using railroad track configurations laid in the 19th century as an instrument for neighborhood segregation that ensued *after* the great Black migration North following WWII. This is an ambitious and creative paper.

The 2007 *QJE* paper by Baum-Snow analyzes a closely related question: to what degree did declines in the cost of commuting brought about by the construction of the US highway system caused the depopulation of central cities in the post-WWII period. Using highways planned as of 1947 as instruments for highways built in subsequent decades, Baum-Snow finds that while aggregate central city populations fell by 17 percent between 1950 and 1990 (despite 72 percent population growth in metropolitan areas), counterfactually, central city populations would have risen by 8 percent had the interstate highway system not been built.

The 1999 paper by Cutler, Glaeser and Vigdor evaluates a number of explanations for 'ghettoization,' including centralized racism (such as restrictive mortgage covenants and explicit and implicit threats of violence), decentralized racism (i.e., white and black preferences), and 'port of entry' effects whereby recent migrants had a preference for settling with others of the same race group. The paper has a huge amount of interesting factual material. The analysis, while somewhat impressionistic, is intriguing. The authors conclude that decentralized racism has replaced centralized racism as the primary force behind ongoing high levels of segregation in the US. Although US cities remains highly segregated—with more all-black areas than ever before—average levels of segregation have substantially fallen since the 1970s.

4 The impact of ghettos on economic outcomes

Why should we care about segregation? Disembodied scientific curiousity is certainly one valid reason. But clearly the reason that most people care about this question is that ghettos are associated with many social maladies, including poverty, unemployment, school dropout and violence. Many suspect that ghettos (or, more broadly, bad neighborhoods) cause these bad outcomes, at least in part. If so, this might have great relevance for policy—especially if we believe (as per Schelling) that ghettoization reflects a coordination failure where the collective outcome is often *not* representative of the preferences of the median neighborhood dweller. These has been an extensive effort to analyze the causal effects of neighborhoods on individual and collective outcomes. The causal effect on individual outcomes is perhaps more readily assessed, though perhaps not as economically important. We'll discuss some evidence on this point, and then turn to the question of collective outcomes.

4.1 Neighborhood effects

What is the consequence of removing families from poor neighborhoods. There are now many good studies on this topic. It's potentially useful to bound the range of effects by considering the minimal and maximal interventions on which we have evidence. For minimal interventions, the Jacob 2004 paper on Chicago public housing demolitions is extremely interesting. For maximal interventions, the authoritative source in Kling, Liebman and Katz's 2007 study of the Moving to Opportunity program.

4.1.1 Jacob, 2004

Context: the Chicago Housing Authority houses almost 5 percent of the Chicago population, and is the third largest housing authority in the US. In 2000, it had 28,335 units with 50,526 residents, most of them families with children. Numerous building closures during the 1990s gave residents an opportunity to relocate. Families required to relocate were given the option to either (1) transfer to another unit within their current development; (2) transfer to another CHA facility; or (3) receive a section 8 voucher as well as paid moving expenses and help with the cost of transfering utilities. [Section 8 is a rent voucher program. Recipients in theory have discretion as to where to use the voucher, but are often constrained by rental prices and willingness of landlords to house Section 8 tenants.] It's important to bear in mind that the subjects of this treatment were not volunteers and were given very limited support to adapt to the change in their housing situation.

The outcome of interest here is academic achievement of children whose families were moved. There three channels by which a causal effect could arise: (1) moving to lower poverty neighborhoods; (2) moving to improved schools; (3) moving to better buildings (it is sometimes argued that 'bad' buildings are intrinsically crime-provoking). Of course, public housing might have benefits in terms of community and convenience. Moreover, forcible moves may have their own adverse consequences.

The identification approach is to contrast outcomes of students who were living in buildings slated for demolition immediately prior to the closure annoucement with students living in buildings in the same project that were not closed:

$$y_{ijt} = D_{ijt}\pi_1 + \gamma_j + \delta_t + \varepsilon_{ijt},$$

perhaps augmented with covariates.

A concern is that mobility out of the Chicago Public Schools (CPS) may be causally affected by moving, which could cause attrition bias. But this does not appear to be the case.

The results of the paper are straightforward but fascinating. Housing demolitions have zero effect on educational outcomes—except for an 4 percentage point (8 percent) increase in dropout among the oldest kids at the time of the move (suggesting that the disruption may have led to dropout).

Public housing evictions do, however, reduce the probability of being in public housing by about 20 percentage points and reduce the census tract poverty rates in which movers live by about 15 percentage points. But residents do not move far (1.3 miles on average) and do not attend better schools as measured by the percent of school peers who meet test norms in mathematics.

Table 6 uses housing demolition as an instrument for living in public housing relative to living in Section 8 housing. There is no evidence of a causal effect of public housing on academic outcomes. Jacob offers a fair amount of discussion of how these IV estimates should be interpreted. His view is that we ideally want to believe that public housing demolition does not affect mobility, neighborhood, school and relocation. Thus, the causal effect identified is the pure treatment effect of living in public housing rather than the causal effect of other intermediating variables that are 'assigned' by public housing (e.g., bad neighborhoods, bad schools, etc.). My view is that this interpretation is a bit narrow. If we want to interpret the experiment is identifying the pure public housing effect, then the treatment assignment *out of* public housing must be ignorable. But we see evidence of a disruption effect of leaving public housing (since school non-completion rose for older kids).

This is a paper offers a compelling null result, though the interpretation of the null is a bit up for grabs. One reading is that moves out of public housing have no benefits because the places to which families move are near identical. A second reading is that neighborhood effects are simply not very important for individual outcomes.

4.1.2 Oreopoulos, 2003

The 2003 paper by Philip Oreopoulos produces a null result consistent with Jacob's paper, here using public housing random assignments in Toronto. The main finding is that children assigned to public housing projects with substantially more crime and worse living conditions fare no worse at age 30 than those assigned to better projects. This result is consistent with the second reading of the Jacob null.

What I most like about the Oreopoulos paper (among other virtues) is its use of something akin to the behavioral genetics framework to make progress on the key question, i.e., how much do neighborhood "environment" matters for outcomes. The starting point of the discussion is that there are *many* attributes of neighborhood quality that will only be imperfectly proxied the available set of neighborhood observables (such as crime rates, poverty, number of Starbucks, etc.). Thus, by running models of the form:

$$\bar{Y}_a - \bar{Y}_b = \alpha\left(\bar{X}_a - \bar{X}_b\right) + \eta_a - \eta_b,$$

where a and b are housing projects, we could erroneously conclude from the fact that $\hat{\alpha} \approx 0$ that neighborhood (or project) environment is unimportant, even if η_a and η_b are economically large.

This observation motivates an approach proposed by Solon, Page and Duncan in 2000 and based loosely on the Behavioral Genetics framework. Let Y_{sfp} be the outcome for sibling s in family f in project p. Write

$$Y_{sfp} = \gamma X_{sfp} + \eta_p + \varepsilon_{sfp}.$$

Assume that X includes all relevant characteristics (even those that are unobserved). The population variance of Y_{sfp} is

$$var\left(Y_{sfp}\right) = var\left(\gamma X_{sfp}\right) + var\left(\eta_p\right) + 2Cov\left(\gamma X_{sfp}, \eta_p\right) + var\left(\varepsilon_{sfp}\right).$$

The covariance of sibling s and s' is:

$$cov\left(Y_{sfp}, Y_{s'fp}\right) = cov\left(\gamma X_{sfp}, \gamma X_{s'fp}\right) + var\left(\eta_p\right) + 2cov\left(\gamma X_{sfp}, \eta_p\right).$$

And the covariance among two unrelated neighbors living in the same project is:

$$cov\left(Y_{sfp}, Y_{s'f'p}\right) = cov\left(\gamma X_{fp}, \gamma X_{f'p}\right) + var\left(\eta_p\right) + 2cov\left(\gamma X_{fp}, \eta_p\right).$$

If families are randomly assigned to projects, then the first and last terms of this expression are zero, and so we can directly estimate $var\left(\eta_p\right)$.

This is useful because the sibling outcome with random assignment is:

$$\rho_{s,s'} = \frac{cov\left(Y_{sfp}, Y_{s'fp}\right)}{var\left(Y_{sfp}\right)} = \frac{cov\left(\gamma X_{sfp}, \gamma X_{s'fp}\right) + var\left(\eta_p\right)}{var\left(Y_{sfp}\right)},$$

while the neighbor correlation with random assignment is:

$$\rho_{f,f'} = \frac{var\left(\eta_p\right)}{var\left(Y_{sfp}\right)}.$$

Thus, a comparison of these two correlations allows us to estimate the relative importance of family (here genes + environment) versus neighborhood. This exercise is interesting to the degree that there is meaningful variation across randomly assigned neighborhoods (otherwise, we can presume that $var\left(\eta_p\right)$ is small). Table I of the paper makes the argument that there is real variation in economic conditions across housing projects in Toronto, despite it being in Canada.

Figure II of the paper is consistent with Jacob (2004). There are no observable differences in the distribution of income at age 29 to 36 among kids who grew up in relatively poor versus relatively prosperous housing projects.

Table VIII presents the variance decomposition. There are two surprising results from this decomposition:

1. Available covariates do a *poor* job of explaining the correlation between sibling outcomes. The raw sibling correlation in income at age 29 to 36 is 0.312. The residual correlation is 0.296. Thus conditioning out the $X's$ does not remove any of the correlation among outcomes within families. This underscores the potential importance of using the variance decomposition to measure common effects rather than attempting to proxy them with $X's$.

2. There is almost no correlation in outcomes among neighbors within public housing developments in Toronto. And the neighbor-correlation within other Toronto neighborhoods is also extremely small. This finding buttresses the case that neighborhood variation of the type examined here (counting both observed and unobserved dimensions) has little impact on life outcomes of children raised in these neighborhoods. These correlations are so low as to almost defy belief. But I have no reason to doubt them.

4.1.3 Kling, Liebman and Katz, 2007

This study analyzes what is arguably a 'maximal' intervention, which is moving familes from high poverty public housing projects to census tracts with poverty rates of below 10 percent (families could move after one year). The sample consists of 4,248 households assigned during 1994-1997 from sites in Baltimore, Boston, Chicago, Los Angeles and New York.

Outcome data were collected on five domains:

1. Economic self-sufficiency

2. Mental health

3. Physical health

4. Risky behavior

5. Education

There are 15 outcomes, four population groups (adults, all youth, female youth and male youth) and two treatment groups, giving rise to 120 treatment effects. It should be clear that this poses some statistical issues. The authors address this problem by creating summary index measures of groups of outcomes in each of the 5 categories. These summary measures are simple averages of standardized values of each outcome in a category, where signs are flipped where needed to make all measures correspond to positive (or negative) outcomes.

There is an important subtlety here. Most hypothesis testing simply tests for coefficients that differ from zero, and is therefore agnostic about sign. The approach used here builds from the prior hypothesis that the treatment should positively or negatively affect a cluster of related outcomes within a domain. Consequently, valence adjusted outcomes that positively covary will increase the power of the test to reject the null. If, however, the treatment has countervailing effects on outcomes within a domain, this will increase the probability that the null is accepted. By contrast, a standard F-test of the significance of the treatment across a variety of outcomes tests only whether this set of outcomes would be likely to occur by chance, and does not otherwise impose any structure of the substantive consistency of the findings.

A summary finding of this paper is that the benefits of neighborhood relocations are generally modest. For adults, mental health improvments and obesity reductions are sizable. For youth, mental health and substance use both improve among females (that is, substance use is reduced). But for males, the opposite appears true.

This general finding—that males don't benefit from social interventions—is rising to the status of a Folk Theorem in economics. For example, the 2008 *JASA* paper by MIT student Michael Anderson presents a reanalysis of the widely heralded, intensive pre-school random-assigment evaluations of the 1970s, including Perry Preschool, Abcederian and Early Learning. Michael finds that the results support the inference that only girls benefited from these interventions. The male results, while highly significant on some dimensions, are consistent with chance.

The fact that this radical, and hugely costly, intervention produced relatively small (except on mental health) and not altogether positive effects suggests that the power of social policy to improve economic outcomes of the disadvantaged through even large changes in neighborhood environment is probably limited. This result is probably not in accord with the priors of the funders of this study. It is an important result for both policy and social science.

4.1.4 Aside: Multiple inference

You should use the opportunity of the Kling et al. and Anderson papers to tool-up on the subject of multiple inference. Multiple inference is an overlooked topic by economists that has recently gained prominence, likely due to the popularity of experimental interventions that plausibly affect numerous outcomes. The Anderson paper is an excellent primer. The unpublished appendix to Kling et al. (online) is also very useful. The Autor-Houseman paper on my homepage, "Do Temporary Help Jobs Improve Labor Market Outcomes for Low-Skilled Workers? Evidence from 'Work First"' also provides a pretty clear exposition of one technique.

Here is an example adapted from the Autor-Houseman paper, which studies the impact of temporary-help and direct-hire job placements on the subsequent labor market outcomes of Work First participants in Detroit. The study uses the rotational assignment of participants to Work First contractors in Detroit (an administrative feature) as a source of random variation in the participants assigned among contractors operating within each adminitrative district within each program year.[2] A-H test whether the data are consistent with random assignment (as expected) by statistically comparing eight characteristics of participants assigned to contractors within each district and year: sex, white race, other (non-white) race, age and its square, number of quarters worked in the eight quarters before program entry, number of quarters employed with a temporary agency in these prior eight quarters, total earnings in these prior eight quarters, and total earnings in the prior eight quarters from temporary agencies.

In testing the comparability of participant characteristics across eight characteristics, A-H are likely to obtain many false rejections of the null (41 tests $\times$ 8 covariates), and this is exacerbated by the fact that participant characteristics are not fully independent. To account for these confounding factors, A-H estimate a Seemingly Unrelated Regression (SUR) system

[2] Contractors sign annual contracts with the City of Detroit, and the set of contractors servicing a district may change from year to year.

to test the hypothesis that the observed distribution of participant covariates across contractors within each randomization district and year is consistent with chance.[3] The SUR accounts for both the multiple comparisons problem and the correlations among demographic characteristics across participants at each contractor.

The procedure is as follows. Let X be an $8 \times N$ matrix containing the 8 baseline covariates listed above for each of the N Work First spells assigned to contractors in all district and years of our sample. Let D be an $N \times k$ matrix of k indicator variables designating the district-year in which each spell is assigned (of which there are 41 in our sample). Let Z be a $N \times (j-k)$ matrix containing a set of $j-k$ indicator variables designating the contractor and year to which each spell is assigned, with one indicator omitted for each of the k district-year pairs. Finally, let λ be a $j \times 1$ vector of parameters to be estimated and I_8 be the 8×8 identity matrix. We estimate the following SUR model:

$$Y = I_8 \otimes (Z \quad D)\,\lambda + \varphi \quad Y = \text{vec}\,(X')\,. \tag{1}$$

In this expression, Y is an $8 \cdot N \times 1$ vector containing the 8 rows of Y transposed and stacked in a column vector, and φ is a matrix of error terms that allows for cross-equation correlations among participant characteristics at each contractor.[4] The p-value of a test that the first $j-k$ elements of λ in this regression system are jointly equal to zero provides an omnibus test for the null hypothesis that participant covariates do not differ among participants assigned to different contractors within a district and year. A high p-value corresponds to an acceptance of this null.

Just to clarify the details, note that

$$\begin{aligned} &Y \text{ is } 8 \cdot N \times 1 \\ &(Z \quad D) \text{ is } N \times j \\ &\lambda \text{ is } j \times 1 \\ &\varphi \text{ is } 8 \cdot N \times k \end{aligned}$$

[3] This method for testing randomization across multiple outcomes is proposed by Kling, Liebman and Katz (2007) in their Web Appendix available at www.nber.org/~kling/mto/mto_exp_a.pdf.

[4] Since the j contractor dummies in Z are mutually exclusive, one is dropped.

The Kroneker product operator, $\otimes$, turns the $(Z \quad D)$ matrix in the above equation (which is $N \times j$) into a block-diagonal matrix of X variables that is $8 \cdot N \times 8 \cdot j$. Point estimates of λ for this SUR system will be *identical* to equation-by-equation OLS estimates of the 8 linear equations in this system. (This should be familiar from your basic econometrics class: SUR and OLS produce identical point estimates when identical right-hand side variables are included in all equations.) What differs between SUR and OLS, however, is the covariance matrix φ. By allowing for covariances among the coefficients in each of the models in φ, we can perform joint F-tests (or chi-squared tests) for the nullity of the elements of λ. These tests account for the non-independence of errors across equations.

Appendix Table 1 of A-H provides the chi-square statistics and p-values of tests of the null hypothesis for estimates of Equation (1) for each of the 41 district-by-year cells in the sample. Consistent with the expectation that the rotational assignment of participants across contractors operating within districts is functionally equivalent to random assignment, the overall p-value of the randomization test is 0.44, which is quite consistent with a chance distribution of covariates. Performing this statistical comparison individually for the 41 district-years in the sample, A-H find that 39 of 41 comparisons accept the null hypothesis at the 10 percent level or higher, and only one comparison rejects the null at conventional levels of significance. In the final row and column of the table, A-H also provide the p-value for the comparison test for each year, pooling across districts, and each district, pooling across years. All but one of these sixteen tests readily accepts the null at conventional levels of significance. In net, these results support the hypothesis that the rotational assignment of participants across contractors generates variation that can be treated as random.

Note that the SUR test used by A-H does *not* take a stand on the signs of the elements of λ. This is unlike the Kling, Liebman, Katz approach, which incorporates the 'valence prior.' For a test of randomization, we are only interested in whether the distribution of covariates is consistent with chance, so the SUR is appropriate for the A-H application. By the KLK approach is more powerful when we have a clear prior on how the treatment effects should covary.

The A-H research design also requires that random assignment to contractors significantly affects participant job placements. To confirm this, A-H estimate a set of SUR models akin to equation (1) where the dependent variables are participant Work First job outcomes (direct-hire, temporary-help, non-employment). Here, the expectation is that job placement outcomes should differ significantly across contractors within a district and year. Tests of this hypothesis provide strong support for the efficacy of the research design: the omnibus test for cross-contractor, within district-year differences in job placement outcomes rejects the null at below the 1 percent level for the full sample, as do 16 of 17 tests for significant differences in placement rates across all districts within a year or within a district across all years.

A-H also calculate partial R-squared values from a set of regressions of job placement type (any job placement, direct-hire job placement, temporary-help job placement) on dummy variables indicating contractor-by-year of assignment after first orthogonalizing these job placement types with respect to demographic, earnings history, and time variables; conversely, they compute partial R-squared values from regressions of job placement types on demographic, earnings history, and time variables after first orthogonalizing the dependent variable with respect to contractor assignment. They find that contractor assignment explains 85 to 130 percent as much variation in job placement type as do demographic, earnings history, and time variable combined. This is a potentially useful exercise for testing the power of the 'first stage' when the instruments are a collection set of dummy variables (so there is no single first stage coefficient).

5 Other consequences of segregation

5.1 Segregation and black political efficacy

The papers above study person level outcomes. It is possible for aggregate outcomes to be affected by segregation as well. It's plausible that some aggregate outcomes are affected even if individual outcomes are not. For example, selection into neighborhoods could be *causally affected* by segregation. Housing prices and political outcomes may also be affected. The NBER working paper by Ananat and Washington examines one mechanism by which segregation may affect collective outcomes, which is by reducing Black political power.

Their analysis exploits the identification strategy used by Ananat in her 2007 paper to identify the effects of ghettos on economic outcomes, that is, 19th century railroad track configurations. This paper is arguably a(n even) better application of this instrument in that the selection questions (does ghettoization lead to bad outcomes due to production or selection) are not central. Ultimately, we probably care about whether ghettoization reduces Black political efficacy regardless of the mechanism (we also care about the mechanism, of course).

Figure 2 and Appendix Figures A1 - A6 of Ananat 2007 show how the Railroad Division Index (RDI) is constructed.

$$RDI_j = 1 - \sum_i \left(\frac{area_i}{\sum_i area_{i,}} \right)^2 .$$

This is essentially a geographic Herfindahl index. An RDI of 0 means that there are no tracks running through an MSA. An RDI of 1 means that the area is essentially infinitely subdivided by track divisions. Figure 3 of Ananat (2007) suggests that there is considerable variation in this measure, and that is highly correlated with segregation levels observed in later decades (Figure 3 does not make it clear which decade is plotted).

- Table II of Ananat and Washington provides the first stage for segregation by decade for the analysis sample of matched MSAs.
- Table IV provides evidence that segregation reduces the fraction of Black House candidates, marginally reduces Black Representatives (i.e., those actually elected), and results in more conservative voting by the Representatives of these districts.
- Table VI suggests that segregation leads to more conservative political attitudes among whites.
- Table VII presents some evidence that these attitudinal differences are coming from recent non-Black movers to segregated cities and from young residents. These results may indicate that selection of residents with more conservative political attitudes into segregated cities is a partial explanation for lower Black political efficacy in these cites.

This paper certainly does not nail the case that segregation is harmful to Blacks. For example, we don't know how a reduction in efficacy as measured in the paper translates into any legislative or governmental actions that affect Black citizens. But it does at least suggest that segregation may have aggregate affects that are not measurable at the individual level.