EC1410 Topic #9

Sorting, a simple heterogenous agents model, and local public finance

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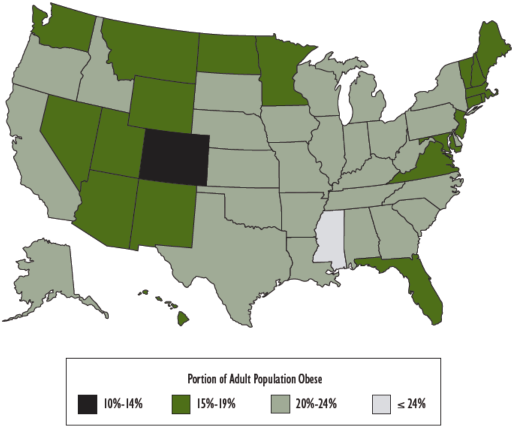
Outline

1. Sorting vs causation?
2. Sprawl and obesity
3. Neighborhood and diet
4. Test scores and house prices
5. Neighborhood and human capital
6. Spatial equilibrium and Tiebout sorting

People are different in different places

FIGURE 3. Obesity* Among U.S. Adults

*BMI≥30 or ~30lbs overweight for a 5'4" woman



Ewing and McCann (2003). $BMI \equiv kg/m^2$. Obesity rates are different in different places.

Table 2: Cities with Large and Small Percent of College Graduates in 2000

	(1)
Cities with the Largest Percentage of College Graduates in 2000	
San Francisco, CA	.436
Washington, DC–MD–VA–WV	.418
Columbia, MO	.417
Madison, WI	.406
San Jose, CA	.405
Bloomington, IN	.396
Fort Collins–Loveland, CO	.395
Raleigh–Durham–Chapel Hill, NC	.389
Gainesville, FL	.387
Champaign–Urbana, IL	.38
Bryan–College Station, TX	.37
Ann Arbor, MI	.369
Austin–San Marcos, TX	.367
State College, PA	.363
Bloomington–Normal, IL	.362
Seattle–Bellevue–Everett, WA	.359
Rochester, MN	.347
Santa Cruz–Watsonville, CA	.342
Denver, CO	.342
Trenton, NJ	.34

Cities with the Smallest Percentage of College Graduates in 2000
Jacksonville, NC
Beaumont–Port Arthur, TX
Hagerstown, MD
Stockton–Lodi, CA
Huntington–Ashland, WV–KY–OH
Modesto, CA
Altoona, PA
Ocala, FL
Hickory–Morganton–Lenoir, NC
Bakersfield, CA
Brownsville–Harlingen–San Benito, TX
Lima, OH
Yuba City, CA
McAllen–Edinburg–Mission, TX
Johnstown, PA
Mansfield, OH
Vineland–Millville–Bridgeton, NJ
Visalia–Tulare–Porterville, CA
Danville, VA

From Moretti (2004). College graduation rates are different in different places.

Why?

- Do places change people? Or do people sort? (Yes)
- Can we think about spatial equilibrium when everyone is different? (Yes)
- Will municipalities provide the right levels of public goods when people sort? (Maybe/Sometimes)

We'll tackle these three questions in turn.

Sprawl and obesity

- It's hard to define/measure urban sprawl.
- For almost any measure you use, people are heavier in more sprawling places.
- Do sprawling places make people obese, or do obese people move to sprawling places?

Eid et al. (2008) construct panel data reporting individual BMI and neighborhood, and then look at cross-sectional relationships and at what happens to weight when people move to more or less sprawling neighborhoods.

$$\begin{aligned} i, t &\sim \text{person, year} \\ BMI_{it} &\sim \text{Body Mass Index} \\ x_{it} &\sim \text{Demographics} \\ z_{it} &\sim \text{Sprawl} \end{aligned}$$

Sprawl is measured in two ways, ‘share of undeveloped land within 1km of residential address’, or ‘count of retail establishments within 1km’.

Consider the following description of the BMI process,

$$BMI_{it} = c_i + \beta x_{it} + \gamma z_{it} + u_{it}$$

Here, c_i is really important, it is the individual's mean BMI across all the years when they are observed. We want to know γ. Estimate the two regressions,

$$BMI_{it} = \beta x_{it} + \gamma z_{it} + u_{it}$$

This will mainly compare BMI to sprawl using cross-sectional variation. γ indicates whether people are heavier in more sprawling neighborhoods.

Next, first difference,

$$\begin{aligned} BMI_{it} &= c_i + \beta x_{it} + \gamma z_{it} + u_{it} \\ \underline{BMI_{it-1}} &= \underline{c_i + \beta x_{it-1} + \gamma z_{it-1} + u_{it-1}} \\ \implies \Delta BMI_{it} &= \beta \Delta x_{it} + \gamma \Delta z_{it} + \Delta u_{it} \end{aligned}$$

First differencing means all time-invariant individual characteristics, ‘the time invariant propensity for obesity,’ drop out. This regression compares how much BMI changes for people who move to how much it changes for people who don’t.

Here is what they find. ‘*’ indicates precision of the estimate.

$$\begin{aligned} BMI_{it} &= \beta x_{it} + 0.46^{*}\text{Sprawl}_{it} - 3.95^{***}\text{Mixed-use}_{it} \\ \Delta_t BMI_i &= \beta \Delta_t x_i - 0.04^{*}\Delta_t\text{Sprawl}_i + 0.50^{***}\Delta_t\text{Mixed-use}_i \end{aligned}$$

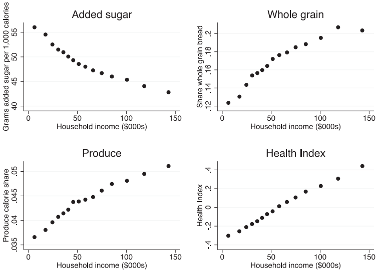
There is a strong relationship between BMI and neighborhood characteristics, but changes to neighborhood characteristics do not affect BMI. The entire cross-sectional relationship appears to be driven by sorting.

Food deserts and nutrition I

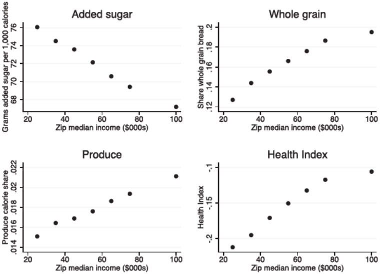
Allcott et al. (2019) look at how the availability of big grocery stores affects diet. Less formally, if you live in a 'food desert' does the entry of grocery store change your diet?

Answering this question requires data describing grocery purchases and the set of available grocery stores for a set of people over time.

- Scanner data tracks household grocery purchases by date and store. 169k Households, with some demographic information.
- Store level descriptions of sales and opening date.
- USDA nutrition data by product.



Use the scanner data to create household level measures of the healthfulness of grocery purchases. Wealthier people purchase healthier groceries, however you measure it.



Create a store level index of healthiness. Average over all products, per 1000 calories. Stores in wealthier neighborhoods sell healthier stuff, however you measure it.

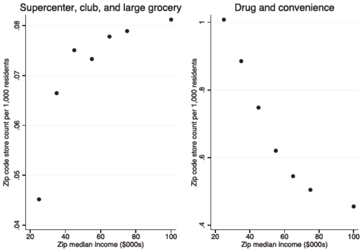


FIGURE III

Store Counts by ZIP Code Median Income

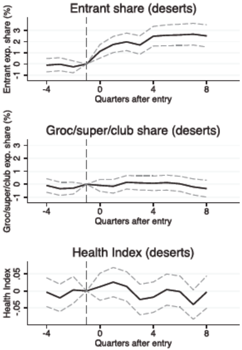
This figure presents counts of stores per 1,000 residents for the average ZIP code in each income category, using 2004–2016 ZIP Code Business Patterns data. Large (small) grocers are defined as those with 50 or more (fewer than 50) employees.

Use store level data to split stores into full service grocery stores ($>$ 50 employees) and convenience stores. Wealthy neighborhoods have more big grocery stores. Poor neighborhoods have more convenience stores.

Do people in poor neighborhoods have worse diets because they have worse stores? That is, are their diets harmed by the fact that they live in ‘food deserts’?

Define a food desert as a zipcode/year that does not contain a large grocery

Check what happens to people in food deserts when a full service grocery opens in their zipcode.



Top: New large grocery stores attract about 3% of all dollars spent on groceries within 15 minutes of a household's home. Middle: Grocery store expenditure share of food budget. Bottom: Healthfulness of household food purchases.

Living in a food desert probably does not have a big effect on your diet. Income does.

House prices and the value of schools I

Education is one of the more important services provided by local governments. In order to think about whether we are providing this service optimally, we would like to know how highly people value it. Given this sort of estimate, we could ask whether this value is above or below the relevant marginal cost.

- In the US, public schools are often determined by address. Your school choice is determined by the district you live in.
- Bayer et al. (2007) look at what happens around school district boundaries where the quality of the school changes.
- Confidential Census block level data (about 100 individuals/30 households). Six San Francisco counties.650,00 people, 242,100 households.

House prices and the value of schools II

- Self-reported house price/rent (actually, a more complicated imputation). Subsample of transaction data from Home Mortgage Disclosure Act (HMDA).
- School attendance zone boundaries.
- Mean school 4th grade math achievement test score.

Can we use these data to learn the value of improving test scores?

Recall that in spatial equilibrium, amenities are capitalized into land prices, so price discontinuities at attendance zone boundaries should tell us about the discontinuous change in school quality at the border.

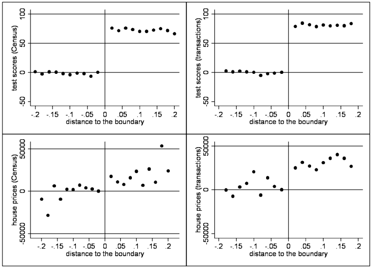


FIG. 1.—Test scores and house prices around the boundary. Each panel is constructed using the following procedure: (i) regress the variable in question on boundary fixed effects and on 0.02-mile band distance to the boundary dummy variables; (ii) plot the coefficients on these distance dummies. Thus a given point in each panel represents this conditional average at a given distance to the boundary, where negative distances indicate the low test score side.

Test scores (top) and House prices (bottom) using self-reported/census house prices (left) and transaction data (right). (This validates census price data.)

House prices increase with test scores at attendance zone boundaries.

Ratio of gap in bottom to top should give unit value of attendance zone test scores.

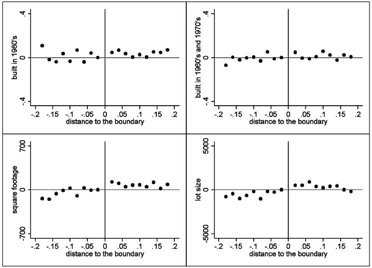


FIG. 3.—Transactions data housing characteristics around the boundary. Each panel is constructed using the following procedure: (i) regress the variable in question on boundary fixed effects and on 0.02-mile band distance to the boundary dummy variables; (ii) plot the coefficients on these distance dummies. Thus a given point in each panel represents this conditional average at a given distance to the boundary, where negative distances indicate the low test score side.

Maybe the attendance zone boundaries are drawn to separate the nice neighborhoods from the crummy ones? House age, house size, and lot size look the same on both sides of the border.

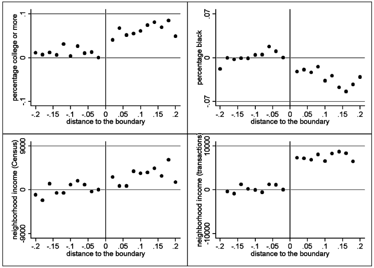


FIG. 4.—Neighborhood sociodemographics around the boundary. Each panel is constructed using the following procedure: (i) regress the variable in question on boundary fixed effects and on 0.02-mile band distance to the boundary dummy variables; (ii) plot the coefficients on these distance dummies. Thus a given point in each panel represents this conditional average at a given distance to the boundary, where negative distances indicate the low test score side.

Bayer et al. (2007) Maybe the people are different? The good school side of the border is richer, more educated, whiter than the bad school side of the district.

This is a problem for this research design. What if people pay more to live on the high score side of the boundary to be around better educated/richer/whier people? Then this doesn't tell us about the value of schooling at all.

What if the high scores just reflect the fact they are working with an easier population? Children of richer, more educated parents do better in school. Maybe the schools are worse once we account for this?

Moving to Opportunity (MTO) and adult wages I

The Moving To Opportunity experiment was adminstered by the US Dep't of Housing and Urban Development (HUD) between 1994-8 in five cities; Chicago, NY, LA, Boston and Baltimore. It is one of the high points of social science research over the past two generations (I think).

Low income families living in public housing projects or other subsidized housing with children were offered the chance to participate in an experiment.

If they agreed they were randomly assigned to one of three treatments,

1. (Experiment) Housing subsidy valid only if they moved to a neighborhood with poverty rate $< 10\%$.
2. Regular Section 8 housing voucher (more later).
3. Control group. Keep current housing and get housing counselling.

All HH were initially in subsidized housing and required to pay 30% of income towards rent. In experimental and section 8 arms, they received a top-up above 30% of income (up to a 'fair market rent' ceiling).

4604 households participated, 15,892 people. Of these, 11,276 were children, and of these, 8603 were born before 1991, and were at least three at the time of the experiment.

A typical particpating household was headed by a young Hispanic or Black single mother without a high school diploma, and was very poor.

Initial analyses of the experiment conducted two years afterward found little effect on test scores, though treated children were healthier (Katz et al., 2001).

Chetty et al. (2016) links MTO children to their social security records to find out what happens to them when they are grown up, between 2008-2012. This is 10-14 years later.

They do two main regressions. To describe them, we need notation,

$$
\begin{aligned}
y_i &\sim \text{outcome for child } i \\
EXP_i &\sim \text{indicator if child is offered experimental treatment} \\
S8_i &\sim \text{indicator if child is offered section 8 treatment} \\
TakeEXP_i &\sim \text{indicator if child is offered and} \\
&\quad\ \text{accepts experimental treatment} \\
TakeS8_i &\sim \text{indicator if child is offered and accepts section 8 treatment}
\end{aligned}
$$

They also worry about city specific and child specific observables, but I'll ignore this in the interests of brevity.

Aside: People for whom $TakeEXP_i = 1$ are sometimes called 'compliers' becasue they are complying with the experimental incentives. Conversely, people for whom $TakeEXP_i = 0$ are 'deniers'.

Chetty et al. (2016) two main estimations,

$$y_i = \alpha + \beta_E^{ITT} EXP_i + \beta_S^{ITT} S8_i + \varepsilon_i$$

This gives us the effect of being offered the treatment and is estimated with OLS. This is called an 'intent to treat' effect. It's the effect on the people you randomly select for the experimental arm.

β_E^{ITT} is 'too small' because not everyone offered the experiment, takes it up. Some stay in their current housing or do something else.

The second procedure corrects for this problem by doing an 'instrumental variables estimation', using experiment to instrument for the outcome. The details of this are outside the scope of this class. These estimates are sometimes called 'the effect of treatment on the treated'.

$$y_i = \alpha + \beta_E^{TOT}\, TakeEXP_i + \beta_S^{TOT}\, TakeS8_i + \varepsilon_i$$

Loosely, we estimate the TOT effect as the ratio of the effect of the experiment on 'offered and accepted' by the effect of the experiment on 'accepted'. In words, divide the ITT estimate by share accepting treatment.

Table 2—First-Stage Impacts of MTO on Voucher Take-Up and Neighborhood Poverty Rates (*Percentage Points*)

	Housing voucher take-up	Poverty rate in tract one year post- RA		Mean poverty rate in tract post-RA to age 18		Mean poverty rate in zip post-RA to age 18	
		ITT	TOT	ITT	TOT	ITT	TOT
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Children < age 13 at random assignment							
Exp. versus control	47.66***	−17.05***	−35.96***	−10.27***	−21.56***	−5.84***	−12.23***
	(1.653)	(0.853)	(1.392)	(0.650)	(1.118)	(0.425)	(0.752)
Sec. 8 versus control	65.80***	−14.88***	−22.57***	−7.97***	−12.06***	−3.43***	−5.17***
	(1.934)	(0.802)	(1.024)	(0.615)	(0.872)	(0.423)	(0.622)
Observations	5,044	4,958	4,958	5,035	5,035	5,035	5,035
Control group mean	0	50.23	50.23	41.17	41.17	31.81	31.81
Panel B. Children age 13–18 at random assignment							
Exp. versus control	40.15***	−14.00***	−34.70***	−10.04***	−24.66***	−5.51***	−13.52***
	(2.157)	(1.136)	(2.231)	(0.948)	(1.967)	(0.541)	(1.113)
Sec. 8 versus control	55.04***	−12.21***	−22.03***	−8.60***	−15.40***	−3.95***	−7.07***
	(2.537)	(1.078)	(1.738)	(0.920)	(1.530)	(0.528)	(0.921)
Observations	2,358	2,302	2,302	2,293	2,293	2,292	2,292
Control group mean	0	49.14	49.14	47.90	47.90	35.17	35.17

Top panel is children younger than 13 when they were offered the experiment. Bottom panel is older children.

Section 8 and experimental treatments moved to neighborhoods with MUCH lower poverty rates. The experimental treatment is a larger than the section 8 effect.

The experiment ‘worked’ it randomly assigned poor children to better neighborhoods. This is true for both age groups.

Note that take-up rates are lower for the experimental than the section 8 group, and lower for families with older children. This can be a problem for the TOT estimates if the sets of compliers are not representative of the treated set.

Notice how close the TOT estimates are to ITT divided by the take-up rate.

Table 3—Impacts of MTO on Children's Income in Adulthood

	W-2 earnings ($) 2008–2012 ITT	Individual earnings 2008–2012 ($)			Individual earnings ($)		Employed (%) 2008–2012 ITT	Hhold. inc. ($) 2008–2012 ITT	Inc. growth ($) 2008–2012 ITT
		ITT	ITT w/ controls	TOT	Age 26 ITT	2012 ITT			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Children < age 13 at random assignment									
Exp. versus control	1,339.8**	1,624.0**	1,298.9**	3,476.8**	1,751.4*	1,443.8**	1.824	2,231.1***	1,309.4**
	(671.3)	(662.4)	(636.9)	(1,418.2)	(917.4)	(665.8)	(2.083)	(771.3)	(518.5)
Sec. 8 versus control	687.4	1,109.3	908.6	1,723.2	551.5	1,157.7*	1.352	1,452.4**	800.2
	(698.7)	(676.1)	(655.8)	(1051.5)	(888.1)	(690.1)	(2.294)	(735.5)	(517.0)
Observations	8,420	8,420	8,420	8,420	1,625	2,922	8,420	8,420	8,420
Control group mean	9,548.6	11,270.3	11,270.3	11,270.3	11,398.3	11,302.9	61.8	12,702.4	4,002.2
Panel B. Children age 13–18 at random assignment									
Exp. versus control	−761.2	−966.9	−879.5	−2,426.7	−539.0	−969.2	−2.173	−1,519.8	−693.6
	(870.6)	(854.3)	(817.3)	(2,154.4)	(795.4)	(1,122.2)	(2.140)	(11,02.2)	(571.6)
Sec. 8 versus control	−1,048.9	−1,132.8	−1,136.9	−2,051.1	−15.11	−869.0	−1.329	−936.7	−885.3
	(932.5)	(922.3)	(866.6)	(1,673.7)	(845.9)	(1213.3)	(2.275)	(11,85.9)	(625.2)
Observations	11,623	11,623	11,623	11,623	2,331	2,331	11,623	11,623	11,623
Control group mean	13,897.1	15,881.5	15,881.5	15,881.5	13,968.9	16,602.0	63.6	19,169.1	4,128.1

Top panel is children younger than 13 when they were offered the experiment. Bottom panel is older children.

Younger children have higher adult wages in the experimental arm. Section 8 also looks positive, but is probably smaller than the experimental treatment.

The effect on older children is negative, but not distinguishable from zero.

These results seem to line up qualitatively with the Roca and Puga (2017) finding that spending time in a big city increases a person's wage. It looks like where you live affects labor market outcomes in these data, also.

Do these results suggest that we should tear down neighborhoods where children don't succeed?

Sorting vs Causation

- People are different in different places.
- Sometimes, these differences appear to be driven by the sorting of different people into different locations, e.g. obesity and sprawl, diet and food deserts.
- Sometimes, it looks like people are sorting to be near other people, school quality, race education and income.
- Sometimes, it looks like places actually change people,e.g., MTO wages for young children, learning in cities, and sometimes they don’t, e.g., MTO wages for older children.

So, people are different in different places, and sometimes it's because people sort and sometimes it's because places change people.

If you want to know which is happening in any particular case, you really need to implement a quasi-experimental (or experimental) research design to check.

We don't have any theory to help us understand which characteristics are subject to change by place of residence, and which are not. This is a question that researchers have not even begun to address.

Sorting and spatal equilibrium

The world is complicated in two ways that we have not yet tried to address with our theory

- People are different in lots of important and interesting ways.
- They sort across locations at least partly on the basis of these observations
- Some of the place specific attributes that people sort on are provided or influenced by local governments, e.g., sprawl, public schools, grocery stores. Some of these place specific goods seem like public goods.

This invites two questions

- Is this complicated process consistent with our basic notion of spatial equilibrium? (Yes, but not too surprisingly, it's a little messy)
- What incentives do local government have to provide these 'local public goods'.

Tiebout sorting and public finance

Many of the services that local governments provide look like public goods; police, fire, schools, roads, transit, water, electricity, gas, trash collection.

These goods are not ‘excludable’. That is, it’s hard or impossible to deny them to anyone in the service area. This makes it difficult to charge people prices that reflect the marginal cost of the services.

These good are typically financed with location specific taxes, but there are lots of other possibilities, sales taxes, excise taxes on cars and other property, gas taxes.

This creates a problem. The collection of tax revenue and the provision of local public goods are not as tightly connected as for private goods. If a restaurant gives me a bad meal, I can choose not to go back. If the city doesn't pick up my trash, I can't withhold my property tax payment.

In a classic paper Tiebout (1956) makes the argument that people can 'vote with their feet' and move away from municipalities that do not provide good value for money, that is, good public services per dollar of tax revenue.

Once we allow this sort of mobility, the provision of local public goods looks more like a private good. If a restaurant is bad, I don't go back. If a municipality is bad, I move away.

This means that any municipality that does not provide

- A bundle of public services that consumers want.

- Produce them in the cost minimizing way

is going to see all of its residents move away.

This has three really interesting implications.

- We should expect to see optimal provision of local public goods. If you compare this to the pure theory of public goods, this is a remarkable and really neat conclusion.
- The effect of a marginal change in the property tax rate should be zero. Why? If services are provided optimally, then their marginal cost exactly equals their marginal value to households, and so the change in public services and the change in property tax exactly offset each other.

- We should expect to see municipalities specialize in serving populations with different tastes for public goods. There should be communities that have high taxes and lots of public services and communities that have low taxes and fewer services.

Tiebout wrote his paper in 1956, before the profession began to rely so heavily on mathematical models. In order to understand how the Tiebout's model works a little better, it's helpful to think about it in the context of a spatial equilibrium model with heterogenous agents.

A simple model of heterogenous agents

This discussion follows Chapter 8 of Sieg (2020) pretty closely.

Let's think about a population of agents with preferences over consumption, housing, and a local public good.

Introduce the following notation

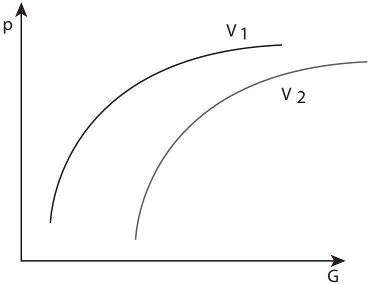
$$
\begin{aligned}
i &= 0, 1 \text{municipality index} \\
G_i &\sim \text{Local public good in municipality } i \\
w &\sim \text{income} \\
p_i &\sim \text{price of housing}
\end{aligned}
$$

People have different incomes, w, but are otherwise the same. For completeness, let c and h be consumption and housing.

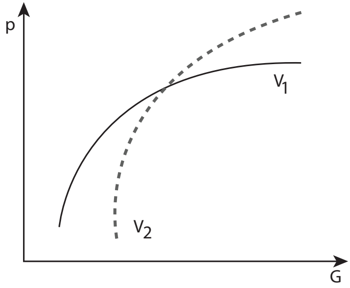
Describe preference with an indirect utility function,

$$V(G, p, w, \alpha) = -\left[\alpha G^{\rho} + \left(\frac{w}{p^{\beta}}\right)^{\rho}\right]$$

$\beta \in (0, 1)$ measures the strength of the taste for housing. $\rho < 0$ describes the willingness to substitute between the public good and the other goods, and α measures the strength of the preference for public goods.



This is a complicated looking indirect utility function. But, note the minus sign and the negative value of ρ, so, V decreases in the price of housing, and increases in w and G just as it should. The figure shows two indifference curves in (p, G) space. $V_2 > V_1$.



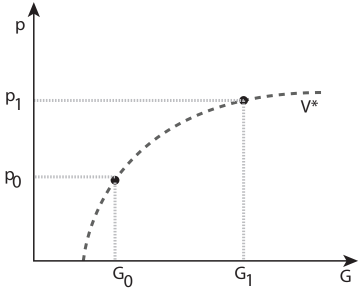
This class of sorting models assumes that indirect utility functions satisfy a ‘single crossing’ property. In math, this property says that indifference curves in (p, G) space get steeper as w increases. This is illustrated in the figure.

Fix a household’s consumption of housing. Then an increase in p translates to a decrease in c of ph. Thus, the slope of this indifference curve tells us the willingness to trade c for G. As w goes up people will trade more c per unit of G.

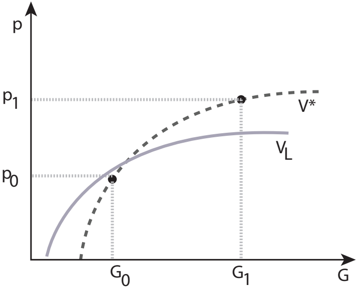
The single-crossing condition just says that this increase has to be pretty uniform for all possible values of G and w.

Suppose we have a set of people whose wages are uniformly distributed between $[\underline{w}, \overline{w}]$. (if you don't know what this means, it is close to saying 'there are the same number of people with each possible wage' but adjusted to the fact that $[\underline{w}, \overline{w}]$ is a continuum.)

We would like to divide these people between two municipalities offering (p_1, G_1) and (p_0, G_0). Suppose that municipality 0 is the low service, low tax municipality, so that $p_1 > p_0$ and $G_1 > G_0$.



Suppose we can find $\widehat{w}$ such that this household is exactly indifferent between the two municipalities. Note, this need not be possible. What if $p_1 > p_0$ and $G_1 < G_0$?



Under the single crossing condition, all households with $w > \widehat{w}$ choose municipality 0 and conversely. Recall, the 'preferred set' is down and to the right.

and we've done it. We've constructed a spatial equilibrium in which there are a continuum of different types of agents.

But if you think carefully, about it, there are still a number of questions.

- How does this model reflect Tiebout equilibrium? As a municipality offers 'worse' combinations of (p, G) fewer agents will want to choose it. This is just Tiebout sorting.
- 'spatial equilibrium' is a different concept in this model than we have used up to now. Here, an equilibrium is where everyone chooses their favorite from $\{V_1, V_2\}$. Up until now, it has been, everyone gets the same utility everywhere. Introducing heterogeneous agents changes the equilibrium concept.

- We started talking about taxes and public goods. This model is about house prices and public goods. Implicitly, the house prices in this model are after tax. That is, $p_i = (1 + \tau_i)\widetilde{p}_i$, where τ and $\widetilde{p}$ are the property tax rate and the before tax price of housing.
- The model doesn't describe where p comes from, as the monocentric city model does. We need to describe the housing market to understand this. Suppose that there are exactly as many housing units as there are people, evenly divided between the two municipalities. Then, given G_0 and G_1, the before tax prices need to adjust so that $\widetilde{w}$ is $\frac{\overline{w}+\underline{w}}{2}$ in order to get the housing market to clear.

- In the monocentric city model, agents are all the same, so the all get exactly the same utility. In this model, prices are the same for everyone, even though their utility is different. Some people will get surplus in equilibrium.
- This makes evaluating welfare much more complicated in these models. Indeed, research using these models often disregards land rent altogether in welfare calculations.

Empirical evidence I

- Bayer et al. (2007) show that property markets capitalize school quality (or peer effects) and that people sort across school districts.
- Dachis et al. (2012) suggest that changes in the property tax rate in municipal Toronto were almost completely capitalized into real estate prices.
- Rosen (1982) finds that house prices almost completely capitalize the property tax decrease that came with California's Proposition 13 tax cuts.
- Coury et al. (2021) find that constructing municipal sewer and piped water supplies in late 19th century Chicago increased land prices by about 60 times the cost of construction.

So, where does this leave us?

Empirical evidence II

- It is clear that property prices reflect local service quality. But this prediction is common to all of the models we've studied this term.
- It is also clear that people sort on the basis of unobservable and observable characteristics, and that this is economically important. The model with heterogenous agents is really the only one we've studied that can make this prediction.

Empirical evidence III

- The Tiebout hypothesis does not hold strictly. Some tax changes, e.g. Prop 13, are valued as if the associated loss in services has no value. In the case of Proposition 13, this strains credibility, it seems to have really hurt California's schools. Some changes, e.g., sewers in Chicago, look really cost effective. We are not seeing that tax changes have zero effect on property prices, or at least not very often.

I think the take-away here is that sorting and capitalization are important, and surely put some pressure on municipalities to provide good services cheaply, such sorting does not seem sufficient to result in an equilibrium where municipalities are really optimizing.

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