RESEARCH AND TEACHING STATEMENT

David L. Clingingsmith

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RESEARCH

I am an applied microeconomist and economic historian. My research has primarily been in social economics and the economics of innovation. I trained in development economics and labor economics as well as economic history, and my empirical practice continues to be grounded in those fields.

My research in social economics has focused on the topics of religious identity, language, and social status. To study the relationship between Muslim religious identity and social tolerance I conducted a large-scale field experiment in Pakistan in which some participants were randomly allocated a visa to undertake the Hajj pilgrimage to Mecca. I found that the Hajj strengthened religious identity, but contrary to the concerns of many observers, it also increased the tolerance of pilgrims both toward other Muslims and toward non-Muslims as well.

I have done two historical studies on the economics of language. Using a panel of Indian census data I compiled and a shift-share instrument, I showed how the expansion of modern industry spurred the growth of bilingualism in India. In a second study using data on a large number of the world's languages, I investigated whether there is a minimum size at which languages remain viable and what the implications of that threshold is for language extinction.

I have investigated mechanisms through which social status affects economic behavior. These studies use controlled laboratory experiments to overcome empirical challenges difficult to handle otherwise. I have investigated conspicuous consumption as a means of signaling social status to others and whether high status enables individuals to earn economic rents in a competitive setting via the Matthew effect.

My work in innovation has two components. The first uses a field experiment to investigate factors that make the pitches entrepreneurs make to investors successful. I train entrepreneurs in best-practice pitch skills and measure the short and long run effects. Recently I have begun a historical project that seeks to measure the effects of automation technologies (transfer machines, numerical control, and robots) on employment in the United States since 1950.

My research is unified by an emphasis on credible causal evidence and an attention to social context. I strive to use experimental methods when feasible. When other approaches are necessary, I strive to be clear about how they may fail to reach the standard of a randomized experiment (Angrist and Pischke 2010). I believe that the correct interpretation of a data analysis often hinges on a detailed understanding of decision contexts, institutions, policy environments, and the socio-historical setting, and endeavor to identify those factors among these that matter in a given study.

1. Social Economics

Religious identity

A robust finding from both social psychology and economics is that individuals tend to favor members of their own group and to be hostile to outsiders (Akerlof and Kranton 2000, 2010; Chen and Li 2009; Goette, Huffman, and Meier 2006; Tajfel 1982; Tajfel and Turner 1986). Because it is very difficult to experimentally manipulate the intensity with which a person identifies with a real social group, such as a religion or nationality, much of this work has relied on artificial identities constructed by experimenters.

In *Estimating the Impact of the Hajj: Religion and Tolerance in Islam's Global Gathering*, my coauthors and I report on a unique experimental study in which we overcame this challenge. We varied the intensity of religious identification of Pakistani Muslims by randomly allocating the right to undertake the Hajj pilgrimage to Mecca (Clingingsmith, Khwaja, and Kremer 2009.) This paper appeared in *The Quarterly Journal of Economics*. The Hajj is an annual pilgrimage that able-bodied Muslims have a religious duty to undertake during their lifetime. About 2.5 million people complete the Hajj every year to engage in six days of rituals that emphasize their unity before God. Many religions feature pilgrimages with similar characteristics.

The impact of the Hajj on the beliefs and behavior of pilgrims toward others has been a concern of many national governments going back to imperial Britain. Mecca is located in Saudi Arabia, the home of conservative Sunni schools of thought such as *Wahhabism*. Governments have worried that pilgrims will become radicalized by the pilgrimage. While pilgrim accounts stress that the Hajj leads to a feeling of unity with fellow Muslims (e.g. X 1965), concerns about hostility toward out-groups find support in social identity theory. The impact of the pilgrimage on the attitudes of the average pilgrim both toward other Muslims and toward non-Muslims is an important question.

Our study was designed to answer if undertaking the Hajj pilgrimage 1) increased the strength of religious identification, 2) increased in-group favoritism and out-group hostility, and 3) increased the social status of pilgrims on their return. The challenge in answering such questions is that participation in rituals such as the Hajj are matters of individual choice. The discretionary nature of religious activity makes it difficult to separate causal and confounding claims about the effects of religious activity on behavior.

We solved this difficulty using an institutional random assignment of the opportunity to participate in the Hajj for Pakistanis. We secured the list of applicants from the government and assured ourselves that the results were consistent with random assignment. Between six and eight months after the pilgrimage ended, we located and surveyed 1,605 applicants about a wide ranging set of topics relating to religious belief and practice, tolerance, politics, and social roles.

Our causal estimates show Hajj increased both the intensity of religious practice and adherence to orthodox religious principles. Belief in and practice of less orthodox religious rituals declined. As we expected, Hajj increased the intensity of the pilgrims' religious identity.

While Western observers often connect Muslim religious orthodoxy with intolerance, we found that to the contrary participation in the Hajj increases belief in equality and harmony among ethnic groups and Islamic sects. This is consistent with the nature of the Hajj rituals, which promote the mixing of pilgrims across national and sectarian lines. Moreover, Hajj did not increase the antipathy of pilgrims toward non-Muslims or increase belief in radical political ideas. Instead, Hajjis show increased belief in peace, and in equality and harmony among adherents of different religions. The positive effects on attitudes to out-groups extend to non-Muslims.

Our exploration of the mechanisms through which Hajj had its effect suggests that these changes are likely due to exposure to and interaction with Hajjis from around the world: Pilgrims with greater contact with non-Pakistanis experienced larger effects, while the Hajj did not affect formal religious knowledge or change the social role of pilgrims on their return.

This project has been recognized as methodologically innovative and was featured as a running example in a recent textbook on field experiments (Gerber and Green 2012). The paper was also part of the evidentiary base used to garner support for a U.S. Army program called *Voices of Moderate Islam*.[1] The program sponsored a study trip for 33 Afghan leaders to Jordan and Mecca in 2010 and was made a counterinsurgency program-of-record for the Afghan theater by Gen. David Petraeus.

Language

I have tackled three sets of empirical questions in my research on the economics of language. The first set has to do with how many languages we should expect there to be in the long run. A language can be thought of as a network that enables low-cost communication among members (e.g. Church and King 1993; Grin 1992; Lazear 1999). The theory of networks suggests that the value of joining a network increases with the number of members. In equilibrium, many networked technologies, such as operating systems and social networks, feature a small number of networks, each of which is large in size. Yet there are more than 6,500 spoken languages in the world today, and the median language has just 10,000 speakers. How can we reconcile these facts? In light of globalization and what we know about networks, should we expect mass extinction of small languages?

In *Are the World's Languages Consolidating? On the Dynamics and Distribution of Language Populations*, I investigate the stability of the size distribution of languages and evaluate whether we are heading for a mass extinction of small languages and the cultures they carry (Clingingsmith 2015a). This paper appeared in *The Economic Journal*.

[1] Maj. Matthew Yandura (Senior Military Analyst, USAIPO), email to David Clingingsmith, February 21, 2012.

The consensus in the existing literature, primarily to be found in political science and linguistics, is that this convergence is underway and that only a few dozen large languages plus those smaller ones with state protection—perhaps 5% of the total—will survive (Crystal 2000; Laitin 1993; van Parijs 2000; De Swaan 1993; Wickstrom 2005). This view is strongly informed by the notion that network externalities confer important advantages to large languages. It also has a simple, testable implication that I am the first to investigate systematically: small languages ought to experience slower population growth than large languages.

To test whether small languages are in fact growing more slowly than large ones, I first produced population level and growth estimates for 344 languages using population censuses from 14 countries. This data shows no correlation between growth rate and population size for languages with more than 35,000 speakers. This implies that the size distribution of languages larger than 35,000, which spans more than four orders of magnitude, is in equilibrium. Only languages below the threshold are systematically shrinking relative to the rest. This result is robust to a set of economic and demographic controls.

How can network externalities be consistent with such a broad size distribution of languages? I address this question by developing the theoretical model of language population dynamics that treats language choice as a local coordination problem among networked individuals. The model draws on evolutionary game theory and the theory of networks (Jackson 2008; Newman et al 2001; Young 1998). The model predicts that individuals will indeed converge to a common language, but only within connected components of a network.

To see the intuition behind the model, imagine a population occupying two valleys separated by a mountain range as shown in Figure 1. Suppose that people are connected to neighbors who are nearby as long as mountains do not intervene. The model predicts that each valley will coordinate on a common language, but that this language need not be the same across valleys. In Figure 2, A and B will share the same language even though they are far apart because of their intermediate connections. They are part of the same connected component, and that component will converge on a common language. However, while B and C are close, due to the mountains they are part of

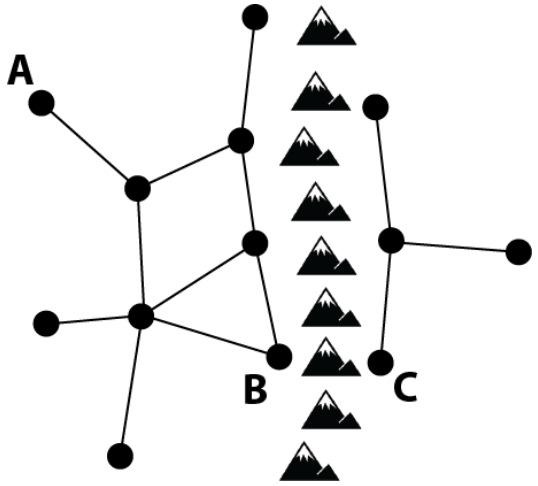

Figure 1: Illustration of Connected Components of Language Network

different connected components and may speak different languages. If the connected components of a language network vary widely in size, so will the populations speaking any given language. Simulations in which the network of speakers is based on spatial proximity suggests that languages population will follow a double Pareto distribution above a threshold level. This power-law distribution is consistent with most languages being quite small but a small number being very large.

I test if language size indeed follows this distribution using cross-sectional population estimates for 6,210 languages from the World Language Mapping System (WLMS) database. I estimate the parameters of the double Pareto distribution from the WLMS data using maximum likelihood. I find that the best fit is achieved with a size threshold between 17,900 and 36,000.

This second finding provides convergent evidence that language populations are in equilibrium above a size of about 35,000 speakers. While the evidence on language growth rates covered on 344 languages, this second piece of evidence encompasses all known languages. Languages above 35,000 speakers account for 29% of extant languages. My evidence suggests that the minimum viable size of a language is much smaller than the literature mentioned above has suggested. Rather than 5% of extant languages being safe, it suggests 29% are. Speakers of languages larger than 35,000 account for 99.5% of the human population.

Language has an economic function in facilitating economic exchanges such as the trading of goods and services or the organization of complex production processes. In *Industrialization and Bilingualism in India,* I show that growth of the industrial sector in India lead to an increase in bilingualism, particularly among people whose mother tongue was in the minority where they lived (Clingingsmith 2014). This paper appeared in the *Journal of Human Resources.* Bilingualism is more sensitive to changes in the incentives to learn languages than mother tongue because

additional languages can be learned at any point in the life cycle, though the cost of doing so rises with age. Prior work on bilingualism has mostly focused on migrants in the West[2], so this is the first evidence I am aware of showing that economic development leads to linguistic change.

This finding is significant in two primary ways. First, hundreds of millions of citizens in developing countries are linguistic minorities, and my research suggests that bilingualism can be a significant factor in accessing jobs in the industrial sector. This has hitherto been under-appreciated by economists and policymakers. Second, bilingualism not only facilitates communication in the workplace but also in social life more generally and also provides access to information and culture in the new language. We can therefore see the results as showing a way in which economic development induces cultural change.

India is one of the world's most linguistically diverse countries, with hundreds of languages in common use. It is an ideal setting to study language change because it is one of the few countries to have collected data on language over the long run. The paper looks at the growth in bilingualism between 1931 and 1961, when industrial employment in India roughly doubled. Historical evidence suggests that communication was important in industrial workplaces of the era and that workers who spoke a minority mother tongue were often bilingual. I created a panel dataset from published volumes of the census of India to conduct the analysis, and identify the effect of industrial employment growth using a Bartik-style instrumental variable (Bartik 1991).

We learn that industrial expansion did in fact lead to strong growth in bilingualism. While bilingualism increased for speakers of the local majority language, the growth was particularly strong for those whose mother tongues were locally in the minority. Mother-tongue speakers of the local majority language were pushed strongly toward learning Hindi and English, while mother- tongue speakers of minority languages were pushed most strongly toward English and the majority language. Literacy is also a skill in demand in the industrial sector and, for some, bilingualism is a step to becoming literate. I present evidence that even accounting for this indirect motivation to become bilingual, industrial expansion still produces strong additional growth in bilingualism. While I find no evidence that industrial expansion produced assimilation of minority language lineages to the majority language, even a 30-year panel may be too short a time-span to see effects given the generational time scale of mother-tongue change.

Social status

Social status refers to an individual's position within a socially recognized hierarchy (Heffetz and Frank 2011). In a meritocratic society, social status is closely connected to economically valued characteristics, such as intelligence and charisma. Having high status is desirable in part because it signals the presence of such characteristics to others.

[2] See for example Berman, Lang, and Siniver, 2003; Bleakley and Chin, 2004; and Lang and Siniver, 2009.

In *Status and the Demand for Visible Goods: Experimental Evidence on Conspicuous Consumption*, coauthor Roman Sheremeta and I conduct an experiment that provides convincing evidence that the desire to signal social status drives demand for goods that are visible to others (Clingingsmith and Sheremata, Forthcoming).

Veblen (2009 [1899]) argued that part of the motivation for wearing a fine suit or driving an expensive car is to signal high status to others. He coined the term "conspicuous consumption" to refer to status signaling via the purchase of publicly visible goods. However, there are other reasons to buy nice clothes and cars, and free and direct ways to communicate status to others. It is not obvious that luxury consumption is necessarily conspicuous consumption.

While simply understanding the social function played by consumption is important, there are also policy considerations. Signaling a rank using consumption can generate an arms race and inefficiently high spending on visible goods (e.g. Frank 2008).

Most existing evidence about conspicuous consumption uses observational data.[3] Both observational and field experiment studies face two challenges in identifying conspicuous consumption as a motivation that we overcome in our laboratory experiment. The first challenge is that status and income are correlated in naturally-occurring settings in a way that is difficult to disentangle. When a high-income person chooses a Mercedes over a Toyota, to what extent is that due to a pure income effect and to what extent to status signaling? It is difficult to argue that a random shock to income (e.g. winning a lottery) that is sufficiently large to induce changes in consumption behavior in the field would itself confer no change in social status. This is an issue not faced in the lab.

The second challenge is that consumer goods bundle many characteristics. The visibility to others that makes a good suitable for conspicuous consumption does not naturally vary independently of these other characteristics. Automobiles and outerwear are always publicly visible, while mattresses and underwear are not. However, we can induce variation in visibility alone in the highly controlled setting of the laboratory.

In our experiment, individuals are gathered in groups and accrue income. At the beginning of the experiment, participants take a math quiz and are ranked by their score. This rank is an indicator of social status. Each person then has an opportunity to purchase a desirable consumer good—chocolate truffles—using their income.

We vary the conditions under which participants make their purchase. First, income is either assigned at *random* or based on *rank*. Second, chocolate purchases are either *private* or *public* to the entire group. The *rank-public* treatment features the two conditions Veblen's theory suggest leads to conspicuous consumption: 1) income is correlated with status and 2) consumption choices are visible to all, so they can serve as a signal. The *random-public* and *rank-public* treatments each remove one of these conditions, while *rank-private* removes both.

[3] See Charles et al 2009; Grinblatt et al 2008; Heffetz, 2011; and Kuhn et al 2011.

Figure 2 shows that the experiment supports Veblen's theory of conspicuous consumption. Demand for chocolate in the *rank-public* treatment is much greater than in the other treatments. By comparing the demand curves in rank-private and rank-public, we estimate that the average welfare change from making consumption public is as large as the consumer surplus when the price of chocolate is approximately the retail price. The change in welfare comes primarily from men, who also account for most of the conspicuous consumption.

An active literature explores whether the high compensation of U.S. corporate executives can be explained by extraction of economic rents rather than productivity (e.g. Bivens and Mischel 2013; Piketty 2014). This literature typically locates the source of economic rent in market power, such as the well-documented influence executives have over the setting of their own wages (Bebchuck and Fried 2004).

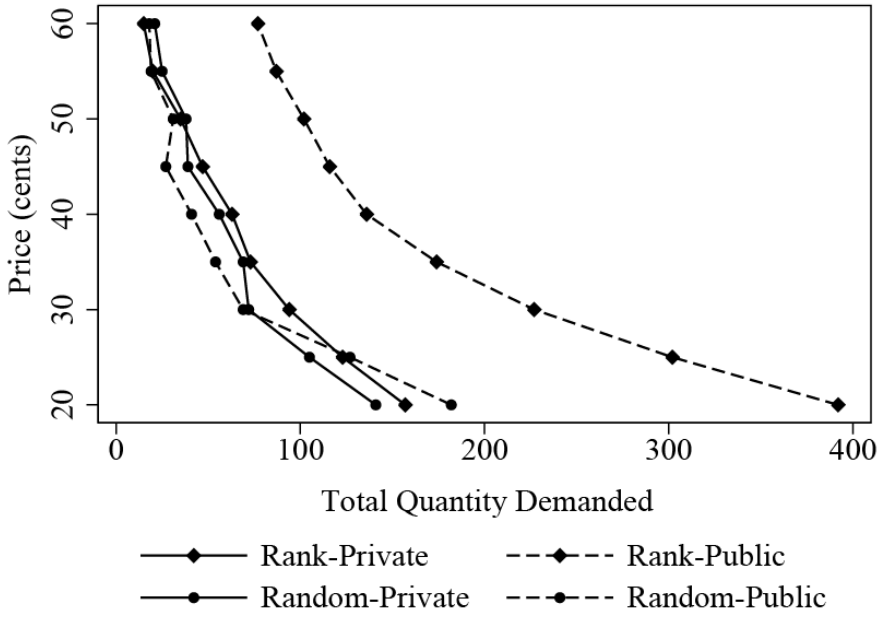

Figure 2: Aggregate Chocolate Demand by Treatment (Clingingsmith and Sheremata 2015)

In *Status and Economic Rent: Experimental Evidence on the Matthew Effect*, my coauthor Roman Sheremeta and I propose and experimentally test an alternative explanation for the emergence of economic rents based on a cognitive bias that does not require market power (Clingingsmith and Sheremata 2018). If rents can emerge in a competitive market, as we show, they may be more prevalent than previously imagined.

Long-term principal-agent relationships in both the labor and financial markets are often initiated based on past achievements of the agent, such as a resume sent to a prospective employer or a history of past returns advertised to investors. The principal forms beliefs about the expected

performance of the agent based on these past achievements. However, as the agent works, the principal gets additional information on their productivity at the task for which they have been hired. The Matthew Effect[4] describes the possibility that the principal will insufficiently adjust their beliefs about the expected performance of the agent based on the new information, which can lead high-status agents to earn economic rent above marginal productivity.

Designing a test for the Matthew Effect in a field experiment or using observational data is difficult because doing so requires that productivity be observable and that productivity be the only motivation of principals in contracting with agents. We therefore create a laboratory experiment to test the theory. Principals observe the pre-assigned status—*Gold, Silver,* or *Bronze*—of a group of three agents. The agents then perform the simple task of adding sets of numbers over nine periods. In each period, the principals allocate $6 among the three agents and are paid the number of sets added by the agent multiplied by their investment. When a period is complete, the principal learns the actual performance of all three agents.

The logic of the design is as follows. In the first period of investment, the principals only know the status of each agent. To the extent that the principal believes status is an indicator of performance in adding up sets of numbers, they should invest more in agents with high status. As time goes on, the principals have an increasing amount of information about the performance of each agent at adding numbers. To the extent this information is more predictive of subsequent performance than status, the principals should weight it more heavily in their decisions.

We find that when the status categories *Gold, Silver,* or *Bronze* are assigned *randomly*, the principals do not take them into account. Instead, allocations to each agent are strongly influenced by the average performance of the agent in past periods. The status categories are also not predictive of the performance of the agents. However, when the status categories are assigned based on a short cognitive test of *math ability*, principals disproportionately allocate funds to high-status agents. Even though conditional on past performance, status is uncorrelated with future performance, status is still a significant determinant of investment. In other words, they exhibit the Matthew Effect.

We learn from this experiment that when principals are exposed to a status signal before making their investment in an agent, they tend to exhibit the Matthew Effect and insufficiently adjust their subsequent investment to reflect actual performance. Our results suggest that rents can emerge even in full-information competitive settings because people tend to insufficiently adjust their opinion of high-status individuals to recent performance. This suggests that rents may be more common in the labor market than previously thought.

[4] The term “Matthew Effect” was coined by sociologist Robert Merton in his study of Nobel laureates (1968). Merton’s interviews lead him to believe that contributions by high-status scientists were disproportionately favored for citation relative to their objective merit. In other words, the scientific community places too much weight on the past achievements—i.e. social status—of the contributors and insufficient weight on the merits of the work itself.

2. Innovation

Investment by venture capitalists and angel investors support many high-potential startup companies in the United States. [5] Investors and entrepreneurs are matched through a staged process in which the investor learns progressively more about the venture and then decides to learn more or discontinue investigation. The first stage often involves the entrepreneur giving a short verbal pitch of the venture idea, called an elevator pitch. The goal of the elevator pitch is to arouse sufficient interest in the investor that the investor will follow up at greater length.

Not surprisingly, a robust market has emerged for practitioner guides that offer instruction to would-be entrepreneurs on how to pitch investors; a related management literature exists as well.[6] These works identify types of information that entrepreneurs ought to include in their pitches in order to improve their success with investors. However, little credible evidence exists on the determinants of pitch success, or on the effectiveness of pitch training. It is an open question as to whether following "expert pitch advice" affects pitch success, and, if so, how.

My collaborator Scott Shane and I undertook a large field experiment to begin to address the dearth of knowledge about pitch performance and success. The experiment brought 273 student entrepreneurs and 50 investors[7] together in a series of pitch competitions in which the entrepreneurs competed for monetary prizes.[8] The entrepreneurs were randomly assigned to a panel of investors to pitch their idea for a new venture. The investors served as judges, and scored the pitches based on how likely they would be to further investigate each opportunity in their investment practice. Before they pitched, entrepreneurs were randomly assigned to receive either pitch training or a null training. We collected written drafts of the entrepreneurs' pitches both before and after training, recorded their pitches on video, and collected the judges' evaluations of their pitches.

The first paper resulting from the experiment is *Training Entrepreneurs to Pitch Experienced Investors* (Clingingsmith and Shane, Forthcoming). We find that, contrary to the claims of the literature, pitch training does not increase an entrepreneur's evaluation of the average venture. Instead, we find that training tends to increase the variance of scores while having no effect on the mean. This result can be understood through a simple real options framework in which pitch training increases the precision of the signal delivered by a pitch by including more information. Having more information better enables judges to distinguish good from bad pitches and to rely

[5] More than 75,000 U.S. entrepreneurs are funded to the tune of $50 billion annually by venture capitalists and business angels. The elevator pitch is an element of most of these efforts (Clark, 2008).
[6] Examples of the practitioner literature include Coughter 2012; Getty 2014; Klaff 2011; McGowen 2015; and Soorjoo 2012. Academic papers include Chen et al 2009; Mason and Harrison 2003; Maxwell and Levesque 2011; Nagy et al 2012; and Parhankangas and Ehrlich, 2014.
[7] The investors included venture capitalists, business angels, mentors at accelerators, and mentors at campus-based entrepreneurship programs.
[8] There were four competitions. The prizes were $2,500 for first place, $1,000 for second place, and $500 for third place.

more on the pitch and less on their prior beliefs about new venture quality. We find that pitch training does increase the information included in pitches.

An additional prediction of our framework is that judges who have different prior beliefs will be affected differently by the increase in information induced by pitch training. We show that experienced judges[9] tend to have lower priors than inexperienced judges. The model suggests this will mean training will have a more positive effect on the scores of experienced judges than inexperienced judges, which is the pattern we see in the experiment.

Pitch training only improves the scores of entrepreneurs with good ideas who are pitching to experienced investors. Scores in all other cases fall. While the recommendations of the practitioner literature do not deliver better immediate outcomes for all entrepreneurs, they do seem to improve the efficiency of the evaluation of new venture ideas. In the long run, this is probably better for all entrepreneurs since the intense scrutiny in stages of the financing processes will likely reveal any flaws not revealed at the pitching stage.

Several other papers analyzing the pitch experiment are in progress. In *Pros and Cons of Pitch Training* (Clingingsmith and Shane 2018), we conduct a follow-up survey of participants in the pitch competition 30 months after it concluded to measure the long-run outcomes of training. We also coded participant behavior during their pitches. We found evidence that training disrupted their performance in the short run. In the long run, training increased effort made by participants to improve pitches and knowledge about entrepreneurship

In a recent set of papers, Acemoglu and Restrepo (2017, 2018a,b) have studied the impact of automation technologies on labor markets. Their empirical work has focused primarily on the impact of robots since 1993. Robots the most recent of three major automation technologies that transformed American manufacturing processes after World War Two. The two others were transfer machines and numerically controlled machine tools. Leah Boustan and I are working on a project to measure the diffusion of these three technologies across manufacturing industries from the early 1960s to the present and the effects this diffusion had on the employment of different skill and occupation groups in manufacturing. We use the network of patents that cite the foundational innovations in transfer machines, numerical control, and robots to measure diffusion. The patents are linked to industrial sector by exploiting classifications of patents by industry of use done by the Canadian patent office between 1978 and 1993. We use machine learning techniques to build a classifier for US patents using the Canadian patent texts and classifications.

2. Other Work

Deindustrialization in India

My interest in the social context of economic behavior stems in part from my training as an economic historian. Beyond my work on language, an additional paper in economic history

[9] Defined as those who are above the median volume of early-stage deals of judges in the experiment.

analyzes the causes of deindustrialization in early modern India. In *Deindustrialization in 18th and 19th Century India: Mughal Decline, Climate Shocks and British Industrial Ascent*, coauthor Jeffery Williamson and I attempt to resolve one of the largest questions in Indian historiography: Did British colonialism cause the deindustrialization of India in the 18th and 19th centuries (Clingingsmith and Williamson 2008)? Perhaps because anti-colonial leaders frequently cited deindustrialization as one of the evils of British colonialism, quantitative assessments of the timing of deindustrialization and the impact of British policy have been lacking.

We compile a variety of evidence, including price series that we interpret through a three-sector neo-Ricardian model, historical accounts of the Indian economy, and information on climate shocks. We find convergent evidence that Indian industry was already seriously impaired by climate shocks and political disintegration before Britain's colonial expansion was fully underway. Moreover, the main impact was through the cost-competitiveness of mechanized British industry, rather than more direct forms of colonial exploitation. This paper appeared in *Explorations in Economic History* and won the Explorations Prize for best article in that journal in 2008.

Mental accounts

We are all approached on occasion by others asking for money. In *Mental Accounts and the Mutability of Altruism*, I investigate how the sources of the funds at our disposal act to frame our decision and influence how altruistic we are in response (Clingingsmith 2015b). This paper has a revise and resubmit from the *Journal of Economic Psychology.* These funds come to us from a wide variety of sources, such as wages, bonuses, tax refunds, and so forth. The theory of mental accounting holds that individuals view some kinds of spending as more appropriate for some categories of income than others (Thaler 1999). Earned income and windfall income are thought to be particularly different in their appropriate uses.

I recruited 1,022 participants from an online labor market to play a dictator game.[10] Dictators could earn income via a real-effort task and/or were given windfalls. On beginning the experiment, dictators were randomly assigned to one of 25 cells that varied in the expected earnings and expected windfall. Some cells featured a single income source, either windfall only (WO) or earnings only (EO), while the remaining featured both sources (EW). In all cases, dictators made their choices with respect to their total income. The design ensures marginal changes in either income source are not correlated with changes in total income. Once earned and windfall income were realized, they then made a decision about how much of their income to share with receivers. In analyzing the experiment, expected earnings and windfall serve as instrumental variables for actual earnings and windfall.

[10] The dictator game is a simple decision about sharing resources between two people, labelled the dictator and the receiver. The dictator is provided a sum of money to divide with the receiver. They may choose any amount between nothing and the full sum to give to the receiver.

Pooling all treatments, I found that dictators shared about 8.5% of marginal windfall income and 1.9% of a marginal earned income on average. This is inconsistent with fungibility of income across sources and supports the mental accounting view of income.

Focusing on the single-source treatments (EO/WO), I found that especially large differences in the dictators' willingness to share. The marginal willingness to share additional windfall income (the WO treatment) was 13%, while the marginal willingness to share additional earned income (the EO treatment) was 6%. This is consistent with existing evidence on income sources and sharing.[11] More strikingly, I found that marginal sharing from either source falls dramatically when both sources of income are present. With both sources present, the marginal willingness to share was 2% for windfall income and 0% for earned income.

We learn two primary lessons from this experiment. First, a sharing choice whose framing contains both earned and windfall income will lead to lower generosity than one that contains a single source. This means that those soliciting funds ought to be careful in their framing of charitable asks about the ways in which income might be referred to. The paper thus contributes to a growing literature about framing effects on altruistic behavior.[12] For example, one may want to avoid highlighting an individual's good fortune and hard work in the same ask. Second, we learn that individuals do not treat different income sources as fungible at the margin. While the theory of mental accounts is popular, field studies that attempt to measure its primary theoretical prediction, infungibility, have been scarce (Milkman and Beshears 2009; Hastings and Shapiro 2012).

Negative emotions, well-being and income

In *Negative Emotions, Income, and Welfare*, I consider what the empirical relationship between emotional experience and income can tell us about the welfare effects of marginal changes to the income distribution (Clingingsmith 2016). This paper is forthcoming in the *Journal of Economic Behavior and Organization.*

Revealed preference is a core axiom of modern microeconomic theory. It says that when an individual can afford both items A and B and chooses item A, he or she prefers A to B. Its chief advantage is in allowing us to make statements about the welfare effects of policy changes without having to measure their impact on the psychological states of individuals. Using the Pareto criterion, we can say a policy change is welfare improving as long as it raises the income of someone without reducing the income of anyone. A weakness of revealed preference is that it does not help us to evaluate the welfare effects of policy changes, such as redistributive taxation, that leave some people better off and others worse off. Are those who are hurt harmed more than those who benefit? Revealed preference does not provide us with the tools to answer this question.

[11] See Cherry 2001; Cherry et al 2002; Cherry and Shogren 2008; Hoffman et al 1994; and Oxoby and Spraggon 2008.
[12] See Dana et al 2007; Lazear, et al 2012; Hamman et al 2010; and Haisley and Weber 2010.

A solution to this problem is to try to measure the effects of income on psychological states directly. By psychological states I mean the flow of affective experience—the feelings of pleasure and pain—that Bentham and Mill referred to as "utility" and that are central to what most of us mean when we think of well-being or welfare (see Kahneman et al 1997). This is a difficult task, and indeed the perception that it could not be done is the reason why revealed preference was embraced in the first place (Fisher 1892; Pareto 1906; Samuelson 1938). However, there has been substantial progress in our ability to measure psychological states since Samuelson's time, and indeed doing so is the only way to make progress in answering empirical questions about the welfare effects of redistribution.

I measure affective experience using a psychological instrument, the K6 index, that asks about the frequency with which six different negative emotions have been experienced in the past month (Kessler et al 2003). My main source of data is the Panel Study of Income Dynamics (PSID), which has included the K6 index in its seven most recent waves. In interpreting the results, one should keep in mind that negative and positive affect may not be perfectly correlated, and that other psychological states besides affect may be components of welfare.

I identify the effect of family income on negative emotional experience using an instrumental variables approach. I link the industry of employment and region each individual reported in the first year in which the K6 was collected for them to average weekly income measures from the Current Population Survey for each year in which the individual appears in the data. These average income measures serve as the instrument conditional on initial industry by region effects.

I find that the marginal effect of income on the experience of negative emotion falls strongly in the level of income. Concretely, the benefit in terms of less negative emotion of an additional dollar of income to someone with $16,000 in income is about 6.7 *times* larger than the benefit of that same dollar to someone with income of $160,000. The marginal effect of income on negative emotion approaches zero at incomes of around $200,000.[13] In order to fully answer the question of whether redistribution from the 90th to the 10th percentile of income enhances aggregate welfare, we would also need to know about the effects of taxation on income. However, the near order of magnitude difference in marginal effects suggests that those effects would have to be very large to be fully offsetting.

[13] Income adjusted to 2013 dollars using the chained CPI.

TEACHING

1. Undergraduate

During the 11 years I have taught as Case Western Reserve University, I have primarily taught advanced level undergraduate classes in economics, such as The Economy in the American Century (ECON 395), Advanced Topics in Economics (ECON 391), and Designing Experiments (ECON 328). I have recently decided to add Principles of Microeconomics (ECON 102) to my complement of courses.

The primary goal of all principles of microeconomics classes is to get students to internalize a small set of core concepts, such as opportunity cost, optimization, competition, market power, and price and income effects, so that they retain them and can apply them correctly and with confidence. When I developed my principles class, I also believed it was important to add three additional goals. I want students to be introduced to concepts common to the way contemporary applied microeconomists think but that are not often emphasized in principles, such as game theory, bargaining, and contract theory. I also want students to be familiar with the breadth of social phenomena to which economists have contributed to understanding. Lastly, I want students to understand that economists demand scientific rigor in their work. I believe that these additional goals are critical to conveying the intellectual rewards of studying economics to undergraduates who are deciding whether more economics ought to be in their future.

In my advanced classes, the overarching goal of my course design and teaching activities was to develop my students' skill at making and evaluating arguments that connect economic reasoning with empirical data. I believe this skill has both practical value for students in their future professional lives and in further study, whether in economics or elsewhere, and a social value by making students better analysts of the issues of the day. As the volume and variety of digital data being generated about human activities continues to increase, the skill of careful evaluation of arguments made using such data will only grow. Empirical economics is uniquely well suited to provide this education because, among the statistics-using disciplines, it places the greatest emphasis on distinguishing causal claims from mere correlations.

The well-received class I developed about designing experiments (ECON 328) illustrates this approach (Course rating: 4.26, Instructor rating: 4.65). After learning some basic concepts in casual inference and experimental design, we spend most of our time reading and discussing significant experiments in the literature. By exposure to examples and engaging in critical discussions of them, students learn how to formulate causal hypotheses and develop experimental designs that address them. This practice enables students to ask the extremely powerful heuristic question "what experiment would we have to run to test this hypothesis?" whenever they confront a claim that someone makes using data. The ability to ask and answer this question is extremely useful for identifying the flaws in statistical arguments.

As part of their work for the class, students develop two experimental designs to answer a social science question that is of interest to them and that stems either from their everyday life or from

the reading. Recent examples include grading bias among undergraduate TAs and the effects of incentives within fraternities for required service work. While the students don't implement their experiments, I do give them extensive "as if" feedback. This has proven to be one of the most rewarding aspects of my teaching year as the students are exceptionally creative and insightful in developing their designs.

I also teach our department's SAGES departmental seminar and capstone offerings. In the seminar (ECON 391), we explore both active areas of economics research and current events from an economic perspective (Course rating: 4.44, Instructor rating: 4.66). The topics and our pace through them are chosen through regular consultation with the students. For the past two years we have spent the first half of the class reading Thomas Piketty's *Capital in the Twenty-First Century* and related writing on inequality. During the second half we study topics chosen by the students. This past year the topics were the economics of crime and criminal organizations and the growth and collapse of civilizations. Each session has a common reading and begins with a student presentation of a complementary reading. I provide students with discussion questions as starting points to get things going. The most important output of this class is the opportunity to engage in high-level discipline-based discussion.

The goal of the capstone class (ECON 395) is to provide a setting in which students can marshal the knowledge and skills they have gained as undergraduates, both in economics and in the other coursework they have taken, to complete a project in the area of economics that most interests them. To reach this goal, the class takes an era of U.S. economic history as the trunk of a tree off from which many different themes may be investigated. In recent years, the eras we have concerned ourselves with are the financial crisis of 2008 and ensuing Great Recession along with the 1920s and 1930s (Course rating: 4.02, Instructor rating: 4.39). The class begins with a set of comparative lectures by me that outline the main institutions, events, and processes involved in this most recent crisis and in the Great Depression of the 1930s. This builds a shared conceptual understanding for students to draw on as they go on to develop class-long group presentations and individual papers. Students have chosen topics such as the development of derivatives markets, the Greek Debt crisis, the evolution of consumer credit, the Euro crisis, and the Panic of 1792.

Individual mentorship is an important part of my teaching role. I have supervised ten undergraduate students in our department's two-semester honors thesis project as well as three independent studies. More informally, I always make myself available to my advisees and students in my classes who want to talk about economics, academics more generally, or career concerns. I keep up with many students long after they have graduated from Case.

2. Graduate

My graduate teaching experience has been more limited. I developed and taught an Executive MBA elective in Behavioral Economics (EMBA 477) in the spring of 2010 (Course rating: 4.20, Instructor rating: 4.10). The course was centered on learning about common biases of judgment and decision making and applying that knowledge to business situations.

SERVICE

1. Profession

I have served as a referee for twenty-one professional journals, including the Review of Economic Studies, Quarterly Journal of Economics, and the Journal of Political Economy. I have also reviewed a book manuscript for Oxford University Press and a grant application for the Israel Science Foundation.

2. Department

At CWRU I was a member of our department's recruiting committee from 2009-2012 and again in 2017-18. During my stints on the committee, we met with many dozens of talented junior scholars and added five to our department. I also served on the department's seminar committee, which invites 8-10 economists each year to present their work and interact with our faculty and students, from 2009-2014. Since 2014, I have coordinated the brownbag seminars within the department and served on the undergraduate program assessment committee and the McMyler lecture committee. From time to time I have also been involved in projects to analyze and better understand our pattern of undergraduate course and major enrollments and to advocate for the department with senior leadership at the school and university levels.

3. School

I have served on the Weatherhead School of Management’s Faculty Council between 2013 and 2016 as both a member and secretary. Council meets monthly during the semester to consider matters of school policy. As secretary of council I am also secretary of the faculty. In these roles I am responsible for the minutes of both the council and faculty assembly.

During the 2015-16 academic year, I served on the Weatherhead School of Management’s Committee on Appointments (i.e. promotion and tenure) as an untenured observer. After I was tenured, my department elected me to be our representative to the committee and I have served since the beginning of the 2017-18 academic year.

4. University

I have been the Weatherhead School of Management's member on CWRU's honorary degree committee since 2010. This committee reviews between 5 and 10 nominations for honorary doctoral degrees each year and makes recommendations of worthy candidates to the board of trustees. During this time, we have forwarded twenty-four recommendations to the Board of Trustees. All have been approved, and to date six have been conferred.

From 2015-18, I was a faculty representative on the Faculty Senate Committee on Undergraduate Education Academic Standing Subcommittee. Several times each semester, this committee gathers to review cases of students whose performance has placed their academic standing in jeopardy and to consider what action the university should take. We review several hundred cases per year.

Since 2010, I have regularly served as a faculty representative on the academic integrity boards at both the undergraduate and graduate levels. The board hears cases of students charged with violations of academic integrity, such as cheating and plagiarism. The board makes judgments of responsibility and imposes sanctions. I became a member after uncovering a case of plagiarism in one of my classes. I have participated in about 25 half-day hearings to date.

I have also served as a faculty marshal at convocations and commencements since 2010.

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