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Essays on the Economics of People and Places

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This dissertation contains three essays that examine the long-run consequences of economic and social processes on people, places, and the economy. The first chapter examines the long-run effects of the 1980–1982 recession on education and income. Using confidential Census data, I estimate difference-in-differences regressions that exploit variation across counties in recession severity and across cohorts in age at the time of the recession. For individuals aged 0–10 in 1979, a 10 percent decrease in earnings per capita in their county of birth reduces four-year college degree attainment by 9 percent and income in adulthood by 3 percent. I find little evidence that states with more generous or more progressive transfer systems mitigated these long-run effects. Simple calculations suggest that, in aggregate, the 1980–1982 recession led to 1–3 million fewer college graduates and $64–$145 billion less earned income per year.

The second chapter, with Evan Taylor, examines the effects of birth town migration networks on location decisions. We study over one million long-run location decisions made during two landmark migration episodes by African Americans from the South and whites from the Great Plains. We develop a new method to estimate the strength of migration networks for each receiving and sending location. Our estimates imply that when one randomly chosen African American moves from a birth town to a destination county, then 1.9 additional black migrants make the same move on average. For white migrants from the Great Plains, the average is only 0.4. Networks were particularly important in connecting black migrants with attractive employment opportunities and played a larger role in less costly moves.

The third chapter, also with Taylor, estimates the effect of social connectedness on crime across cities from 1970 to 2009. Migration networks among African Americans from the South generated variation across destinations in the concentration of migrants from the same birth town. Using this novel source of variation, we find that social connectedness considerably reduces murders, rapes, robberies, assaults, burglaries, larcenies, and motor vehicle thefts, with a one standard deviation increase in social connectedness reducing murders by 23 percent and motor vehicle thefts by 18 percent. Social connectedness especially reduces murders of adolescents and young adults committed during gang and drug activity.

Chapter 1

The Long-Run Effects of Recessions on Education and Income

Recessions receive tremendous attention from economists, policymakers, and the public. Most of this attention focuses on the short-run effects of recessions on workers' labor market outcomes and firms' investment and employment decisions. In addition to these short-run effects, recessions could have consequential long-run effects, especially if they affect human capital attainment. Influential theoretical and empirical work emphasizes the role of opportunity cost: by reducing labor market opportunities, recessions might increase educational attainment (e.g., Atkin 2016; Becker 1962; Black, McKinnish, and Sanders 2005; Cascio and Narayan 2015; Charles, Hurst, and Notowidigdo 2018).

The opportunity cost channel is most relevant for individuals making high school and college enrollment decisions during the recession. However, recessions could affect younger individuals, for whom mechanisms besides opportunity cost, such as a decrease in human capital investments during childhood, might dominate. Consequently, the size and even sign of long-run effects are an empirical question. This paper estimates the long-run effects of recessions on education and income, for individuals who were children, adolescents, and young adults when the recession began.

I focus on the 1980–1982 double-dip recession in the United States, which followed large increases in interest rates and the price of oil. The recession was concentrated in certain industries, like durable goods manufacturing and wood products, and counties with preexisting specialization in these industries experienced a more severe recession. The 1980–1982 recession is a valuable setting because its timing permits the study of counties' prerecession economic conditions and individuals' long-run outcomes. I show that the recession led to a persistent relative decrease in earnings per capita and median family income in negatively affected counties. This persistent decrease in local economic activity is central in considering the long-run effects on individuals.

I estimate the long-run effects of the 1980–1982 recession using newly available confidential data. I link the 2000 Census and 2001–2013 American Community Surveys to the Social Security Administration NUMIDENT file, which allows me to observe outcomes in adulthood and county of birth for 23 million individuals born between 1950 and 1979. With these data, I estimate generalized difference-in-differences regressions that compare education and income in adulthood of individuals born in counties with a more versus less severe recession (first difference) and individuals who were younger versus older when the recession began (second difference). I isolate the effect of local labor demand shifts that emerged during the recession by instrumenting for the 1978–1982 change in log real earnings per capita,

with the log employment change predicted by the interaction of a county's preexisting industrial structure and aggregate employment changes (Bartik 1991). This empirical strategy identifies the overall effect of the recession on long-run outcomes, inclusive of any mitigating actions by parents and communities.

I find that the 1980–1982 recession led to sizable long-run reductions in education and income. For individuals aged 0–10 in 1979, a 10 percent decrease in real earnings per capita in their county of birth, which is around one standard deviation, leads to a 3.0 percentage point (9.4 percent) decrease in four-year college degree attainment. The negative effects on college graduation are most severe and essentially constant for individuals aged 0–13 in 1979. This age profile suggests that the underlying mechanisms are a decline in childhood human capital or a long-term decline in parental resources to pay for college. Because I estimate small and statistically insignificant effects on college graduation for individuals aged 14–19 in 1979, short-term credit constraints in paying for college appear less important. I find little evidence of an effect on college attendance or high school graduation. For individuals aged 0–10 in 1979, a 10 percent decrease in real earnings per capita leads to a $1,300 (3.2 percent) decrease in earned income and a 1.7 percentage point (13.9 percent) increase in the probability of living in poverty. Simple calculations and additional regressions suggest that much of the long-run effects on income stem from the effects on education.

An important question is whether government policies mitigated these long-run effects. To examine this, I characterize two relevant dimensions of state transfer systems as of 1970. First, I measure transfer generosity as the amount of transfers per capita, conditional on standard economic and demographic variables. Second, I measure transfer progressivity as the degree to which states transferred higher amounts to poorer counties, conditional on standard variables. I find little evidence that states with more generous or more progressive transfer systems mitigated the long-run effects.

Several pieces of evidence support the validity of my empirical strategy. First, I show that earnings per capita evolved similarly before 1980 in counties with a more versus less severe recession. Second, I find no evidence of a relationship between the severity of the recession and the evolution of maternal education, infant birth weight, or infant mortality before 1980. Finally, I conduct falsification tests by estimating the effect of the recession on education for individuals aged 23–28 in 1979, using 29-year-olds as a comparison group. I find no evidence of an effect for 23–28-year-olds, who largely completed their schooling before the recession. These findings strongly suggest that my estimates reflect the long-run effects of the 1980–1982 recession.

The magnitude of my estimates and the large number of affected individuals suggest that the 1980–1982 recession

depresses aggregate economic output today. To gauge the aggregate effects, I scale my estimates by the 105 million individuals born in the United States from 1951 to 1979. Depending on the assumed evolution of earnings per capita in the absence of the recession, back of the envelope calculations suggest that the 1980–1982 recession led to 0.9–2.1 million fewer four-year college graduates, $64–$145 billion less earned income per year (as of 2000–2013), and 0.5–1.3 million more adults living in poverty each year. These numbers represent 1–3 percent of the stock of college-educated adults in 2015, 0.4–0.8 percent of 2015 GDP, and 1–3 percent of the number of individuals in poverty in 2015. While these calculations could understate or overstate the true aggregate effects, as I discuss in detail, the long-run effects that I document clearly are important channels through which recessions affect welfare and economic growth.

This paper shows that the 1980–1982 recession persistently decreased earnings per capita in negatively affected counties, and children and adolescents born in these counties have less education and income as adults. In addition, I show that every U.S. recession since 1973 has persistently decreased earnings per capita in negatively affected counties. This novel stylized fact indicates that the 1980–1982 recession is not unique in its persistent effects on county-level earnings per capita, which suggests that it might not be unique in its long-run effects on human capital attainment. Similar long-run effects could arise from other shocks leading to persistent declines in local economic activity, such as Chinese import competition (Autor, Dorn, and Hanson 2013) and NAFTA (McLaren and Hakobyan 2016).

This paper contributes to four distinct literatures. First is the literature on the effect of labor market opportunities on contemporaneous schooling enrollment, with a focus on the role of opportunity cost (Atkin 2016; Black, McKinnish, and Sanders 2005; Cascio and Narayan 2015; Charles, Hurst and Notowidigdo 2018). I complement this literature by estimating effects on a wider age range, including children and adolescents, which is possible because of newly available data.

Second is the literature on the role of early life conditions in shaping human capital and productivity (for recent reviews, see Almond, Currie, and Duque [2017] and Heckman and Mosso [2014]). This literature provides very little direct evidence on the long-run effects of recessions on education and income. Furthermore, Chetty and Hendren (2018) find little relationship between local economic conditions and the effect of a place on children's long-run outcomes. As a result, considerable uncertainty exists about the magnitude and sign of recessions' long-run effects. Evidence on the size of these effects is necessary for weighing the potential benefits of mitigating policies, and for assessing whether these long-run effects should be incorporated into theoretical and empirical models of recessions and the economy. My primary contribution, facilitated by newly available data, is estimating the long-run effects of a reces-

sion on education and income. I find that the long-run effects on children's and adolescents' income are more severe than the effects on individuals entering, or already in, the labor market at the time of the recession.[1]

This paper also contributes to work on the welfare costs of recessions. Most of this literature focuses on the costs of intertemporal substitution in consumption (see the review in Lucas [2003]). In contrast, I study costs that stem from long-run declines in children's and adolescents' human capital attainment. My results imply that recessions are costlier than previously thought and point to new possible targets for social insurance and economic stabilization policies. Finally, this paper contributes to the literature studying how recessions affect subsequent economic activity. An influential strand of this literature examines recessions' cleansing effects, which increase productivity as less productive firms exit and resources are reallocated to remaining firms (e.g., Davis, Haltiwanger, and Schuh 1996; Foster, Grim, and Haltiwanger 2016; Schumpeter 1939). My results demonstrate that recessions also affect productivity in a markedly different way, by reducing children's and adolescents' human capital attainment.

Chapter 2

Social Interactions and Location Decisions: Evidence from U.S. Mass Migration

(with Evan Taylor)

Theoretical and empirical research emphasizes the role of expected real wages, amenities, and moving costs in individuals' location decisions (e.g., Kennan and Walker 2011; Sjaastad 1962). While theory suggests that social networks might matter as well (Carrington, Detragiache, and Vishwanath 1996), estimating the importance of this factor has proven difficult because of a lack of suitable data sets and research designs. For example, it is well known that immigrants from the same country tend to live in the same place, but this fact does not distinguish between the role of social networks and numerous common factors, such as moving costs, human capital, and language. Evidence on the effects of social networks on location decisions would inform our understanding of past and future migration episodes, the equilibration of local labor markets, and the impacts of policies that affect migration incentives. Furthermore, social networks might continue to attract migrants to their chosen destination for many years, thus limiting adjustments as economic conditions change, and ultimately contributing to spatial mismatch.

This paper provides new evidence on the effects of social networks on location decisions. We focus on the mass migrations in the mid-twentieth century of African Ameri-

cans from the U.S. South and whites from the Great Plains. We proxy for social networks using birth towns, which are particularly relevant in this setting, and we use administrative data that measure town of birth and county of residence at old age for most of the U.S. population born from 1916 to 1936. Our setting and data provide a unique opportunity to study the long-run effects of migration networks. We use detailed geographic information to distinguish the effect of birth town migration networks from other determinants of location decisions, such as moving costs determined by geography or railroad lines. For example, we observe that 51 percent of African American migrants born from 1916 to 1936 in Pigeon Creek, Alabama, moved to Niagara County, New York, while less than 6 percent of black migrants from nearby towns moved to the same county. This comparison underlies our research design, which asks whether individuals born in the same town are more likely to live in the same destination in old age than individuals born in nearby towns.

We combine this transparent research design with a new method of characterizing birth town migration networks. Our new parameter, which we call the network index, allows us to estimate the effect of migration networks on location decisions for each receiving and sending location and then relate these estimates to locations' economic characteristics. We show that existing methods may mischaracterize the effect of migration networks in our setting. In particular, the influential approach of Bayer, Ross, and Topa (2008) could estimate strong effects for popular destinations, even if true effects are relatively weak, and as a result could misstate the overall effect of networks. Our method does not suffer from this problem. Under straightforward and partly testable assumptions, the network index identifies the effect of birth town migration networks and maps directly to structural network models.

We find that migration networks strongly influenced the location decisions of Southern black migrants. Our estimates imply that when one randomly chosen African American moves from a birth town to a destination county, then 1.9 additional black migrants make the same move on average. Migration networks drew African Americans to destinations with a higher share of 1910 employment in manufacturing, a particularly attractive sector for black workers in our sample. This evidence highlights an important role for migration networks in providing job referrals or information about employment opportunities. We also find that networks drew black migrants to destinations that were closer and more connected by railroads, pointing to the importance of access to information and low moving costs in the functioning of these networks. In addition, networks are stronger in sending counties with higher literacy rates in 1920, suggesting that education and related factors facilitated network development.

We estimate weaker effects of migration networks on the location decisions of whites. For migrants from the Great Plains, our results imply that when one randomly chosen

migrant moves from a birth town to a destination county, then 0.4 additional white migrants make the same move on average. Results for Southern white migrants are similarly small. Furthermore, migration networks among whites are less sensitive to employment opportunities and moving costs. There are many possible explanations for the different effects of networks on black and white migrants. Given the myriad unobserved differences between these groups, this paper does not attempt to explain the black-white gap. However, one explanation supported by historical context and our results is that black migrants relied more heavily on their networks to overcome discrimination in labor and housing markets and a lack of financial resources.

To further study the role of migration networks, we map the network index to a structural model that generalizes the model in Glaeser, Sacerdote, and Scheinkman (1996). We estimate that 34 percent of Southern black migrants and 13 percent of Great Plains white migrants chose their long-run destination because of migration networks. In the absence of networks, Chicago would have 29 percent fewer Southern black migrants, and Los Angeles, Detroit, Philadelphia, and Baltimore would have 11–25 percent fewer black migrants. Eliminating migration networks would reduce the number of Great Plains white migrants in several places in California, including Los Angeles, Bakersfield, and Fresno. While our model does not account for all possible general equilibrium effects, the direction of these effects is not clear: reducing migration from a town to a county could make that destination more attractive because of higher wages or lower housing costs, or less attractive because of fewer individuals with a similar background. Still, the model suggests that migration networks had important effects on the U.S. population distribution.

We use the structural model to examine whether migrants would live in destinations with better economic opportunities in the absence of networks, as could occur if networks contributed to spatial mismatch. In the absence of migration networks, Southern black migrants would live in counties with a slightly smaller African American population share, unemployment rate, and poverty rate, while Great Plains white migrants would live in counties that are nearly identical. Migration networks have little effect on destination characteristics because migrants who did not follow their network moved to similar destinations.

Several pieces of evidence support the validity of our empirical strategy. Our research design, which compares the location decisions of migrants from nearby towns, implies that destination-level network index estimates should not change when controlling for birth town characteristics, because geographic proximity controls for the relevant determinants of location decisions. Reassuringly, our estimates are essentially unchanged when adding several covariates. We also estimate strong network effects in certain locations,

like Rock County, Wisconsin, for which qualitative work supports our findings (Bell 1933; Wilkerson 2010).

This paper makes three contributions. First, we develop a new method of characterizing migration networks. Our approach integrates previous work by Bayer, Ross, and Topa (2008) and Glaeser, Sacerdote, and Scheinkman (1996). It has desirable theoretical and statistical properties and can be used to study networks in other settings and for outcomes besides migration. Second, we provide new evidence on the importance of birth town migration networks and the types of individuals and economic conditions for which networks are most important.[2] Previous work shows that individuals tend to migrate to the same place, often broadly defined, as other individuals from the same town or country, but does not isolate the role of social networks in the decision of where to move (Bartel 1989; Bauer, Epstein, and Gang 2005; Beine, Docquier, and Ozden 2011). Third, our results inform landmark migration episodes that have drawn interest from economists for a century (e.g., Boustan 2010; Chay and Munshi 2015; Collins 1997; Hornbeck 2012; Margo 1990; Scroggs 1917).

Chapter 3

The Effect of Social Connectedness on Crime: Evidence from the Great Migration

(with Evan Taylor)

For almost 200 years, the enormous variance of crime rates across space has intrigued social scientists and policy-makers (Guerry 1833; Quetelet 1835; Weisburd, Bruinsma, and Bernasco 2009). Standard covariates explain relatively little of the cross-city variation in crime, which suggests a potential role for social influences (Glaeser, Sacerdote, and Scheinkman 1996). One possible explanation is peer effects, whereby an individual is more likely to commit crime if his or her peers commit crime (e.g., Case and Katz 1991; Damm and Dustmann 2014; Glaeser, Sacerdote, and Scheinkman 1996). A nonrival explanation is that cities differ in the degree of social connectedness, or the strength of relationships between individuals, including those unlikely to commit crime.

There is widespread interest in the effects of social connectedness and the related concept of social capital. This interest partly stems from the possibility that relationships between individuals can address market failures and generate desirable outcomes that are difficult to accomplish with government policies. However, estimating the effects of social connectedness and social capital has proved to be challenging. Some of the most influential evidence comes from correlations between outcomes, such as income and crime, and proxies for social capital, like individuals' partic-

ipation in community organizations, their stated willingness to intervene in the community, and their stated willingness to trust others (Putnam 2000; Sampson, Raudenbush, and Earls 1997). These proxies for social capital reflect individuals' contemporaneous decisions to invest in their communities, which raises the concern that these correlations reflect reverse causality or omitted variables bias. As a result, the empirical importance of social capital continues to be debated (Durlauf 2002).

This paper uses a new source of variation in social connectedness to estimate its effect on crime. Migration networks among millions of African Americans who moved out of the South between 1915 and 1970 generated variation across destinations in the concentration of migrants from the same birth town. For example, consider Beloit, Wisconsin, and Middletown, Ohio, two cities similar along many dimensions, including the total number of Southern black migrants that moved there. Around 18 percent of Beloit's black migrants came from Pontotoc, Mississippi, while less than 5 percent of Middletown's migrants came from any single town. Historical accounts trace the sizable migration from Pontotoc to Beloit to a single influential migrant, John McCord, getting a job in 1914 at a manufacturer in search of workers. Furthermore, ethnographic and newspaper accounts suggest that Southern birth town networks translated into strong community ties in the North. Guided by a simple economic model, we proxy for social connectedness using a Herfindahl-Hirschman Index of birth town to destination city population flows for African Americans born in the South between 1916 and 1936, whom we observe in the Duke SSA/Medicare data set. We focus on social connectedness among black migrants because birth town migration networks are especially strong among this group (Chapter 2), and qualitative and quantitative evidence supports our empirical strategy.

We estimate regressions that relate cross-city differences in crime from 1970 to 2009 to cross-city differences in social connectedness. The historical literature suggests that, conditional on economic and social opportunities, variation in social connectedness stems from idiosyncratic factors, like the right migrant being in the right place at the right time. To exploit this variation, we control for population, manufacturing employment (the largest sector in which African American migrants worked), and the black population share from 1920 to 1960. Our regressions also include the number of Southern black migrants who live in each city, to adjust for differences in the overall attractiveness of cities to black migrants, and for contemporaneous population, land area, and state-by-year fixed effects. City-level crime counts come from FBI Uniform Crime Reports.

We find that social connectedness leads to sizable reductions in crime rates. The elasticity of the crime rate with respect to social connectedness ranges from −0.09 to −0.26 across the seven index crimes of murder, rape, robbery,

assault, burglary, larceny, and motor vehicle theft, and is statistically distinguishable from zero for every crime. At the mean, a one standard deviation increase in social connectedness leads to a precisely estimated 22 percent decrease in murder, the best measured crime in FBI data. Our estimates imply that replacing Middletown's social connectedness with that of Beloit would decrease murders and robberies by 37 percent and motor vehicle thefts by 30 percent. By comparison, the estimates in Chalfin and McCrary (2018) imply that a similar decrease in murders would require a 55 percent increase in the number of police officers.

Because social connectedness arises from individuals' location decisions, a natural concern is whether our estimates reflect causal effects. The validity of our empirical strategy hinges on whether social connectedness is correlated with unobserved determinants of crime from 1970 to 2009, conditional on the covariates described above. Historical accounts emphasize the importance of migrants who were well connected in their birth towns and who worked for an employer in search of labor in establishing concentrated migration flows from Southern birth towns to Northern cities (Gottlieb 1987; Grossman 1989; Scott 1920). It is unlikely that these idiosyncratic factors are correlated with unobserved determinants of crime some 50 years later. These considerations provide qualitative support for our empirical strategy.

We marshal a wide range of quantitative support for our empirical strategy. First, 79 percent of the variation in social connectedness stems from a single birth town-to-destination city migration flow. This agrees with historical accounts that emphasize the importance of the right migrant being in the right place at the right time. Second, social connectedness is not correlated with murder rates from 1911–1916 or 1936–1939. This implies that connected groups of migrants did not simply locate in low crime cities. The first two pieces of evidence support our key identifying assumption.

To provide even stronger support, we show that our results are robust to selection on both observed and unobserved variables. Our results are robust to a battery of additional controls: contemporaneous economic and demographic factors, the number and concentration of white migrants and immigrants, and characteristics of counties from which migrants came. Our results also are robust to controlling for the share of migrants in each destination that moved there because of their birth town migration network. This variable, which we estimate using a structural model of location decisions, controls for a range of unobserved characteristics of migrants. Finally, we develop a more general test of selection on unobserved variables. The main threat to identification is that connected groups of migrants moved to cities with low crime rates, and unobserved determinants of crime persisted over time. In the presence of this unobserved selection, controlling for the 1960–1969 crime rate would eliminate the relationship between crime and social connectedness from 1970 to 2009. In contrast, if our empirical strategy is valid,

then controlling for the 1960–1969 crime rate would partly attenuate the estimated effect of social connectedness, and this attenuation would diminish over time; this is exactly what we find, which rules out the main threat to identification. All this evidence supports our empirical strategy.

Several additional results clarify the mechanisms through which social connectedness reduces crime. Social connectedness reduces crimes that are more and less likely to have witnesses, which suggests that an increased probability of detection is not the only operative mechanism. The effect of social connectedness on crime is not driven by effects on employment, education, homeownership, the prevalence of single parents, or crack cocaine use (which emerged in the mid-1980s). Other mechanisms, such as effects on norms or noncognitive skills, likely matter. We see the largest reductions in murders of adolescents and young adults, committed by acquaintances or strangers, during gang, drug, and other felonious activity. Furthermore, the effect of social connectedness on crime is persistent: even in the 2000s, when many of the original Southern migrants were no longer alive, crime rates were lower in cities with higher social connectedness. Natural explanations for this persistence include changing norms or skills, which are passed down across generations, and path dependence in crime (Nagin and Paternoster 1991).

We use variation in social connectedness that has the unusual and attractive property of being initially established decades before we measure outcomes as the result of a known process—birth town migration networks. This facilitates our primary contribution, which is providing new, more credible evidence on the effect of social connectedness on crime. We also contribute to the literature in economics studying how social capital and trust relate to various outcomes, including growth and development (Knack and Keefer 1997; Miguel, Gertler, and Levine 2005), government efficiency and public good provision (Alesina, Baqir, and Easterly 1997; La Porta et al. 1997), financial development (Guiso, Sapienza, and Zingales 2004), microfinance (Karlan 2005, 2007; Cassar, Crowley, and Wydick 2007), and intergenerational mobility (Chetty et al. 2014).[3]

More broadly, there is enormous interest in the causes and consequences of criminal activity and incarceration in U.S. cities, especially for African Americans (Evans, Garthwaite, and Moore 2016; Freeman 1999; Neal and Rick 2014), and this paper demonstrates the importance of social connectedness in reducing crime. We also add to the literature on the consequences of the Great Migration for migrants and cities, which has not considered the effects of social connectedness. This paper draws on Chapter 2 of my dissertation, which examines the role of birth town migration networks in more detail.

Notes

1. Recent studies on the effect of entry labor market conditions on workers include Altonji, Kahn, and Speer (2016); Kahn (2010); Hershbein (2012); Oreopoulos, von Wachter, and Heisz (2012).
2. This complements research on the effects of social networks on labor market outcomes (e.g., Bayer, Ross and Topa 2008; Beaman 2012; Hellerstein, McInerney, and Neumark 2011; Ioannides and Loury 2004; Munshi 2003; Topa 2001). These papers do not focus on the formation of social networks, which in some cases, like Munshi (2003), arise from location decisions.
3. Social connectedness is a broader concept than social capital, trust, or collective efficacy. For example, social connectedness might reduce crime by increasing the probability that criminals are identified, and this behavior typically is not included in definitions of social capital, trust, or collective efficacy. At the same time, our measure might capture social capital that was transported from the South. Definitions of social capital vary, but Portes (1998) argues that a consensus definition is "the ability of actors to secure benefits by virtue of membership in social networks or other social structures" (p. 6). Bowles and Gintis (2002), Fukuyama (1995), and Putnam (2000) emphasize the role of trust and reciprocity in their definition of social capital. Karlan (2007) makes distinction between social capital and social connections similar to ours.

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