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Essays on firm location decisions, regional development and choices under risk

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ABSTRACT

My dissertation consists of four papers. The first two papers study regional economic development. In particular, they focus on broadband Internet and agglomeration economies in rural areas. One paper tests whether broadband improves rural economy and find positive broadband effects on new firm location choices. The other paper explores whether agglomeration economies operate even in rural areas and find that agglomeration economies are important for new firm location choices and commuting decisions. Those findings from the two papers have useful implications to regional economic development policies.

My two other papers study choices under risk. One paper focuses on risk elicitation methods with a multiple price list format, which is widely used in the literature. The paper compares subjects' choices between the elicitation method and one question selected from the method. The paper finds significant differences in the comparison and show that the differences occur due to reference-dependent preferences. Those results suggest that the elicitation method is not reliable because loss aversion influences elicited risk aversion. The other paper tests whether pre-play learning removes inconsistent preference rankings between choice and pricing for lotteries. Inconsistent preference rankings have been studied last four decades because standard economic theory cannot explain inconsistent preference rankings. Pre-play learning is simple ex-ante lottery learning, where subjects observe playing lotteries before they make decisions. The paper finds that pre-play learning removes inconsistent preference rankings, which suggests that pre-play learning makes preference rankings consistent between choice and pricing as predicted in standard economic theory. Those results from the two papers have meaningful implications to the literature.

CHAPTER 1. GENERAL INTRODUCTION

My dissertation consists of four papers, which are located in chapters 2-5. Chapters 2 and 3 study regional economic development. Chapter 2 examines whether broadband Internet improves rural economic growth. Broadband is believed to improve firm productivity by decreasing production costs and expanding the potential size of the firm's market. However, broadband could also harm the rural economy because broadband Internet may increase competition between urban and rural firms and because rural residents may buy more products online rather than buying at local retail stores. After controlling sample selection issues related to local broadband availability, we find that broadband improves the rural economy. Chapter 3 explores how agglomeration economies influence rural economies. The nature of agglomeration economies is not clear in rural areas. For example, if a certain size of agglomeration is required for agglomeration economies to operate, agglomeration economies may not exist in rural areas. We test this issue using rural-urban comparison for agglomeration economies. We find that agglomeration economies are important in rural areas, and that agglomeration factors have similar sizes of elasticities between urban and rural areas. That suggests that rural areas lacking sufficient agglomeration can only survive by accessing urban labor markets that offer agglomeration.

Chapters 4 and 5 study choices under risk. Chapter 4 investigates how elicitation procedures influence elicited risk attitudes. In particular, we test risk elicitation methods with a multiple price list format. We compare subjects' choices between the elicitation method and one question selected from the method. We find significant differences in the comparison. We test possible causes for the differences: the lack of incentive compatibility in a pay-one-randomly incentive mechanism, decoy effect, imperceptive preferences and reference-dependent preferences. We find that reference-dependent preferences are consistent with our data. Our results suggest that risk elicitation methods with a multiple price list format are not reliable because loss aversion influences elicited risk aversion. Chapter 5 explores whether lottery learning removes inconsistent preference rankings between choice and pricing for lotteries. In particular, we focus on pre-play learning where subjects observe playing lotteries before they make decisions. Inconsistent rankings between choice and pricing have drown researchers' attention because standard economic theory cannot explain the inconsistent rankings. However, we find that pre-play learning removes inconsistent rankings between choice and pricing. In other words, pre-play learning makes preference rankings consistent between choice and pricing as predicted in standard economic theory. Chapter 6 provides suggestions for future research.

CHAPTER 2. BROADBAND INTERNET AND NEW FIRM LOCATION DECISIONS IN RURAL AREAS

Younjun Kim and Peter F. Orazem

ABSTRACT

Improving rural broadband access has been touted as a rural development strategy, but there is limited evidence that broadband service affects rural economic growth. We measure the effect of broadband deployment on locations of new rural firms. Location-specific fixed effects are controlled by a counterfactual baseline that measures how local broadband service in the early 2000s affected local new firm entry in early 1990s before broadband was available anywhere. The change in location choice probability of new firms from the counterfactual baseline to the actual response ten years later is the Difference-in-Differences estimate of the effect of broadband deployment on locations of new firms. We find that broadband availability has a positive and significant effect on location decisions of new firms in rural areas. The broadband effect is largest in more populated rural areas and those adjacent to a metropolitan area, suggesting that broadband effect increases with agglomeration economies.

Keywords: *broadband Internet, firm location, rural, agglomeration economies*

1. Introduction

Access to broadband Internet is widely presumed to increase economic growth because it lowers firm production costs and broadens the market for firm output.[1] For example, broadband and e-commerce decrease transaction costs, ease coordination and streamline face-to-face communication with nearby upstream suppliers and downstream consumers (Gasper and Glaeser, 1998; Borenstein and Saloner, 2001; Kinsey, 2000; Henderson *et al*., 2004; Henderson, 2001; and Lamie *et al*., 2011). Broadband also helps firms reach more distant consumers and suppliers.[2] Broadband may bring footloose service jobs such as call center into rural areas (Stenberg, 2009). Broadband can facilitate better matching between firms and workers (Autor, 2001) and faster learning on market information.[3] These productivity-enhancing factors would raise the location-specific profitability of firms in areas with broadband access.[4] Therefore, in competitive markets, firms should have a higher probability of entering markets with higher anticipated profitability.

However, broadband may have negative effects on the rural economy (Fox and Porca, 2001; Malecki, 2003). Broadband benefits available for rural firms are also available for urban firms, and so broadband may allow urban firms to sell more products to rural customers.[5] Broadband may shut down rural branch offices because basic services in branch offices can be replaced with online customer services.

[1] For an extensive review on economic impacts of broadband, see Holt and Jamison (2009). For a comprehensive review on economic impacts of information technologies, see Cardona *et al*. (2013).
[2] Consumers living further from retail stores are likely to spend more over the Internet (Sinai and Waldfogel, 2004; Mishra *et al*., 2009).
[3] As suggestive evidence, another information technology, mobile phone improves access to market information and reduces price dispersion in developing countries (Jensen 2007; Aker, 2010). Aker and Mbiti (2010) review how mobile phone reduces search cost and improves coordination among firms in Africa.
4 Vu (2011) found that Broadband and Internet have larger impacts on economic growth than other information technologies such as personal computers and mobile phones.
[5] In contrast with that concern, Whitacre (2011) found that e-commerce does not harm local retail sales in rural areas.

Potential economic benefits from broadband in rural towns may be negligible due to the lack of agglomeration economies.[6] Agglomeration economies have been found to improve firm productivity and local economic growth (Puga, 2010; Duranton and Puga, 2014).[7] Broadband benefits may be largest in more densely populated areas because of complementarities between broadband and agglomeration economies,[8] and because cities have more skilled workers whose skills are complemented by information technologies (Bresnahan et al., 2002; Beaudry et al., 2010). These benefits interacted with agglomeration may attenuate with distance from the urban center (Rosenthal and Strange, 2003, 2008). Rural areas that lack agglomeration or proximity to an urban market would be expected to have the smallest marginal returns to broadband access.

This study investigates the effects of local broadband service on new firm location decisions rural markets. In this paper, we make advances on two fronts. First, we exploit advantages of location data of new firms, which allows us to do more rigorous tests than previous studies. The few empirical studies that have explored the effect of broadband on rural economy (Stenberg, 2009; Kandilov and Renkow, 2010; Mahasuweerachai *et al*., 2010; Kolko, 2012; Atasoy, 2013; Whitacre *et al*., 2014) have mixed results. Stenberg, Kolko, and Atasoy report that broadband availability increased rural economic growth. Whitacre *et al*. reported negligible impacts of broadband availability but found that rural areas with higher adoption rates grew faster. Kandilov and Renkow and Mahasureerachai *et al*. did not find significant effects

[6] A review paper of Fox and Porca (2001) on rural infrastructure investment discusses that rural areas adjacent to urban areas would have larger economic gains from improved infrastructure than remote rural areas because those areas have more local factors necessary for firm production, probably related to agglomeration economies.
[7] Agglomeration economies rely on three mechanisms: sharing, matching and learning (Duranton and Puga, 2004). Sharing facilities and infrastructures, input suppliers and a labor pool would improve firm productivity. Better matching between firms and workers, and buyers and sellers would enhance firm productivity. Faster learning new technologies, business practices and market information would increase firm productivity. Broadband and its applications such as e-commerce and wiring labor markets may improve existing agglomeration economies.
[8] Sinai and Waldfogel (2004) and Bekkerman and Gilpin (2013) empirically support the complementarity between Internet and cities. They found that residents in larger cities are likely to use more locally accessible information.

from local broadband service. This study seeks to resolve this ambiguity by focusing on the location decisions of start-up firms. New firm location decisions are predicated on current local infrastructure including whether or not broadband service is available, whereas most existing firms in the location entered before broadband was available in any market. The previous studies measured the effects of broadband availability on aggregate employment or establishment, but these measures are dominated by the decisions of firms whose location decisions were unrelated to broadband availability and for whom the cost of relocation would be much larger than any potential return from broadband availability. Newly entering firm location decisions would be the most sensitive to the presence or absence of local high-speed Internet service.[9]

Another advantage to this study is its ability to control for unobservable firm-specific and location-specific fixed factors that cloud previous measured effects of local broadband availability on local economic growth. Broadband will most likely be installed in areas that are already more profitable for new firm entry, requiring a control for preexisting, location-specific fixed factors that influence profitability even without the broadband availability. As evidence, the correlation between broadband availability in a rural ZIP code in 1999 and new firm entry in the same ZIP code in 1990-1992 before broadband was available anywhere is 0.49.[10] Cleary, broadband availability in a ZIP code is predicated on past conditions for growth in the ZIP code which can lead to spurious correlation between current local broadband availability and contemporaneous local economic growth. However, this correlation between current broadband and past growth allows us to estimate a "counterfactual" broadband effect on location choice probability of new firms before broadband was available anywhere. The change in location

[9] Rosenthal and Strange (2003) and Jofre-Monseny *et al*. (2011) advanced similar arguments to justify their focus on new firm location decisions.
[10] This correlation is based on non-agricultural and non-mining rural firms in Iowa and North Carolina.

choice probability from the counterfactual entry rate to the location choice probability after broadband started to become available in early 2000s is interpretable as the Difference-In-Differences measure of the broadband effect on location choices of new firms.

We apply our method to data on the universe of new firm start-ups in rural areas of Iowa and North Carolina. We choose rural areas because very rapid deployment of broadband eliminated meaningful variation in broadband availability in urban areas. Broadband deployment started in 1998 and spread quickly in urban areas that had the largest customer base.[11] In urban Iowa and North Carolina, 67% of ZIP codes had at least one provider within a year. In contrast, broadband deployment was considerably slower in rural areas with only 35% of rural ZIP codes having service within one year in Iowa and North Carolina. We find that rural firms are significantly more likely to locate in areas with broadband availability. The effect is larger in rural areas adjacent to a metropolitan area or with larger population.

Federal and state governments have invested considerable resources to encourage rural broadband deployment and to reduce the digital divide between urban and rural areas (Gilroy and Kruger, 2013; NCSL, 2012). Our findings support the view that rural firms are more likely to enter a market with broadband availability. However, our findings do not suggest that universal rural broadband deployment will cause the gap in economic growth between urban and rural areas to close. While broadband availability will increase the likelihood that a firm will locate in a rural area relative to other rural towns lacking broadband, the total number of firms locating in rural towns may not be affected by broadband. Moreover, the complementarity between broadband and agglomeration suggests that broadband is most valuable to the rural places close

[11] Faulhaber (2002) dates the timing of the earliest available broadband service during 1998 although the legal basis for broadband deployment was set by the 1996 Telecommunications Act.

to urban markets or with greater population. The uneven deployment of broadband across rural locations has caused rural firms to concentrate in a small number of towns, the resulting agglomeration may continue to favor firm location in these relatively few towns, even if broadband access were to be made universal. Future research will need to investigate whether broadband deployment into rural markets increases the total number of rural firm start-ups or just redistributes them toward the markets with available broadband service.

2. Literature review

There is overwhelming evidence that Information Technology (IT) raises productivity, which is reviewed in Cardona et al. (2013).12 Productivity gains from IT are also found in developing countries such as Brazil and India, as well (Commander et al., 2011). Firms that adopted IT earlier experienced more rapid productivity gains than similar firms that did not (Dunne et al., 2004). Workers who worked in firms that used information technologies more intensely experienced faster wage growth than comparable workers in firms lacking IT investments (Autor, Katz and Kruger, 1998; Acemoglu, 2002). These findings are consistent with predictions of endogenous growth theory (Romer, 1986); generation and distribution of information and ideas are important factors in economic growth. Specifically, IT raises firm productivity because it decreases the cost of communication and information processing, changes business processes and work practices (Brynjolfsson and Hitt, 2000), and creates new products and values through e-commerce (Borenstein and Saloner, 2001). Röller and Waverman (2001) show that the growth effect from IT occur generally across countries, using an analysis of the spread of voice telephony infrastructure.

[12] As one example, Jorgenson, Ho and Stiroh (2008) found that IT investments were responsible for 33% of total factor productivity growth and 32% of labor productivity growth between 1959-2006. The importance of IT has increased so that by 1995-2000, IT represented 58% of total factor productivity growth and 59% of labor productivity growth.

Numerous studies have shown productivity gains from broadband deployment. Grimes et al. (2012) found that in New Zealand, higher Internet connection speed through broadband raised firm productivity compared to firms with no connection or firms that only had access through dial-up service. Gillett et al. (2006), Shideler et al. (2007), Crandall et al. (2007), Koutroumpis (2009), Czernich et al. (2011), Kolko (2012), and Atasoy (2013) all found that broadband deployment is positively associated with economic growth. Ford and Koutsky (2005) found that broadband increases per-capita gross sales. Mack et al. (2011) found that the presence of broadband is important to firm location in a subset of service industries such as information, and finance and insurance. The review by Holt and Jamison (2009) confirm these positive broadband impacts from other empirical studies.

A challenge that has plagued all such studies is the endogeneity of broadband deployment. Economic growth in the United States has been concentrated in populous areas (Rosenthal and Strange, 2004), areas that also attracted early broadband deployment. That complicates identification of the unique broadband effect independent of correlated local factors that also affect growth. The review by Holt and Jamison (2009) notes that there are several studies that have found localized economic growth following broadband deployment, but all are subject to skepticism regarding their identifying restrictions. To confront this concern, Kolko (2012) used an instrumental variable approach which used the average slope of local terrain as an instrument for local broadband penetration. The instrument is only valid if local topography does not affect local employment growth, an assumption which may not be valid as he acknowledges, and his instrumental variable estimates of the broadband effect on employment growth are implausibly large.

A second challenge faced by researchers is that the very rapid deployment of broadband eliminated most meaningful variation in access across urban areas. The Federal Communications Commission estimated that by 1999, 59% of ZIP codes representing 91% of the population in the United States had at least one broadband provider (FCC, 2000, p.37), even though broadband deployment began in earnest just one year earlier. As a result, studies focused on the effects of broadband on growth in metropolitan areas have had to rely on variation in the number of providers rather than on the presence or absence of service, even though it is the presence versus absence of broadband that should have the largest impact on growth. Furthermore, changes in the number of broadband providers in metropolitan areas would be due in part to the exit of providers from unprofitable areas as well as added providers to the most rapidly growing areas, adding an additional source of endogeneity in measured local broadband service.

Deployment was much slower in rural than in urban areas. Only 35% of the rural zip codes in Iowa and North Carolina had access by 1999 and only 52% by 2002. In contrast, 67% of urban ZIP codes had access by 1999 and 80% by 2002. If it is the presence or absence of broadband that is most important for local economic growth as opposed to variation in the number of local broadband providers, there will be more fruitful variation to exploit in rural areas.

An additional advantage of studying the impact of broadband on economic development in rural areas is the near one-to-one correspondence between a community and a zip code. This is important because broadband deployment is reported at the zip code level. Consequently, one can tie growth of a distinct zip code area to broadband service provision for the same area. In urban areas where broadband deployment is spread over multiple zip codes, it is more difficult to tie a community to a given zip code area.

There are also reasons to suspect that broadband service may be particularly important in rural markets. Agglomeration economies led to the creation of cities (Quigley, 1998; Glaeser, 2008) and explain the persistent wage gap favoring urban workers over rural workers (Renkow, 1996; Mills and Hazarika, 2001). The Internet has the potential to change the geography of production. Services may be produced at a distance from the customers of the service. Stages of production may be geographically dispersed and still coordinated. Consequently, proximity between employer and employee or customer and producer may become less important. The possibility of telecommuting also makes it potentially feasible for workers in rural areas to earn back some of the agglomeration surplus that previously only went to metropolitan workers. These possibilities have led some to conjecture that high-speed Internet will create communities of electronically linked rather than spatially linked individuals. Liebowitz (2002) predicted that the Internet will reduce the advantage of "locational monopolies" by which an urban company's proximity to its customers gave it a competitive advantage. If these conjectures are true, there should be substantial benefits for new firms to locate in rural areas that offer broadband service compared to rural areas that do not.

3. Model

Our model illustrates the role of locational fixed factors on new firm start-ups and offers an avenue by which those fixed factors may be held fixed in empirical applications. To that end, suppose that we have J areas (j=1, 2, …, J) which are defined geographically by ZIP codes. These J areas are distributed across C counties (c=1,…,C). We define t=0 for a period before broadband was available in any of the *J* areas. Period t=1 designates a time when broadband was available in at least some but not all of the *J* areas.

Price-taking firms maximize their profit in two stages. In the first stage, firm *i* calculates its expected profit in each area *j* at time *t*. Then the firm chooses the location with maximum profit (π_{it}^{*}) in the second stage:[13]

$$\pi_{it}^{*} \equiv Max_j \pi(I_{jt}, z_{jt}, m_c, \mu_j, p_t, w_{jt}, r_{jt})$$

where location *j* is included in county *c*. Firm profit (π) is affected by broadband availability (I_{jt}). Local demand shifters, z_{jt}, are measured by the income and education level of residents in the locality and may increase or decrease firm profits. County and state characteristics (m_c) include dummy variables indicating adjacency to a metropolitan area and size of urban population, which may be related to agglomeration economies improving firm productivity. m_c also includes a dummy variable indicating whether the ZIP code is located in North Carolina or Iowa. Firm profit (π) increases in the common market price p_t and location-specific fixed effects (μ_j), and decreases in local wages (w_{jt}) and the rental rate on capital (r_{jt}).

We assume a spatial equilibrium where wages and capital costs are adjusted to local attributes affecting firm productivity (Rosen, 1979; Roback, 1982). If areas are competitive, firms will expect to make zero economic profits in all areas. If areas that acquire broadband access (I_{jt}=1) increase firms productivity and profitability, the areas will attract additional entry relative to areas that do not have broadband access ($I_{j't}$=0). Entering firms will bid up the input prices for labor and capital until expected profits from additional entry are reduced back to zero.

[13] Empirically, new firm entry is an appropriate indicator for future economic growth. Reviews by Carree and Thurik (2010), and Fritsch (2011) found that new firm entry increases local economic growth.

Hence, wages and rents will also be functions of local attributes such as μ_j and I_{jt}. Absent any other sources of productivity differences between the two areas, wages and rents would have been identical. Of course, that is too strong an assumption, and so we allow additional variation in local demand and location-specific labor productivity differences in the form of z_{jt} and m_c.

At time period 1, the linear approximation to our reduced form profit for firms in area *j* is:

$$\pi_{ij1}^{*} \equiv \gamma_I^1 I_{j1} + \gamma_z^1 z_{j1} + \gamma_m^1 m_c + \mu_j + \varepsilon_i + \varepsilon_1 + \varepsilon_{j1} \quad (1)$$

where superscripts on the parameters indicate the time period. The error term ε_i is unobservable firm-specific characteristics. ε_1 is a common factor that affects profitability in all areas such as a country-wide expansion or recession. ε_{j1} reflects transitory factors that the firm observes in assessing its profits in area *j* but that are not observed by the econometrician.

In principle, if we observe the fixed effect μ_j, we can estimate equation (1) directly. However, we do not observe μ_j. If the fixed effect is correlated with broadband availability, the estimated broadband effect would be biased. To address this issue, we use a counterfactual broadband availability when broadband was not available anywhere. To derive the counterfactual, we begin with the linear approximation to the firm's profit function in area *j* at time period 0:

$$\pi_{ij0}^{*} \equiv \gamma_z^0 z_{j0} + \gamma_m^0 m_c + \mu_j + \varepsilon_i + \varepsilon_0 + \varepsilon_{j0} \quad (2)$$

If we introduce broadband availability counterfactually into equation (2), its estimated coefficient would reflect its correlation with the fixed effect (μ_j). Recall from the introduction that broadband availability in 2000 is highly correlated with new firm entry a decade earlier. That correlation will allow us to estimate the impact of the fixed effects on firm entry in period 0 which will in turn, allow us to take out the fixed effect bias on our estimate of broadband access in period 1.

Consider the projection of the area *j* fixed effect on past and current observed market factors plus the broadband availability indicator in period 1 (I_{j1}, z_{j0}, z_{j1} and m_c):

$$\mu_j \mapsto \theta_I I_{j1} + \theta_z^0 z_{j0} + \theta_z^1 z_{j1} + \theta_m m_c + \omega_j \quad (3)$$

where ω_j is an *i.i.d.* error composed of elements of the fixed effect that are uncorrelated with the presence of broadband or of other local factors. Each coefficient in equation (3) reflects its correlation with the fixed effect. Replacing μ_j in equation (1) with equation (3), we rewrite equation (1):

$$\pi_{ij1}^* = (\gamma_I^1 + \theta_I)I_{j1} + \theta_z^0 z_{j0} + (\gamma_z^1 + \theta_z^1)z_{j1} + (\gamma_m^1 + \theta_m)m_c + \varepsilon_i + \varepsilon_1 + \varepsilon_{j1} + \omega_j \quad (4)$$

Note that θ_I represents a bias in estimated broadband effect. Replacing μ_j in equation (2) with equation (3), we rewrite equation (2):

$$\pi_{ij0}^* = \theta_I I_{j1} + (\gamma_z^0 + \theta_z^0)z_{j0} + \theta_z^1 z_{j1} + (\gamma_m^0 + \theta_m)m_c + \varepsilon_i + \varepsilon_0 + \varepsilon_{j0} + \omega_j \quad (5)$$

Note that the coefficient on the counterfactual broadband availability I_{j1} in equation (5) is θ_I, the bias in the estimated broadband effect in equation (4). We can tease out the true broadband effect γ_I^1 by merging equation (4) and equation (5) under Difference-In-Differences framework:

$$\pi_{ijt}^* \equiv (\gamma_I^1 D_{t=1} + \theta_I)I_{j1} + (\gamma_z^0 D_{t=0} + \theta_z^0)z_{j0} + (\gamma_z^1 D_{t=1} + \theta_z^1)z_{j1} + (\gamma_m^t + \theta_m)m_c + \varepsilon_i + \varepsilon_t + \varepsilon_{jt} + \omega_j \quad (6)$$

where $D_{t=\tau}$ is a dummy variable indicating time period τ.

To estimate equation (6), we use the conditional logit model; each new firm chooses one of the potential *J* areas to enter, based on anticipated profitability. Define the dichotomous variable $E_{ijt} = 1$ if the firm opts to enter area *j* in period *t* and $E_{ijt} = 0$ otherwise. Specifically,

$$E_{ij\mathrm{t}} = 1 \text{ if } (\gamma_I^1 D_{t=1} + \theta_I)\left(I_{j1} - I_{j'1}\right) + (\gamma_z^0 D_{t=0} + \theta_z^0)\left(z_{j0} - z_{j'0}\right) +$$

$$(\gamma_z^1 D_{t=1} + \theta_z^1)(z_{j1} - z_{j'1}) + (\gamma_m^t + \theta_m)(m_c - m_{c'}) > \zeta_{ijt} \ \ \forall j \neq j^{'} \qquad (7)$$

where $\zeta_{ij\mathrm{t}} \equiv \left(\omega_{j'} + \varepsilon_{j't}\right) - (\omega_j + \varepsilon_{jt})$. If the error term $\omega_j + \varepsilon_{jt}$ follows the type 1 extreme distribution, we can estimate equation (7) using the conditional logit estimation.

Our identification of the true broadband effect on new firm entry relies on the assumed independence between broadband availability (I_{jt}) and the error terms ($\varepsilon_i + \varepsilon_t + \varepsilon_{jt} + \omega_j$). If this assumption is violated, then estimates of γ_I^1 will be biased. But two of those error terms, the firm-specific effects ε_i and the common economic shock ε_t, are differenced away in the conditional logit estimation as they do not affect relative profitability across areas. The fourth error source, ω_j, is not correlated with I_{jt} by construction in equation (3).

The only error source that remains as a potential source of bias is the unobserved time-varying effect ε_{jt}. This will bias our estimates if ε_{jt} is correlated with I_{jt}. This would happen if larger rural towns grow faster than smaller rural towns over time, and broadband deployment is sorted into larger rural towns. That would create a positive correlation between the error term and the observed broadband dummy variable I_{jt}, and that would cause an upward bias in the estimated broadband effect. We will test the proposition by examining whether changes in years for period 0 alter the estimated impact of broadband availability. If ε_{jt} is correlated with I_{jt}, then transitory changes in the error terms generated by changing years for period 0 will affect the estimated broadband effect. As we will show later, we do not find significant changes in the broadband effect by altering the time period used in estimation.

To mitigate the potential violation of Independence of Irrelevant Alternatives (IIA) assumption underlying the conditional logit model, our specification includes two dummy variables indicating adjacency to a metropolitan area and size of urban population in the county based on Rural-Urban Continuum Codes (RUCC). Our concern is that ZIP codes may be closer substitutes for other ZIP codes located the same distance from a metropolitan area or that have similar population densities. To address that concern, Bartik (1985) introduced a strategy of grouping alternatives by close substitutability. Levinson (1996) applied the strategy to examine how environmental regulations affect the siting of manufacturing establishments.

Our estimation uses six years of new-firm location data: 1990-1992 and 2000-2002. For new firms in 2000-2002, we use one-year lagged broadband availability. We pick one year from 1999-2001 for the counterfactual broadband availability for all new firms in 1990-1992 and 2000-2002 in order to allow for possible reporting error on which ZIP codes had service. As we will show later, the estimated broadband effects are consistent regardless of the years for counterfactual broadband availability.

4. Data

We define ZIP codes in counties with urban population less than 20,000 as "rural" based on 1993 Rural-Urban Continuum Codes (RUCC). In our empirical model, each new firm chooses one out of 1,015 rural ZIP codes across the two states, which sets J=1,015. These ZIP codes are distributed across 137 rural counties across the two states, which sets C=137.

We apply our empirical model on a sample of 63,341 "commercial" establishments that entered a rural ZIP code in either Iowa or North Carolina during years 1990-1992 and 2000-2002.[14] We restrict the sample to firms with a clear profit motive, and so we exclude non-profit organizations, government agencies and establishments with a public service emphasis such as museums or historical sites. We also remove firms in agriculture and mining because they cannot move freely across locations as their entry decision is affected by site-specific land or resource availability.[15] Firm attributes such as ZIP code-level location and industry are obtained from the National Establishment Time Series (NETS) which provides information on the universe of all firms that opened for business in Iowa and North Carolina in 1990-1992 or 2000-2002 that had a Duns number.[16] These proprietary data are available at a per state fee, and so our choice of states is based on a budget constraint and a decision to pick two states from different economic regions that had many small counties across a broad continuum of rural and urban settings.

We obtain broadband availability information from Federal Communications Commission (FCC) Form 477. Broadband is a general term for communication technologies enabling "high-speed" data transmission. FCC defines data transmission faster than 200 Kbps in at least one direction "high-speed." Broadband is contrasted with dial-up connection to Internet less than 56 Kbps. In the early 2000s, cable and DSL broadband platforms were popular, but

[14] In our paper, we use terms firm and establishment interchangeably.

[15] The following industries are excluded: Agriculture (2-digit 2002 NAICS 11), Mining (21), Postal Service (3-digit NAICS 491), Monetary Authorities-Central Bank (521), Nursing and Residential Care Facilities (623), Social Assistance (624), Museums, Historical Sites, and Similar Institutions (712), Religious, Grantmaking, Civic, Professional and Similar Organizations (813), Private Households (814), and Public Administration (2-digit NACIS 92).

[16] Kunkle (2011) discusses advantages of NETS data compared to public data such as the Quarterly Census of Employment and Wages (QCEW). Differently from public data based on unemployment insurance filing, NETS data include very small establishments such as sole-proprietors. Excluding establishments having less than 3 employees does not change our results. Estimates are available from the authors upon request.

fixed wireless and satellite broadband platforms were rare.[17] The Form 477 reports the number of broadband service providers with subscribers in each ZIP code. We create a broadband availability dummy variable (I_{j1}), which is equal to one if the ZIP code has at least one broadband provider and zero otherwise. We use broadband availability in December, 1999-2001, which are one-year lagged compared to our sample of new firms.

The broadband availability variable (I_{j1}) is subject to measurement errors that may bias our results. Our measured broadband availability only indicates that service is available somewhere in the ZIP code, not that it is available everywhere within the ZIP code. For example, ZIP codes with at least one satellite broadband subscriber would be reported to have broadband although its subscription had very small portion of high-speed lines in early 2000s.[18] This problem is more severe in rural areas because on average, rural ZIP codes span a greater area than urban ZIP codes (Gillett, 2006). This overstatement might lead to underestimation of the broadband effect if many areas are characterized by low broadband penetration rates. Luckily, broadband effects appear at penetration rates as low as 10-20% (Czernich *et al*. 2011), so we are unlikely to miss effects by overstating rural broadband penetration.

Broadband availability can be understated because providers with less than 250 lines in the state are not required to report to the FCC. It is possible that rural ZIP codes are covered by very small broadband providers who do not report to the FCC. If so, then our control group of ZIP codes lacking broadband service will be contaminated by areas that do in fact have service.

[17] Cable (51.3%), ADSL (Asymmetric Digital Subscriber Line; 13.4%), fiber (11.3%), wireless and satellite (1.8%) and other wirelines (22.1%) in terms of shares of high-speed lines as of December 1999 (FCC, 2005).

[18] To avoid the overstatement of broadband availability from satellite broadband, Mahasuweerachai et al. (2010) used placement of DSL and cable modem platforms. This kind of information may not be appropriate for our study because "technical" broadband availability does not necessarily mean existence of broadband subscribers. We compared FCC form 477 with Iowa Utility Board broadband survey, and found that there were many rural ZIP codes where broadband was technically available but did not have any broadband subscribers in 2000 and 2001.

Lacking data in these very small providers, we cannot test formally for the importance of the problem beyond noting that this would tend to bias our estimates against finding an impact.

The other included time-varying local attributes (z_{jt}) are education and income levels of residents in the ZIP code. The education variable is measured by people over 25 years old with at least a two-year college degree in that ZIP code, and the income variable is median household income in the ZIP code. Those measures are available from the 1990 and 2000 Census. Given significant travel costs, these variables are expected to reflect local demand for goods and services that are presumed to have an impact on local firm profitability.

County and state characteristics (m_c) consist of three dummy variables indicating whether the county is adjacent to a metropolitan area, whether the county has at least 2,500 urban population, and whether the ZIP code is located in North Carolina rather than in Iowa. The first two variables are based on 1993 Rural Urban Continuum Code (RUCC).[19] While other justifications for including these measures can be advanced, our interest relates to the plausible importance of agglomeration economies as possible complements with or substitutes for broadband availability. Agglomeration economies can improve firm productivity by promoting technology diffusion and innovation (Rosenthal and Strange, 2004). Proximity to upstream suppliers and downstream customers can decrease transaction costs. As opposed to more remote ZIP code areas, these benefits are presumably larger in rural areas adjacent to a metropolitan area or areas with more dense populations (Partridge *et al.*, 2008). However, broadband may alter the importance of proximity which would make its availability even more important in remote counties.

[19] Counties in RUCC 6 and 7 have urban population of 2,500 to 19,999 while those in RUCC 8 and 9 have less than 2,500 urban populations. Counties in RUCC 6 and 8 are adjacent to a metropolitan statistical area.

Table 1 presents the 1990 and 2000 average education and income levels for ZIP codes with and without broadband availability in 2000. Recall that broadband was not available in 1990, but even then, education and income are higher in ZIP codes that had the earliest access to broadband service. Education and income rise between 1990 and 2000 in both ZIP code groups but remain significantly larger in ZIP codes with broadband availability. It is apparent why these time varying, location-specific attributes must be incorporated into the analysis as persistent differences in income and education levels are correlated with local broadband availability. Moreover, if firm entry responds to positively to local income and human capital levels, we will have greater firm entry in the broadband ZIP codes due to their advantages in income and education, even if broadband availability has no effect. Our estimates of the impact of broadband availability on new firm entry will be purged of these potentially confounding effects of local education and income on firm entry and early access to high-speed Internet service.

We include 1,015 ZIP Code Tabulation Areas (ZCTA) in our data set. We required a consistent geographical area over the two periods separated by ten years. We assume that the geographical boundaries of ZIP codes are consistent between 1990 and 2000 if the ZIP code numbers are the same over time. We also assume that U.S. Postal Service ZIP codes indicate the same areas as ZCTA codes indicate. Of 1,031 rural ZCTA codes in 2000, 952 were matched to corresponding 1990 Census ZIP codes exactly. The reminder of ZCTA codes was matched with 1990 Census ZIP codes closest to them in terms of distance between geographic coordinates provided by Census Gazetteer Files. 16 ZCTA out of 1,031 were excluded because they did not have any firm entrants in any of the six years (1990-1992 and 2000-2002).

5. Results

Before turning to the results from our estimation strategy, we illustrate the type of results obtained when endogenous broadband provision is not controlled. These estimates assume that firms are selecting the highest expected profit π^*_{ij1} from all J markets in equation (1). These estimates will control for the firm-specific and time-specific errors, ε_i and ε_1, but they will still be biased if the location-specific fixed effect μ_j is correlated with I_{j1}.[20]

The first specification assumes that broadband provision is exogenous, and so only observable variables from period 1 (I_{j1}, z_{j1} and m_c in equation (1)) are included in the estimation. In this case, the fixed effect (μ_j) is an omitted variable in the specification and will be included in the error term. If the fixed effect is positively correlated with broadband availability (I_{j1}), the estimated broadband coefficient will be overestimated. We report the coefficients and then, in brackets, the implied proportional changes in the probability of firm entry relative to not having local broadband service.[21] To put the proportional changes in context, note that the average probability that a firm picks any random zip code is 0.001. The estimated broadband effect in column (1) of Table 2 implies that the firm entry probability increases by 280%, an implausibly large impact.

[20] The number of ZIP codes in those estimation is 1,006 since we include ZIP codes having at least one new firm entry in 2000-2002 into a choice set.

[21] The proportional change in the probability of firm entry with respect to broadband availability is calculated for each firm and each ZIP code and averaged across all firms and ZIP codes. For firm *i*, ZIP code *j*, and observed local broadband service level $\{0,1\} \in I^O_{j1}$,

$$\frac{\partial P(E_{ij}=1)}{\partial I_{j1}} \cdot \frac{1}{P\left(E_{ij}=1 \middle| I^O_{j1}\right)} \approx \frac{P\left(E_{ij}=1 \middle| I_{j1}=1\right) - P\left(E_{ij}=1 \middle| I_{j1}=0\right)}{P\left(E_{ij}=1 \middle| I^O_{j1}\right)}$$

where $P\left(E_{ij}=1 \middle| I_{j1}=1\right)$ is the probability that firm *i* chooses ZIP code *j* when broadband is available in that ZIP code, and $P\left(E_{ij}=1 \middle| I^O_{j1}\right)$ is the probability that firm *i* chooses ZIP code *j* when broadband service is set at the observed service level.

The second specification adds a past number of new firm entrants as a proxy for the location-specific fixed effect μ_j in equation (1).[22] However, an examination of equation (2) shows that the past numbers of firm entrants are also dependent on past values of z_{j0}, m_c, ε_i and ε_0. The past number of new firms is almost certainly correlated with those past values in (1) which would bias the coefficients. As a result, we build in other sources of bias using this strategy. As shown in column (2) of Table 2, the proportional change in the probability of firm entry attributed to local broadband availability is 1.63 which is still implausibly large.[23]

In Table 3, we report our Difference-In-Differences impact of broadband availability on location choice probability of new firms. The top half of panel (a) reports the counterfactual coefficients θ_I from equation (7) for local broadband availability in the [1999-2001] period on location choice probability in the 1990-1992 period. The estimates are done three times, one with broadband availability as reported in 1999, the second with broadband availability in 2000 and the third with availability in 2001. Results are not overly sensitive to the timing of broadband service. The bottom half of panel (a) reports γ_I^1: the additional impact of broadband availability in 1999-2001 on firm entry a year later, which should be the true broadband effect controlling for unobservable location-specific fixed effects. The γ_I^1 coefficients are converted into their implied marginal effects on probability of entry which are reported in brackets. In column (1) with counterfactual broadband availability in 1999, the estimate of γ_I^1 is 0.66 with an implied proportional change in probability of firm entry attributed to local broadband equal to 0.71. Using our average probability that a randomly chosen firm picks a given location as the

[22] Similarly, Jofre-Monseny *et al*. (2011) argued that including the past number of existing firms would control for unobserved location-specific fixed effects.
[23] Kolko (2012) also found implausibly large broadband impact on employment growth from his instrumental variable estimation; a unit of increase in broadband increases employment growth by 64 percent points over 7 years (1999-2006).

baseline, broadband availability increases the probability that a firm chooses that location from 0.001 to 0.0017. Similar results are found in columns (2) and (3). Our finding that broadband raises new firm entry probability is consistent with Stenberg (2009), Kolko (2012), Atasoy (2013) and Whitacre *et al*. (2014), but our estimates of the proportional change in the probability of firm entry due to broadband ranging from 0.59 to 0.98 are of more plausible magnitudes than the much larger effects found in Table 2.

In columns (4) to (6) of Panel (a) in Table 3, we add interactions between broadband availability (I_{j1}) and two county characteristics: adjacency to a metropolitan area and size of urban population. The top half of Panel (a) shows that the correlation between later broadband availability and the location-specific fixed effect is largest in more populated counties that are distant from a metropolitan area. The bottom half of Panel (a) shows that the effect of broadband on firm entry is largest in rural counties that are both adjacent to a metropolitan area and that have relatively large urban population. We summarize the implied effect of broadband on probability of firm entry in Panel (b) of Table 3. These are the average effects across the three sets of estimates in columns (4) to (6) of Panel (a). Rural counties adjacent to a metro with at least 2,500 urban populations (RUCC 6) have the largest proportional gain in probability of firm entry associated with local broadband service at 0.81. The smallest gain from local broadband service is in the least populated counties remote from a metro (RUCC 9) with a proportional gain in probability of firm entry of 0.50.

Our results suggest that local broadband availability increases new firm entry most in rural counties that are close to areas with urban agglomeration economies and that have greater population. This result is consistent with prior findings that both broadband and agglomeration are complementary with greater concentrations of skills, and so it is not surprising that agglomeration and broadband appear to be complements in production. Our findings are also consistent with the Watson *et al* (2005) finding that firms in larger rural towns have greater willingness-to-pay for e-commerce information. It contrasts with Kolko (2012) and Atasoy (2013) who found that local broadband service has the largest impact on economic growth in less densely populated areas. It may be that that broadband service has a different effect on new firm location decisions (our measure) compared to employment growth of incumbent firms (their measure), but it may also be that the bias related to unobserved location specific effects and endogenous placement of broadband service is largest in the most remote markets, a finding consistent with the larger correlation between broadband service in 1999-2001 and the rate of new firm start up-in nonadjacent rural counties reported in Table 3.

We also examine whether the broadband effects differ by industry in Table 4 by estimating coefficients associated with interactions between broadband availability (I_{j1}) and firm industry dummy variables. The joint test that the broadband effect is common across industries is reported toward the bottom of Table 4 rejects the null hypothesis of a common broadband effect across industries. However, the implied magnitude of the differences in proportional change in the probability of firm entry are very small with no differences larger than 0.04. Our finding of homogeneous broadband effects across industries is consistent with Grimes *et al*. (2012).

The literal interpretation of our finding that broadband availability raises new firm entry probability suggests that broadband presence raises firm profitability. This has to be a transitory effect as other areas for labor and capital should adjust to cause wages and rents to rise in areas where broadband raises productivity, causing profits to equalize across areas with or without broadband access. There is some evidence supportive of those wage and rent effects. Gillette et al. (2006) find that broadband Internet is positively associated with rents. Wages are less sensitive to broadband availability. Forman *et al*. (2012) find that high-speed Internet does not affect wage rates except in places with highly educated and more dense urban populations with concentrations of IT-intensive industries. Kolko (2012) finds no effect of broadband on average wages. Because capital is less mobile than labor, these findings suggest that the equalizing factor may come from a bidding up of land prices in areas that have broadband access.

As noted in our derivation of our control for location-specific fixed effects, our estimation relies on the independence between the transitory location specific profitability ε_{jt} and the installation of broadband service I_{jt}. A direct test of this assumption is not possible, but we can vary our empirical realization of ε_{jt} by changing the years of our base period before broadband was deployed. This will add new transitory components due to time-specific errors that affected profitability in the prior period compared to the end-period 2000-2002. In table 5, we set the base period as 1995-1997 rather than 1990-1992 as in Table 3. If transitory factors bias our results, we should get different results than in Table 3. However, comparing Tables 3 and 5, there are no large discrepancies in sign or magnitude. For our main concern, the range of estimated proportional change in probability of firm entry due to broadband availability is [0.52 - 0.94] in table 5 compared to [0.59 – 0.98] in Table 3.[24]

6. Discussion: Policy implication

Federal and state governments have made investments to deploy broadband Internet and close the "digital divide" between urban and rural households. The National Broadband Plan aims to establish universal broadband service by 2020 (FCC, 2010). Rural areas have been

[24] For another robustness check of our results, we run negative binomial fixed-effect panel estimation advanced by Hausman et al. (1984). In estimation with ZIP-code-specific dummy variables, we find significantly positive broadband effect. Estimation results are available from the authors upon request.

underserved by broadband Internet.[25] The federal government has subsidized rural broadband deployment through broadband programs in two federal agencies; the USDA Rural Utilities Service (RUS) and the FCC Universal Service Fund (USF) (Gilroy and Kruger, 2013). RUS mainly supports up-front capital of broadband infrastructure while USF mainly supports operation cost of broadband networks. RUS has several broadband programs such as Rural Broadband Access Loan and Loan Guarantee Program, and Community Connect Grant Program. USF has programs such as Connect America Fund (formerly, High Cost Program), and Schools and Libraries Program (E-Rate). Money was also allocated to rural broadband under the American Recovery and Reinvestment Act in 2009 (Kruger, 2009). The 2014 Farm Bill contains a Rural Gigabit Network Pilot Program aimed at bringing ultra-high-speed Internet service into rural areas. State governments also have made efforts to promote the roll-out of rural broadband; all 50 states have at least one broadband task force, commission, or a broadband project (NCSL, 2012).

Our results are consistent with the view that government broadband deployment projects in rural areas will increase the likelihood of firm entry in these areas. However, our findings do not support the contention that universal rural broadband deployment will lower the gap in urban versus rural firm start-up rates because broadband encourages the agglomeration of firms. It is possible that the existing distribution of rural broadband deployment has shifted firm entry from areas without broadband toward those with service, but this could have happened without any net gain in the number of rural firms. To establish whether rural broadband results in a net increase in the number of rural firms, we would have to have a country-level study. Koutroumpis (2009) and Czernich et al. (2011) found that broadband penetration increased economic growth in the

[25] FCC (2012) reports substantial urban-rural broadband digital divide. Also, see Dickes et al. (2010) and Whitacre et al. (2013) for more details.

OECD countries, which suggests that broadband has a net positive effect on country-level economic activity.

However, we are cautious in applying the country-level results to remote rural areas with low local agglomeration economies. Recall that the broadband availability effect is largest in counties with greater agglomeration or in close proximity to metro areas with agglomeration economies. That suggests that the smallest and most remote rural towns having few local agglomeration economies will get the smallest economic benefits from government broadband deployment projects compared to larger rural counties closer to metropolitan areas. Olfert and Partridge (2010) also emphasized that connective infrastructure between urban and rural areas is one of the best practices for rural development.

Our discussion above is limited only to economic benefits of broadband. Of course, broadband can provide other types of benefits through telemedicine (Whitacre *et al*., 2013), distance education, broader range of goods and services choices (Mishra *et al*., 2009), and improvement of community interactions (Stern *et al*., 2011). However, economic benefits and other types of benefits are related to size of the population served. Given that "the last mile" that delivers high-speed Internet service from a node of the broadband network to an individual customer represents the highest cost for broadband providers and is presumably more costly in remote rural towns, it is not obvious that the benefit from government broadband deployment exceeds the costs in remote rural towns. As Fox and Porca (2001) and Renkow (2007) suggest, selected broadband provision to rural towns where net benefit of broadband is positive may be socially desirable. And those net benefits will be largest in the relatively few rural labor markets that have sufficient population or proximity to an urban market to offer agglomeration economies that complement local broadband.

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Table 1 Education and income by broadband availability in rural Iowa and North Carolina

	1990 Census information			2000 Census information		
	Broadband in 2000: Yes	Broadband in 2000: No	z-stat. (p-value)	Broadband in 2000: Yes	Broadband in 2000: No	z-stat. (p-value)
Education	0.18	0.15	6.3 (<0.01)	0.22	0.18	7.4 (<0.01)
Income	23.5	22.6	3.3 (<0.01)	26.4	25.6	2.7 (<0.01)
# of ZIP codes	423	608	-	423	608	-

Note: Income is reported in 1,000 constant 1989$ units. Z-statistics and p-values are from the Wilcoxon Rank-sum tests of equal distributions in education and income across the two ZIP code groups.

Table 2 Effect of broadband availability on locations of new rural firms: alternative specifications

Dependent variable: ZIP-code choice of new firms in 2000-2002	(1)	(2)
One-year lagged broadband availability	1.68 (0.01)***	1.21 (0.01)***
	[2.80]	[1.63]
# of new firms in 1990-1992 divided by 100	-	1.08 (0.01)***
Education of residents in 2000	3.42 (0.05)***	1.78 (0.06)***
Income of residents in 1999	-0.03 (<0.01)***	<0.01 (<0.01)***
Adjacent to metro areas (=1)	Yes	Yes
Urban population (2,500+) (=1)	Yes	Yes
Located in North Carolina? (=1)	Yes	Yes
Log-likelihood	-290,844.60	-280,831.54
# of new firms / # of ZIP codes	44,739 / 1,006	

Note: Conditional logit estimation of variations of equation (1). Standard errors are in parentheses. Proportional changes in the probability of firm entry are in the brackets. *** indicates significance at the 1% level.

Table 3 Effect of broadband availability on locations of new rural firms

(a) Estimation results

Dependent variable: ZIP-code choice of new firms in 1990-1992 and 2000-2002		(1) t=1999	(2) t=2000	(3) t=2001	(4) t=1999	(5) t=2000	(6) t=2001
Counterfactual broadband effect (1990 – 1992)	Broadband availability A(t): θ_I	1.28 (0.01)***	1.32 (0.01)***	1.19 (0.01)***	0.79 (0.03)***	1.10 (0.03)***	0.89 (0.03)***
	Broadband availability A(t) x Adjacent to metro areas (=1)	-	-	-	-0.10 (0.02)***	-0.28 (0.03)***	-0.16 (0.03)***
	Broadband availability A(t) x Urban population (2,500+) (=1)	-	-	-	0.67 (0.03)***	0.49 (0.03)***	0.49 (0.03)***
Broadband effect (2000 – 2002)	Broadband availability B: γ_I^1	0.66 (0.02)*** [0.71]	0.55 (0.02)*** [0.59]	0.85 (0.02)*** [0.98]	0.58 (0.03)*** [0.62]	0.25 (0.04)*** [0.25]	0.58 (0.03)*** [0.62]
	Broadband availability B x Adjacent to metro areas (=1)	-	-	-	0.01 (0.03) [0.01]	0.20 (0.03)*** [0.19]	0.05 (0.03)* [0.05]
	Broadband availability B x Urban population (2,500+) (=1)	-	-	-	0.10 (0.04)*** [0.10]	0.25 (0.04)*** [0.25]	0.32 (0.03)*** [0.33]
Log-likelihood		-408,649.15	-409,847.64	-410,957.88	-408,194.32	-409,487.42	-410,653.42
# of new firms / # of ZIP codes		63,341 / 1,015					

Note: Conditional logit estimation based on equation (7). Broadband availability A(t) denotes broadband availability in t for all new firms in 1990-1992 and 2000-2002. Broadband availability B demotes one-year lagged broadband availability for new firms in 2000-2002. Control variables include education and income of residents in 1990 and 2000, and county and state characteristics dummies. Standard errors are in parentheses. Proportional changes in the probability of firm entry are in the brackets. *** and * indicate significance at the 1% and 10% level, respectively.

(b) Proportional changes in location choice probability of new firms by county characteristics by county population and proximity to a metro

Rural Urban Continuum Code (RUCC)	Adjacent to a metro area?	Urban Population	Proportional changes in location choice probability of new firms
6	Yes	$2{,}500 \leq$ Population $<20{,}000$	0.81
7	No	$2{,}500 \leq$ Population $<20{,}000$	0.72
8	Yes	Population $< 2{,}500$	0.58
9	No	Population $< 2{,}500$	0.50

Table 4 Effect of broadband availability on locations of new rural firms by industry

Dependent variable: ZIP-code choice of new firms in 1990-1992 and 2000-2002		(1) t=1999	Effect[a]	(2) t=2000	Effect[a]	(3) t=2001	Effect[a]
Counterfactual broadband effect (1990-1992)	Broadband availability A(t)	Yes		Yes		Yes	
	Broadband availability A(t)×eight industrial dummies	Yes		Yes		Yes	
	Interaction between broadband availability B and:						
Broadband effect (2000 – 2002)	Construction	0.11 (0.07)*	[0.02]	-0.06 (0.07)	[-0.01]	-0.03 (0.06)	[<0.01]
	Manufacturing	-0.11 (0.09)	[<0.01]	-0.22 (0.09)**	[-0.01]	-0.18 (0.08)**	[-0.01]
	Trade, Transportation and Utilities	0.04 (0.06)	[0.01]	0.02 (0.06)	[0.01]	0.01 (0.06)	[<0.01]
	Information	0.09 (0.13)	[<0.01]	0.08 (0.14)	[<0.01]	0.08 (0.12)	[<0.01]
	Financial Activities	0.25 (0.07)***	[0.03]	0.15 (0.08)*	[0.01]	0.21 (0.07)***	[0.02]
	Professional and Business Services	0.10 (0.06)*	[0.03]	-0.18 (0.06)***	[-0.04]	-0.16 (0.06)***	[-0.04]
	Education and Health Services	0.33 (0.09)***	[0.02]	0.18 (0.10)*	[0.01]	0.38 (0.09)***	[0.02]
	Leisure and Hospitality	0 (0.08)	[<0.01]	0.09 (0.08)	[0.01]	0.01 (0.07)	[<0.01]
	Broadband availability B (Reference=Other Services)	0.57 (0.05)***	[0.61]	0.58 (0.05)***	[0.62]	0.87 (0.05)***	[0.99]
Log-likelihood		-408,502.06		-409,717.92		-410,822.06	
Test of the joint hypothesis of equal broadband effects across industries		572.2***		659.0***		569.1***	
# of new firms / # of ZIP codes				63,341 / 1,015			

Note: Broadband availability A(t) demotes broadband availability in t for all new firms in 1990-1992 and 2000-2002. Broadband availability B denotes one-year lagged broadband availability for new firms in 2000-2002. Control variables include education and income of residents in 1990 and 2000, and county and state characteristics dummies. Standard errors are in parentheses. ***, ** and * indicate significance at the 1%, 5%, or 10% level.

[a] Proportional changes in location choice probability of new firms due to local broadband availability are in brackets.

Table 5 Robustness check: Unobservable time-varying location-specific factors

Dependent variable: ZIP-code choice of new firms in 1995-1997 and 2000-2002		(1) t=1999	(2) t=2000	(3) t=2001	(4) t=1999	(5) t=2000	(6) t=2001
Counterfactual broadband effect (1995 – 1997)	Broadband availability A(t): θ_I	1.37 (0.01)***	1.42 (0.01)***	1.29 (0.01)***	0.93 (0.02)***	1.17 (0.03)***	0.93 (0.03)***
	Broadband availability A(t) x Adjacent to metro areas (=1)	-	-	-	-0.07 (0.02)***	-0.27 (0.02)***	-0.13 (0.03)***
	Broadband availability A(t) x Urban population (2,500+) (=1)	-	-	-	0.61 (0.03)***	0.52 (0.03)***	0.55 (0.03)***
Broadband effect (2000 – 2002)	Broadband availability B: γ_I^1	0.59 (0.02)*** [0.64]	0.48 (0.02)*** [0.52]	0.81 (0.01)*** [0.94]	0.47 (0.03)*** [0.50]	0.18 (0.03)*** [0.18]	0.55 (0.03)*** [0.60]
	Broadband availability B x Adjacent to metro areas (=1)	-	-	-	0.02 (0.03) [0.01]	0.21 (0.03)*** [0.11]	0.06 (0.03)* [0.03]
	Broadband availability B x Urban population (2,500+) (=1)	-	-	-	0.14 (0.04)*** [0.11]	0.23 (0.04)*** [0.17]	0.30 (0.03)*** [0.22]
Log-likelihood		-479,454.99	-480,967.02	-482,655.80	-478,993.62	-480,549.52	-482,304.46
# of new firms / # of ZIP codes		74,634 / 1,017					

Note: Broadband availability A(t) denotes broadband availability in t for all new firms in 1990-1992 and 2000-2002. Broadband availability B denotes one-year lagged broadband availability for new firms in 2000-2002. Control variables include education and income of residents in 1990 and 2000, and county and state characteristics dummies. Standard errors are in parentheses. Proportional changes in the probability of firm entry are in the brackets. *** and * indicate significance at the 1% and 10% level, respectively.

Appendix A: Effect of broadband availability on location choices of new rural firms: Establishments with three or more employees

Dependent variable: ZIP-code choice of new firms in 1990-1992 and 2000-2002		(1) t=1999	(2) t=2000	(3) t=2001	(4) t=1999	(5) t=2000	(6) t=2001
Counterfactual broadband effect (1995-1997)	Broadband availability A(t): θ_I	1.36 (0.02)***	1.38 (0.02)***	1.26 (0.02)***	0.82 (0.04)***	1.11 (0.04)***	0.88 (0.04)***
	Broadband availability A(t) x Adjacent to metro areas	-	-	-	-0.11 (0.04)***	-0.31 (0.04)***	-0.15 (0.04)***
	Broadband availability A(t) x Urban population (2,500+)	-	-	-	0.75 (0.04)***	0.57 (0.05)***	0.59 (0.05)***
Broadband effect (2000-2002)	Broadband availability B: γ_I^1	0.60 (0.02)*** [0.66]	0.54 (0.02)*** [0.59]	0.96 (0.02)*** [1.19]	0.48 (0.05)*** [0.52]	0.15 (0.05)*** [0.15]	0.60 (0.04)*** [0.66]
	Broadband availability B x Adjacent to metro areas	-	-	-	0.09 (0.05)* [0.05]	0.29 (0.05)*** [0.15]	0.12 (0.04)*** [0.06]
	Broadband availability B x Urban population (2,500+)	-	-	-	0.11 (0.05)* [0.08]	0.3 (0.06)*** [0.23]	0.4 (0.05)*** [0.30]
Log-likelihood		-198,024.03	-198,740.70	-199,041.18	-197,760.97	-198,502.29	-198,848.60
# of new firms / # of ZIP codes		30,976 / 991					

Note: Broadband availability A(t) denotes broadband availability in t for all new firms in 1990-1992 and 2000-2002. Broadband availability B denotes one-year lagged broadband availability for new firms in 2000-2002. Control variables include education and income of residents in 1990 and 2000, and county and state characteristics dummies. Standard errors are in parentheses. Proportional changes in the probability of firm entry are in the brackets. *** and * indicate significance at the 1% and 10% level, respectively.

Appendix B: Estimation of broadband effect using the negative binomial fixed-effect panel estimation method

We briefly explain a negative binomial fixed-effect panel estimation method. This estimation method is advanced by Hausman et al. (1984). Conditional on the sum of new firms for a ZIP code *j* over the observed years, the probability of receiving a new firm in the ZIP code is:

$$\text{prob}\left(n_{j1},\ldots,n_{jT} \,|\, \sum_t n_{jt}\right) = \left(\prod_t \frac{\Gamma(\gamma_{jt}+n_{jt})}{\Gamma(\gamma_{jt})\Gamma(n_{jt}+1)}\right)\left[\frac{\Gamma(\sum_t \gamma_{jt})\Gamma(\sum_t n_{jt}+1)}{\Gamma(\sum_t \gamma_{jt}+\sum_t n_{jt})}\right]$$

where $\gamma_{jt} = e^{x_{jt}'\beta}$ and $x_{jt}'\beta \equiv \varphi_I I_{jt-1} + Edu_{jt-1} + Inc_{jt-1} + \alpha_j$. I_{jt} is a broadband availability dummy variable. Edu_{jt-1} and Inc_{jt-1} denote education and income levels of residents in ZIP code *j* in year *t*-1, respectively. α_j denotes a ZIP-code dummy variable. n_{jt} denotes the number of new firms in a ZIP code *j* in year *t*. $\Gamma(\cdot)$ is the gamma function, $\Gamma(\cdot) = t^{z-1}e^{-t}$.

Estimation results are reported in Table A2. We use new firm data in 1990-1992 and 2000-2002. In column (1) including ZIP-code dummy variables, we find that an estimated coefficient for broadband is significantly positive; it is 0.63. However, in column (2) including ZIP-code and year dummy variables, we find that an estimated coefficient for broadband is not significant. Note also that the education and income effects shrink in magnitude and may turn insignificant or negative. These results are of questionable validity. Why does that happen? Once we control for ZIP code fixed effects, adding year dummy variables removes ZIP code variation for markets that either consistently had broadband availability or did not have broadband availability all three years (1999-2001). That leaves only ZIP codes that had changing broadband availability over the three years as the source of variation available to identify the broadband effect. That is a minority of the ZIP codes.

Table A2 Negative binomial fixed-effect panel estimation: Effect of broadband on the number of new rural firms in Iowa and North Carolina

Dependent variable: # of new firms in 1990-1992 and 2000-2002	(1)	(2)
Broadband availability one-year lagged to years of new firm entry	0.63 (0.02)***	>-0.01 (0.02)
Education	1.55 (0.28)***	0.21 (0.25)
Income	0.04 (<0.01)***	-0.01 (<0.01)*
ZIP code dummy	Yes	Yes
Year dummy	-	Yes
Log-likelihood	-11,316.84	-9,922.48
# of new firms / # of ZIP codes	63,341 / 1,015	

Note that standard errors are in parentheses. ***: p-value<0.01, *: p-value<0.10.

CHAPTER 3. AGGLOMERATION MATTER EVERYWHERE?: NEW FIRM LOCATION DECISIONS IN RURAL AND URBAN MARKETS

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ABSTRACT

We test whether commonly used measures of agglomeration economies encourage new firm entry in both urban and rural markets. Using new firm location decisions in Iowa and North Carolina, we find that measured agglomeration economies increase the probability of new firm entry in both urban and rural areas. Firms are more likely to locate in markets with an existing cluster of firms in the same industry, with greater concentrations of upstream suppliers or downstream customers, and with a larger proportion of college-educated workers in the local labor supply. Firms are less likely to enter markets with no incumbent firms in the sector or where production is concentrated in relatively few sectors. The same factors encourage both stand-alone start-ups and establishments built by multi-plant firms. Commuting decisions exhibit the same pattern as new firm entry with workers commuting from low to high agglomeration markets. Because agglomeration economies are important for rural firm entry also, policies encouraging new firm entry should focus on relatively few job centers rather than encouraging new firm entry in every small town.

Keywords: *firm entry, specialization, local monopoly, industrial diversity, upstream and downstream firms, education, stand-alone versus expansion start-ups*

1. Introduction

Agglomeration economies are commonly cited as a reason for the growth of cities.[26] New firms are attracted by the possibility of lower production costs or enhanced revenue streams made possible by locating close to customers or other firms. However, studies differ as to the source of these positive externalities.[27] Some agglomeration economies are related to availability of or proximity to production input factors and buyers. Being close to upstream input suppliers or downstream customers lowers transportation costs and may improve information flows between producers and consumers (Jofre-Monseny et al., 2011; Ellison et al., 2010). Large concentrations of educated workers may hasten adaptation of new ideas and lower the costs of labor turnover (Shapiro, 2006; Moretti, 2004). Having a cluster of similar firms may hasten innovation and improve firm productivity because firms can learn from one another or because they can share a larger specialized labor pool (Porter, 2003). Having a highly diversified mixture of firms may improve cross-fertilization of ideas across different firms (Feldman and Audretch, 1999; Glaeser et al., 1992). Local monopoly allows firms to grow much larger which may enable them to exploit the rents from new innovation without risk that the returns will be lost to competitors (Marshall, 1920).

Most previous studies of agglomeration have focused on growth occurring in cities. By definition, rural areas lack agglomeration and so they would seem to lack these productive advantages that benefit firms in cities. Yet rural areas are diverse, and there is a continuum of

[26] For reviews on agglomeration economies, see Duranton and Puga (2014), Puga (2010), Glaeser and Gottlieb (2009), and Rosenthal and Strange (2004).
[27] Duranton and Puga (2004) explain agglomeration economies using three mechanisms: sharing, matching and learning. Firms can receive benefits from sharing facilities and infrastructures, input suppliers and a labor pool. Firms and workers, and buyers and sellers can have better matching in a larger market. New technologies and business practices can be developed and diffused faster in a larger market. The intensity and composition of production activity influences the mechanisms.

sizes along which agglomeration economies may operate. We investigate whether the positive effect of concentrations of economic activity exist in places that are smaller than what we commonly identify as urban. Furthermore, we investigate whether local factors related to agglomeration economies might have different signs or magnitudes of impacts between urban and rural areas.[28] For example, proximity to input suppliers might be more important in rural areas than in urban areas because of the costs of transporting raw materials relative to finished goods (Kilkenny, 1998). Rural firms may be less concerned with the availability of educated workers if they are more concentrated in less-skilled and less technology-oriented industries such as retail or personal services compared to urban firms (Renski and Wallace, 2008).

We use a data set on the location decisions of new firms in Iowa and North Carolina to examine how local agglomeration economies influence firm start-ups, and whether agglomeration economies have different effects on the location choices of rural and urban start-ups. If agglomeration generates productivity or cost advantages to firms in a local market, they should have the greatest influence on firms at the time of entry. Because a new venture can open anywhere in principle, firms will assess the value of local agglomeration and other market factors as they decide where to locate. It is costly to relocate once the entry occurs, and so aggregate growth measures that are heavily weighted toward incumbent firms such as local employment or local output growth will reflect past levels of market factors that may no longer apply.

[28] Rural firms have different characteristics compared to urban firms. See Renski and Wallace (2012) for simple rural-urban comparison of new firms. Differences are found in terms of industries that new firms enter, employment growth within four years after their opening, types of their major clients and education level of owners. Also, see Renski (2008) for a rural-urban comparison of new firm entry, survival and growth. See Yu et al. (2011) for rural-urban comparison of firm longevity.

Another advantage to our use of new firm entry as a measure of market response to agglomeration is that local market factors are pre-determined and outside the control of nascent firms. This reduces the problem of reverse causality between agglomeration measures and more commonly used measures of economic performance such as growth in the numbers of firms or workers. For example, there may be a positive correlation between the number of firms and the proportion of college-educated workers due to the mobility of educated workers rather than spillover productivity effects from a concentration of educated workers. However, the entry of one new firm will not alter the concentration of educated workers in the labor market, but a new firm will enter a market with more college-educated workers if the firm can benefit from the spillover productivity benefits. If educated workers do not have spillover effects, new firms would not react to the current distribution of educated workers.[29]

We investigate the effect of six measures of agglomeration on firm incentives to locate in an urban or rural market: the number of incumbent firms in the industry; the absence of any incumbent firms in the industry; the degree of industrial diversity; proximity to upstream firms; proximity to downstream firms; and the proportion of college educated in the adult population. Our results indicate that the same agglomeration measures that have been associated with urban economic growth are important for rural firm entry as well. The pattern holds for both all firms and manufacturing firms, and for both stand-alone ventures and multi-plant firms looking to expand into new markets. The pattern holds for rural firm entry regardless of whether the area is adjacent to or distant from an urban area. Commuters are attracted to the same agglomeration factors that attract new firm start-ups. As a result, even in rural areas, production would be predicted to concentrate increasingly in relatively

[29] Jofre-Monseny et al. (2011) and Rosenthal and Strange (2003) advanced similar arguments to justify their focus on new firm location decisions.

few locations with workers commuting from the surrounding communities that lack the ability to offer sufficient agglomeration economies.

This paper is organized as follows. In the next section, we review the few studies using a rural-urban comparison to examine agglomeration economies, and discuss different theories in local agglomeration economies. In Sections 3 and 4, we explain our empirical strategy and define our agglomeration measures. In Sections 5 and 6, we provide estimation results and conduct robustness checks of our results using an employment-based analysis. Lastly, we test the role of agglomeration economies in commuting and discuss the policy implications of our results.

2. Literature review

While scholars generally agree on the existence of productive spillovers in cities, they disagree on which local attributes are critical to the existence of the externalities. We focus on six local attributes that have figured prominently in the literature: clusters of similar firms, the ability to exert monopoly power, industrial diversity, proximity to upstream and downstream firms and concentrations of educated workers.

Marshall (1920) argued that there is an advantage for firms in the same industry to locate near one another because workers and firms learn from each other and the learning helps them develop new ideas.[30] Specialization in a given type of production allows firms to share in a pool of workers with similar skills and saves transportation cost of materials. In

[30] "When an industry has thus chosen a locality for itself, it is likely to stay there long: so great are the advantages which people following the same skilled trade get from near neighbourhood to one another. The mysteries of the trade become no mysteries; but are as it were in the air, and children learn many of them unconsciously. Good work is rightly appreciated, inventions and improvements in machinery, in processes and the general organization of the business have their merits promptly discussed: if one man starts a new idea, it is taken up by others and combined with suggestions of their own; and thus it becomes the source of further new ideas. And presently subsidiary trades grow up in the neighbourhood, supplying it with implements and materials, organizing its traffic, and in many ways conducing to the economy of its material." (Marshall, 1920, p.271)

addition, new firms will enter as upstream suppliers of inputs for the cluster of firms in the industry. Marshall also argued that larger firms have advantages over a larger number of small firms because of returns to scale in innovation and because firms can better internalize the returns to their innovation.[31] A single large firm might be most able to capitalize on their innovations, although that would presumably limit the spillovers to other firms.[32] Schumpeter (1942) extended this line of reasoning and argued that firms have no incentive to enter perfectly competitive markets because they will not be able to exploit the full advantages of their innovations.[33]

Porter (1990) followed Marshall in arguing that clusters of similar specialized firms increases the productivity of all the firms through knowledge spillovers. However, he expanded on Marshall to include other firms in the industry as well as upstream suppliers and downstream customers in the cluster. Interaction and information flows across firms within a cluster promote technological innovation.[34] Helsley and Strange (2002) also support importance of various input suppliers. In contrast to Marshall (1920) and Schumpeter (1942), Porter argued that competition helps technological innovation because competitive pressure makes firms adopt and develop new technology to survive.

Jacobs (1969) believed that technology innovation came from industrial diversity rather than industrial specialization. She suggested that seemingly unrelated ideas can

[31] As an example of how larger firms have greater innovation capacity, Marshall states that "[in the pottery trade, a small manufacturer] cannot afford even to make experiments with new patterns and designs except in a very tentative way." (Marshall, 1920, p.281)

[32] "[A multitude of comparatively small rival producers] could not afford to spend as much on improving methods of production and the machinery used in it, as a single large firm which knew that it was certain itself to reap the whole benefit of any advance it made." (Marshall, 1920, p.484) Arrow (1962) and Romer (1986) also view competition as bad for innovation because innovators cannot be compensated for the spillovers from their innovations fully.

[33] "But perfectly free entry into a new field may make it impossible to enter it at all. The introduction of new methods of production and new commodities is hardly conceivable with perfect—and perfectly prompt—competition from the start." (Schumpeter, 1942, pp.104-105)

[34] "Once a cluster forms, the whole group of industries becomes mutually supporting. Benefits flow forward, backward, and horizontally. […] Interconnections within the cluster, often unanticipated, lead to the perception of new ways of competing and entirely new opportunities." (Porter, 1990, p.151)

provide breakthroughs to achieve technological innovation.[35] Following Jacobs, Lucas (1988) also emphasized the role of interactions between people as the rationale for cities. However, Lucas emphasized that it was concentrations of skilled people that were particularly important for the generation of a human capital externality. Productive interactions between educated people make each of them more productive than they would be in isolation. These externalities counteract the natural tendency of cities to fly apart because concentrations of economic activity bid up the costs of land, labor and other factors of production.

The empirical research investigating the effects of agglomeration is mixed. The earliest study to examine the relative strength of these hypotheses empirically was Glaeser et al. (1992). They found that industrial diversity and local competition improve employment growth in cities. Rosenthal and Strange (2003) also found positive impacts of diversity and local competition on new firm entry. In contrast to the employment-based analysis in Glaeser et al., Cingano and Schivardi (2004) found that firm-level productivity was improved by specialization, but not by diversity and local competition.[36] Other studies have found that industrial clusters (specialization) foster more entrepreneurship and lead to higher employment growth (Delgado et al., 2010; Porter, 2003) and higher wages (Wheaton and Lewis, 2002). However, De Blasio and Addario (2005) did not find higher wages in clusters and Duranton (2011) found only modest sized cluster effects. Henderson et al. (1995) found that specialization is important for mature manufacturing industries while industrial diversity is important for high-tech manufacturing industries.

In line with Lucas (1988), Rauch (1993) and Moretti (2004) found empirical evidence that a concentration of college graduates improves productivity of all firms in the market.

[35] "The greater the sheer numbers and varieties of divisions of labor already achieved in an economy, the greater the economy's inherent capacity for adding still more kinds of goods and services." (Jacobs, 1969, p.57)
[36] Their replication of the employment analysis was consistent with Glaeser et al.

The external productivity spillovers are larger when the concentration of educated workers is largest in technologically similar firms, in firms that cite similar patents, and in firms that are upstream suppliers or downstream customers. Winters (2013) and Shapiro (2006) also found that the probability of being employed and local employment growth were higher in areas with relatively large concentrations of skilled workers, respectively. Glaeser and Mare (2001) found that wages grow faster in cities, presumably due to faster human capital accumulation. As for upstream and downstream links, Jofre-Monseny et al. (2011), Ellison et al. (2010) and Glaeser and Kerr (2009) found evidence supporting the importance of upstream and downstream linkages. The estimated magnitude of agglomeration effects on productivity varies widely across studies depending on the context (Melo et al., 2009).

While the bulk of the research on agglomeration effects has focused on cities, there are a number of existing studies that highlight the importance of agglomeration in rural areas. Given that rural firms have different characteristics than urban firms (Renski and Wallace, 2012; Renski, 2008; Yu et al., 2010), they may react to local agglomeration factors differently or not at all. To our knowledge, Gabe (2003) is the only study to compare rural and urban agglomeration economies, although he only studied responses to local specialization rather than a menu of agglomeration measures.

Rural places can experience agglomeration economies in two ways. Because the benefits of agglomeration attenuate with distance (Rosenthal and Strange, 2003 and 2008), remote rural communities would need to experience agglomeration due to sufficient intensity in a sector to affect local economic performance. Research has shown that rural industry clusters increase employment (Henry and Drabenstott, 1996) and local wages (Gibbs and Bernat, 1997). Barkley, Henry and Kim (1999) showed that larger firm clusters in an industry lead to faster employment growth for growing industries in non-metropolitan areas.

Gabe (2005) found that greater specialization in a rural firm's own industry increased its capital investment, but the effect is relatively modest[37].

On the other hand, agglomeration economies can spill over to surrounding areas so that rural areas can access urban agglomeration benefits through commuting (Partridge at al., 2010). For this reason, positive impacts of urban agglomeration are found in rural areas within a commuting distance of cities (Partridge et al., 2007 and 2008; Barkley et al., 1996). In principle, urban agglomeration can have positive or negative impacts on rural areas depending on commuting and migration flows, and the relocation of firms (Renkow and Hoover, 2000; Wu and Gopinath, 2008). For example, if urban residents move to adjacent rural towns and commute to urban areas, rural towns may experience spread: positive economic impacts from urban agglomeration. If instead of commuting, rural residents move to the urban center, agglomeration would result in backwash, causing rural communities to decline.

Establishment location decisions may respond differently to local agglomeration if the plant is part of a multi-plant firm rather than a stand-alone firm. For example, a multi-plant firm may be less sensitive to local upstream suppliers or downstream customers if the plant is part of an integrated but geographically dispersed supply chain. Previous research has found that stand-alone start-ups are more sensitive to local agglomeration factors such as industrial diversity (Glaeser et al., 2010), sectoral firm clusters (Henderson, 2003) and the available supply of educated workers (Barkley and Keith, 1991). None of these studies compared the impact of agglomeration economies on stand-alone versus expansion start-ups across rural and urban locations.

[37] Naturally occurring clusters attract new firm entry presumably because of some underlying local competitive advantage. This is different than trying to create industry clusters as a result of sector-specific government policies. We agree with an anonymous referee that the empirical evidence on the success of government fostering of clusters is decidedly mixed (Feser, 2008; Duranton 2011).

3. Model

We want to examine the possible effects of agglomeration on firm decisions on where to locate. To that end, we sketch an empirically tractable model in which an entrepreneur decides where to locate a new establishment, given information on the nature of the business and its anticipated profitability across all possible locations. To begin, assume that the firm has *C* possible markets in which to locate (*c*=1,…, *C*). We assume these markets follow county boundaries. Each market has up to *K* sectors (*k*=1,…,*K*). Note that the absence of a sector represents a potential entry opportunity for a monopolist.

Price-taking firms maximize their profit in two stages. In the first stage, firm *i* in sector *k* computes its expected profit in each market *c*. Given those computations, the firm chooses the location with maximum profit (π_{ik}^{*}) in the second stage.[38] Expected profit is given by:

$$\pi_{ik}^{*} \equiv Max_{c}\pi(m_{kc}, z_{c}, p_{k}, w_{c}, r_{c}) \qquad (1)$$

The vector of local agglomeration measures, m_{kc}, affects expected profit in county *c* by altering anticipated production costs. The vector of control variables, z_{c}, includes local demand shifters and dummy variables for county characteristics and state. The former influences production quantity and firm profit. The latter reflects common unobservable effects for all firm entrants in a county group and the state in which county *c* is located. Firm profit (π) increases in the sector-specific market price, $p_{k,}$ which we assume is equal across areas, and decreases in local wages (w_c) and capital rents (r_c).

[38] Our assumption of expected profit maximization may not be true. First, people may not compute expected profit correctly, and second, they may be maximizing expected utility, placing a big weight on the location at the expense of the profitability of the firm. However, the reduced form specification we use tests whether the theorized relationships between agglomeration and profit carry over to firm location choices. Our results are consistent with these well-established theories.

We define the vector of local agglomeration measures (m_{kc}) to include measures based on the conceptual and empirical studies reviewed in the previous section:

$$m_{kc} \equiv [CLU_{kc}, MON_{kc}, CON_c, UP_{kc}, DOWN_{kc}, EDU_c] \qquad (2)$$

The variables represent cluster specialization in the sector (CLU_{kc}); the absence of any firms in the sector which would provide a local monopoly opportunity (MON_{kc}); local industrial concentration (CON_c); local access to upstream suppliers (UP_{kc}) and downstream customers ($DOWN_{kc}$) of firms in the sector; and the local availability of college educated workers (EDU_c).

The first attribute corresponds to Marshall's (1920) and Porter's (1990) emphasis that a concentration of firms specialized in a single industry improves productivity through shared knowledge and a shared pool of specialized labor. The second reflects the presumption that a single firm can internalize the local knowledge and innovate at a lower cost than can a large number of small firms, points emphasized by Marshall (1920), Arrow (1962), Romer (1986) and Schumpeter (1942).[39] The third reflects Jacobs (1969) emphasis on a diverse mix of firms in contrast to the preference for specialization emphasized by Marshall and Porter. The fourth and fifth reflect upstream and downstream clusters that are mentioned prominently in Marshall, Porter, and Ellison et al. (2010). The last is related to Lucas's (1988) emphasis on shared pools of high quality labor.

We define the vector of control variables (z_c):

$$z_c \equiv [INC_c, AMENITY_c, TAX_c, \ EXP_{c,} URB_c, R\_ADJ_c, R\_BIG_c, NC_c] \qquad (3)$$

[39] Alternatively, the local monopoly variable may represent negative signal to new firms. In other words, no incumbent firms in the industry in the county may mean that the industry does not have advantage in the county.

where INC_c is median household income in county *c* which indexes local demand for goods and services, $AMENITY_c$ is an index of amenity values, TAX_c is the total tax revenue per capita and EXP_c, is total general direct government expenditures per capita. These amenity and policy-related variables have been considered in empirical studies analyzing local economic growth (e.g. Wu and Gopinath, 2008). We also use a series of dummy variables to indicate county size and state. Using the Rural-Urban Continuum Codes (RUCC) from Economic Research Service, U.S. Department of Agriculture, URB_c indicates the county is considered urban or metropolitan (RUCC = 0-3); R_ADJ_c indicates the county is rural and adjacent to a metropolitan area (RUCC = 4, 6, 8); R_BIG_c indicates the county is rural and has an urban population of 2,500 or more (RUCC = 4-7); and NC_c indicates the county is located in North Carolina. R_ADJ_c addresses the access of rural counties to urban agglomeration economies because agglomeration economies attenuate with distance (Rosenthal and Strange, 2003 and 2008).

We assume a spatial equilibrium where wages and capital costs are adjusted to agglomeration measures (m_{kc}) affecting firm productivity (Rosen, 1979; Roback, 1982). If markets are competitive, firms will expect to make zero economic profits in the long run in all areas. If the agglomeration level m_{kc} changes over time, lower production costs and increase profits in sector *k* and county *c*, market *kc* will attract additional entry relative to other markets. Entering firms will bid up the input prices for labor and capital until expected profits from additional entry are reduced back to zero. That suggests that in the long run equilibrium, wages and rents will be represented as functions of m_{kc}. As Shapiro (2006) shows, both local wages and local rents respond to local amenities and access to productive externalities.

We assume that the firm makes its decision based on the information available on market conditions in the year just prior to entry, so that expected profits are a function of agglomeration measures in year *t-1*. The linear approximation to the reduced-form profit function of firm *i* in industry *k*, county *c* and year *t* is given by:

$$\pi_{ikct} \equiv m'_{kct-1}\gamma_m + z'_{kct-1}\gamma_z + \varepsilon_i + \varepsilon_k + \varepsilon_t + u_{ikct} \qquad (4)$$

where the error terms ε_i and ε_k reflect common unobserved elements that influence profit for firm *i* across all locations and industry *k* profits across all firms. The temporal shock, ε_t, is a common shock to all firm entrants across all locations in year *t*. The last term u_{ikct} is a random shock to profits for firm *i* in county *c* that is assumed to be uncorrelated with all other factors.[40]

Each new firm entrant in year *t* chooses one of the potential *C* areas to enter, based on anticipated profitability. We define the dichotomous variable $E_{ikct} = 1$ if the firm opts to enter area *c* in year *t* and $E_{ikct} = 0$ otherwise. $E_{ikct} = 1$ if $\pi_{ikct} - \pi_{ikc't} \geq 0 \ \forall \ c' \neq c$. From the specification for profit (4), we have

$$E_{ikct} = 1 \ if \ (m'_{kct-1} - m'_{kc\prime t-1})\gamma_m + (z'_{kct-1} - z'_{kc\prime t-1})\gamma_z > \zeta_{ikct} \forall \ \mathrm{c}' \neq c \qquad (5)$$

where $\zeta_{ikct} \equiv u_{ikc't} - u_{ikct}$. Note that the common firm-, sector-, and time-specific economic shocks $\varepsilon_i, \varepsilon_k,$ and ε_t are differenced away because they do not affect relative profitability across markets. That leaves the pure random error term, u_{ikct} which we assume

[40] Our empirical approach has a possible identification issue from unobservable time-varying location-specific factors. For example, local government incentives might encourage firms in the same industry to be concentrated in one location. In this case, our specialization variable would be biased. We test this issue using different years (2003-2004) of new firm entry. We find consistent estimation results, which suggests that our main results are robust to unobservable time-varying location-specific factors. Estimates and elasticities are available upon request.

follows the type-1 extreme distribution. We estimate (5) using the conditional logit estimator.[41]

4. Data

In our application, firms are selecting from C=199 possible county locations because Iowa has 99 counties and North Carolina has 100. Our sample is commercial establishments that entered a county in Iowa and North Carolina over three years (2000-2002). We chose the sample years to align with the availability of Census data. We restrict the sample to firms with a clear profit motive, excluding non-profit organizations, government agencies and firms with a public service emphasis such as museums or historical sites. We also remove firms in agriculture and mining. These firms cannot move freely across locations as their entry decision is affected by site-specific land or resource availability.[42] Our sample has 191,191 establishments; 68% of them are urban start-ups.

Establishment attributes such as location, industry and ownership are obtained from the National Establishment Time Series (NETS).[43] The database provides information on the universe of all establishments that opened for business in Iowa and North Carolina in 2000-2002. The database also includes DUNS (Data Universal Numbering System) numbers of establishments and of their headquarters or parent companies. If an own DUNS number is the same as that of a headquarters or a parent company, the establishment is considered a

[41] We used STATA command "multin" in a package named "groupcl " to estimate the grouped conditional logit. See Guimaraes and Lindrooth (2007) for information on the estimator. (http://www.stata.com/meeting/5nasug/NASUG_Guimaraes.pdf, accessed on June 19, 2014)
[42] The following industries are excluded: Agriculture (2-digit NAICS: 11), Mining (22), Postal Service (3-digit NAICS 491), Monetary Authorities-Central Bank (521), Nursing and Residential Care Facilities (623), Social Assistance (624), Museums, Historical Sites, and Similar Institutions (712), Religious, Grant-making, Civic, Professional and Similar Organizations (813), Private Households (814), and Public Administration (92).
[43] See Kunkle (2011) for advantages of NETS database compared to public data such as Quarterly Census of Employment and Wages (QCEW). (http://exceptionalgrowth.org/insights/NETSvsES-202.pdf, accessed on July.2.2014). Differently from public data based on unemployment insurance filing, NETS data include very small establishments such as sole-proprietors. Excluding establishments having less than 3 employees does not change our main findings. Estimates and elasticities are available upon request.

"stand-alone start-up."[44] Otherwise, it is considered an "expansion start-up." Urban (rural) expansion start-ups are about 8% (6%) of urban (rural) start-ups in the sample period. We use 3-digit and 4-digit North American Industry Classification System (NAICS) codes used in 1997 Standard Use Table from the Bureau of Economic Analysis to define industries. The total number of industries is K=112.

Our agglomeration measures include four that are industry-location-specific (CLU_{kc}, MON_{kc}, UP_{kc} and $DOWN_{kc}$) and two that are location-specific (CON_c and EDU_c). Cluster specialization (CLU_{kc}) is measured as the relative size of the proportion of establishments in industry k in county c to the proportion of establishments in industry k in Iowa and North Carolina:[45]

$$CLU_{kc} = \frac{Establishments\ in\ k, c}{All\ establishments\ in\ c} \Big/ \frac{Establishments\ in\ k\ in\ Iowa\ and\ North\ Carolina}{All\ establishments\ in\ Iowa\ and\ North\ Carolina}$$

The local monopoly index (MON_{kc}) takes a value of 1 if county c has no incumbent firm in industry k, and 0 otherwise. Access to upstream (UP_{kc}) or downstream ($DOWN_{kc}$) firms measures the relative availability of suppliers and customers in industry k in county c. These measures are constructed with data on purchases and sales by industry from the 1997 Standard Use Table, Bureau of Economic Analysis.

$$UP_{kc} \equiv \sum_s \frac{N_{sc}}{N_s} \cdot \frac{Input_{s\to k}}{Input_k} \cdot 100\ \ \forall s \neq k$$

where N_{sc} is the number of establishments in an upstream industry s in county c, and N_s is the number of establishments in industry s in Iowa and North Carolina. The second term is the proportion of input purchases made by firms in industry k from industry s.

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[44] According to personal communication with Walls & Associates who compile the NETS data, the criterion holds except in rare cases.
[45] This is also known as a location quotient.

Similarly, the proximity to downstream firms is

$$DOWN_{kc} \equiv \sum_s \frac{N_{sc}}{N_s} \cdot \frac{Output_{k \to s}}{Output_k} \cdot 100 \ \forall s \neq k$$

where N_{sc} is the number of establishments in an downstream industry *s* in county *c,* and N_s is the number of establishments in industry *s* in Iowa and North Carolina. The second term is the proportion of output purchases made by firms in industry *s* from industry *k.*

Industrial concentration (CON_c) is constructed using wage bill data obtained from the Quarterly Census of Employment, Bureau of Labor Statistics. The data set has ten broad industry categories. Industrial concentration is measured by a Herfindahl- Hirschman Index computed as the sum of squared wage bill shares of industries in a county. It ranges in values from 0 to 1, where values closer to one indicate greater industrial concentration.[46] The human capital concentration (*EDUc*) is measured by the proportion of residents over age 25 with at least a two-year college degree in county *c*. The local demand shifter (INC_c) is measured by median household income in county *c*. Our education and income measures were compiled from the 2000 Census. The amenity index, $AMENITY_c$, was compiled by the the Economic Research Service at USDA. The total tax revenue per capita, TAX_c, and total general direct government expenditures per capita, EXP_c, are compiled from the 1997 Census of Governments.

Summary statistics on key variables are reported in Table 1. Levels of agglomeration differ significantly between rural and urban counties. Rural counties are twice as likely to have a monopoly opportunity in a sector. Urban counties have much greater density of upstream suppliers and downstream customers. Rural counties are more concentrated in relatively few industries and have lower proportions of college educated workers.

[46] Industrial concentration based on output shares would be the most appropriate. Due to data availability, we use wage bill shares instead of output shares.

5. Results

Table 2 illustrates the relative importance of agglomeration versus amenities and fiscal policy on new firm location decisions. The estimation results for the new firm entry (equation (5)) are reported in column (4). All factors are highly significant, due at least in part to the huge sample size. The most immediate way to assess the relative importance of agglomeration measures is to show how the log likelihood changes as these variables are deleted from the full model in column (4) compared to other exclusion restrictions. When the amenities and fiscal policies are removed, the log likelihood decreases by 0.06 percent (or 0.015 per measure), while the log likelihood falls by 2 percent (or 0.33 per measure) when the agglomeration measures are excluded. This suggests that the agglomeration measures are more important for new firm location decisions.

Referring to column (4), new firms are more likely to enter markets with existing firm clusters in the same industry and with better access to upstream and downstream firms and to educated workers. The opportunity for local monopoly lowers the probability of firm entry, as does greater industrial concentration. Firms are also more likely to locate in counties with high per capita incomes, with higher amenity index values, higher government expenditures and lower taxes per capita[47].

Table 3 shows how the agglomeration effects vary by market. The first three columns show the estimates for all industries. Column (1) provides estimates for all firms, replicating results for column (4) in table 2. Column (2) allows the agglomeration effects to vary between rural and urban markets using interaction terms between urban counties (URB_c) and agglomeration measures. Column (3) further distinguishes the agglomeration effects between remote rural counties and rural counties adjacent to urban counties (R_ADJ_c). The direction

[47] Note that counties can raise government expenditures without raising commensurate taxes because of transfers from the federal and state levels.

of the effects of the agglomeration measures is consistent across rural and urban counties, although the magnitudes differ (columns (2) and (3)). In columns (4) to (6), we replicate the analysis limiting the sample to manufacturing firms in order to compare our findings with other studies that focused on manufacturing[48]. The results for manufacturing entrants are consistent with the overall sample.

We gain additional insights by converting these estimates into elasticities. Table 4 shows more clearly the relative magnitudes of the effects across urban, rural adjacent, and remote rural counties. Even though there are significant differences in magnitudes of effects across urban and rural markets, the differences are rarely economically important. Agglomeration measures matter in both markets and they matter similarly. This result is consistent with Gabe (2003) who finds that industry location quotients have positive effects on business openings in metropolitan, and both adjacent and non-adjacent non-metropolitan counties in Maine. In fact, the only large difference is that rural firm entry is much more sensitive to the presence of upstream suppliers, and this is true for both adjacent and remote rural counties.

Among the agglomeration measures, new firm entry is most responsive to access to upstream firms, particularly in rural markets. Proximity to downstream customers also has a significant but smaller positive influence on new firm entry and again the effect is larger in rural markets. Our finding that firms are attracted by close proximity to upstream suppliers and downstream customers was first suggested by Marshall and is consistent with more recent conclusions regarding urban economic growth advanced by Helsley and Strange (2002), Glaeser and Kerr (2009), and Ellison et al. (2010) for urban markets. Skilled labor (EDU_c) has the second largest positive impact on firm entry overall, and is the most

[48] When we limit the sample to manufacturing firms, we exclude three counties from the choice set because they had no manufacturing entrants during the sample period, C=196.

important factor for manufacturing firms, a result consistent with the models of Romer (1986) and Lucas (1988), and with empirical findings reported by Moretti (2004). Consistent with Porter, clusters of local firms in the same industry attract start-ups, while the chance for local monopoly power does not attract firms. Consistent with Jacobs firms are attracted by a more diverse mix of firms. However, the presence of a monopoly deters entry, suggesting that the benefits from captured rents from innovation proposed by Marshall (1920), Arrow (1962), Romer (1986) and Schumpeter (1942) are outweighed by other costs associated with monopoly.[49]

Of the remaining factors, per capita income is the most important for new firm location decisions. The elasticity for government expenditures is only slightly smaller and more than twice the size of the elasticity with respect to per capita tax burden. While the amenity index was statistically significant in table 3, its elasticity is extremely small.

These conclusions carry over to manufacturing firms as well: agglomeration economies have similar effects on new firm entry in urban markets, their adjacent rural neighbors, and their more remote rural counties. One other somewhat large difference is that the presence of a monopoly opportunity in the sector reduces the probability of manufacturing firm entry more in urban than in rural markets. Presumably, the lack of any incumbents in a sector has more information in larger markets because the presence of monopoly opportunities is less common than in smaller markets. It appears that monopoly

[49] We might get different answers if we were focusing on technology or R&D intensive sectors, but most firms in our study will not be producing new products, developing unique production processes, or creating intellectual property.

opportunities are not attractive because they signal a lack of comparative advantage for the sector in that location.[50]

One implication of our findings is that local agglomeration continually adds to the number of firms entering a market. Even though the elasticities are similar between urban and rural markets, the levels of agglomeration are quite different as seen in table 1. While agglomeration matters everywhere, it does not exist everywhere. An urban area whose agglomeration measures are 10 percent over the mean in all the agglomeration measures will have 0.6 percent higher new firm entry; while, a rural area whose agglomeration measures are 10 percent above the rural mean will experience only 0.2 percent higher new firm entry. On the other hand, rural areas that have atypically higher levels of agglomeration do attract new firms; a rural county at one standard deviation above the rural mean will experience 0.5 percent more new firm entry. As a result, places with more concentrated activities will continually attract the greatest share of new firms while places that lack these agglomeration advantages will attract few new entrants[51]. This growth pattern results in a persistent hierarchy of city size as described in central place theory (Christaller, 1933).

Next, we test whether stand-alone start-ups are attracted by different agglomeration factors than expansion start-ups. These results are reported in Table 5. The first three columns show the estimates for expansion start-ups; the next three columns show the

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[50] The proportional change in new firm entry probability for local monopoly is calculated for each observation and averaged across all observations. For each observation, that is calculated as follows:

$$\frac{P(E_{ikc} = 1|MON_{kc} = 1) - P(E_{ikc} = 1|MON_{kc} = 0)}{P(E_{ikc} = 1|MON_{kc} = status\ quo)}$$

where $P(E_{ikc} = 1|MON_{kc} = 1)$ is probability that firm *i* in industry *k* chooses county *c* when there is no incumbent firm in that industry-county. $P(E_{ikc} = 1|MON_{kc} = status\ quo)$ is choice probability when local monopoly is the same as the status quo.

[51] It is important to note that we are not considering congestion effects, which provide a 'check' on increasing levels of concentration. The costs of commuting and other urgan diseconomies limit city size and therefore city growth (Henderson, 2012).

estimates for stand-alone start-ups. The estimated impacts of agglomeration measures on stand-alone start-ups and on expansion start-ups are consistent between urban and rural areas. The one exception is that industrial concentration (CON_c) does not appear to matter for expansion start-ups in rural areas, but does matter in urban areas. Panel (b) in Table 4 reports the elasticities for each firm type. Measured agglomeration effects on new firm entry are similar for expansion and stand-alone start-ups in urban, rural adjacent and remote rural counties.

Our finding that agglomeration measures have similar impacts on firm entry in rural counties adjacent to or remote from a metropolitan area may seem surprising in that adjacency to urban areas has been found to be critical in the rural development literature.[52] The explanation is that agglomeration matters everywhere, but agglomeration is not present everywhere. Areas adjacent to metropolitan areas are more agglomerated than more remote rural areas and that explains their growth advantage. Proximity to an urban center undoubtedly has advantages for growth (Partridge et al., 2008) which leads to the accumulation of agglomeration measures in adjacent counties that induce additional growth.

6. Local agglomeration measures and urban and rural growth using aggregate data

As a robustness check, we examine the impact of county-level agglomeration measures on employment growth. We measure our agglomeration factors in the year 2000 and test how these factors affect local county-by-industry employment 10 years later, in 2010. The unit of observation is the industry-county pair. We have 21,890 industry-county pairs across the two states.[53] Due to many industry-county pairs having no positive local industry

[52] For extensive discussion on rural-urban interdependency for rural development, see Castle et al. (2011), Olfert and Partridge (2010), Irwin et al. (2009) and Partridge et al. (2008).
[53] There are 22,288 potential industry-county pairs with 112 industries and 199 counties. We excluded two industries or 398 industry-county pairs because they did not have any new firm entrants in 2001 and so those sectors would not have been included in our previous analysis.

employment in either 2000 or 2010, our dependent variable is a dummy variable which has a value of 1 if employment in the industry-county pair increased between 2000 and 2010, and 0 otherwise. We estimate a binary logit model with the same set of explanatory variables as in our previous analysis[54]. Hence, this is a net employment growth measure where employment growth can occur from both new and incumbent firms.

We find that the agglomeration measures have similar signs and magnitudes of elasticities between urban and rural areas and regardless of the adjacency to urban areas, which is consistent with our previous analysis. As shown in column 2 of Table 6, the estimated coefficients have the same signs in urban and rural markets. Only two of the coefficients differ significantly between urban and rural markets. The presence of sectoral clusters lower employment in both markets, with the larger effect in urban areas. Monopoly opportunity retains its negative effect on employment growth but the effect is now larger in rural areas. None of the coefficients differ between adjacent and remote rural counties.

The elasticities by market type are reported to the right of the estimates in Table 5. The main differences between these results based on the aggregated data on net employment growth and our previous results based on firm entry are the much smaller elasticities and the switched signs on cluster specialization (CLU_{kc}). There may be a negative relationship between productivity and employment. As pointed out in Cingano and Schivardi (2004), if the price elasticity of demand is inelastic in an industry, productivity improvements can reduce revenue and labor demand. However, the simple correlation between output per worker and aggregate hours employed from 1947-2013 is 0.98, using the U.S. Bureau of

[54] When we use changes in employment numbers, we obtain similar results, except that the MON_{kc} measure becomes large and positive. This result is definitional; any entry into a county that previously had no incumbent firms will increase employment in that sector. The negative effect of monopoly is suppressed because you cannot lower employment from zero.

Labor Statistics' Major Sector Productivity Series for the nonfarm business sector, so the empirical basis for the argument is suspect.

A more compelling argument for the smaller elasticities in Table 6 compared to Table 4 is that at the market level of aggregation, current employment is a primarily the aggregation of past employment decisions made by firms that entered the market under past market conditions and agglomeration levels. Costs of relocation are prohibitive, and so firms may not be in their best location given current market conditions. Consequently, the link between agglomeration measures and the growth of incumbent firms must be weaker than the link between agglomeration factors and the location decisions of entering firms.

More importantly, our conditional logit specification estimating equation (5) controls for the unobserved firm profit component, ε_i, while the estimation using the county aggregate net sectoral employment data does not. These unobserved firm fixed effects are likely to affect the pace of future firm investment, employment and production decisions, so they will create unobserved location-specific profitability and productivity factors that are almost certainly correlated with the observed agglomeration measures. Hence, the coefficients and the elasticities in Table 6 are biased and will not yield reliable inferences regarding the role of agglomeration on county-sector growth.

7. Agglomeration economies and commuting patterns

As a second robustness check, we examine the linkage between agglomeration economies and commuting patterns. In theory, agglomeration will raise location-specific productivity, some of which will be captured by workers in the form of higher wages. That will create an incentive for workers in low agglomeration locations to move to or commute to locations with greater agglomeration levels. Importantly, the cost of switching job locations within a 2-3 county radius is relatively low compared to the potential returns and so commuting decisions can respond more elastically to changing market conditions than can firms.

There is evidence suggesting that commuting is sensitive to local job growth. Renkow (2003) found that a substantial portion (one-third to one-half) of new jobs is taken by in-commuters. Khan et al. (2001) found that economic growth in one community positively interacts with growth in nearby communities, possibly through commuting. Because we show that agglomeration increases new firm entry, we can conjecture that commuting is sensitive to agglomeration economies as well. However, testing this hypothesis has been limited due to the difficulty in measuring magnitudes of local agglomeration economies. Recent commuting studies measured the spillover of urban agglomeration economies to rural towns using the distance between urban centers and rural towns (Ali et al., 2011; Partridge et al., 2010), but this measure does not capture variations in agglomeration economies within rural or urban areas.

To test the importance of these agglomeration economies on commuting patterns, we consider a commuting destination decision of workers. Suppose that a worker i living in a home county h (h=1,…,H) chooses a county d for work (d=1,…,D). D counties are located within commuting distance which we set arbitrarily at 100 miles from her home county. The choice set of potential counties of work varies with the location of each county of residence.[55]

We assume a latent utility function of worker i living in a home county h and working in a county d below. As in our analysis of new firm entry, we assume a spatial equilibrium model where wages and rents are functions of local factors affecting firm productivity (Rosen,

[55] In our commuting data, 94% of workers commuting within a state (Iowa or North Carolina) commute to a county within 100 miles from their home county. We do not allow commuting across states because of data complications. Both North Carolina and Iowa have metropolitan areas that cross state lines, making it difficult to distinguish agglomeration levels across counties with in the same metropolitan area.

1979; Roback, 1982). Workers choose the county for work that gives them the highest utility conditional on residing county *h*:

$$U_{ihd} = \beta_A \frac{AE_d}{AE_h} + \beta_D DIST_{hd} + \beta_U URB_d + \beta_F \frac{FIRM_d}{FIRM_h} + \varepsilon_h + u_{ihd} \qquad (6)$$

The explanatory variables are lagged and pre-determined at the time of the commuting destination decision. For notational convenience, we omit a subscript for time period.

The first term is the ratio of the agglomeration economy at the destination county relative to the county of residence. Based on (5), we estimate the agglomeration index for each county by

$$AE_{ikc} \equiv \frac{\exp(m'_{kc} \cdot \gamma_m + m'_{kc} \cdot URB_c \cdot \gamma_{mu} + m'_{kc} \cdot R\_ADJ_c \cdot \gamma_{ma})}{\sum_c \exp(m'_{kc} \cdot \gamma_m + m'_{kc} \cdot URB_c \cdot \gamma_{mu} + m'_{kc} \cdot R\_ADJ_c \cdot \gamma_{ma} + z'_{kc} \cdot \gamma_z)}$$

where the coefficients are taken from Table 2. This index is the contribution of the six agglomeration economy measures to the probability of new-firm entry in county *c*, relative to the probability of entry across all counties. We take the average across all firms to obtain the unconditional estimate of AE_c. Note that the ratio will be greater than one when a county within 100 miles has a larger level of agglomeration than the home county. When $h=d$, meaning the worker lives and works in the home county, the ratio is 1. If agglomeration raises productivity, they should raise wages as well, and so a county *d* with a high level of agglomeration should attract more commuters from neighboring counties.

Commuting distance from a home county *h* to a county *d* ($DIST_{hd}$) raises the cost of commuting, and so probability of commuting should decrease as distance increases. URB_d indicates whether the commuting destination *d* is urban, and captures unobservable local factors other than our observed agglomeration measures that may influence commuting. $FIRM_d$ /$FIRM_h$ is the proportion of existing firms in county *d* relative to in county *h*. We add

$FIRM_d$ /$FIRM_h$ to take out the effect of relative county size so that the relationship between agglomeration measures and commuting patterns is not just a reflection of county size. The error term, $\varepsilon_{h,}$ represents home-county-specific unobservable impacts, and is differenced away in the conditional logit estimation because it does not affect the relative utilities across counties. We assume that u_{ihd} is a random error not correlated with the explanatory variables. We estimate the utility function using the conditional logit estimation.[56] This framework is similar to the population migration study of Davies et al. (2001).

We obtain commuting patterns in Iowa and North Carolina from the 2006-2010 American Community Survey County-to-County Worker Flow File, U.S. Census Bureau. Distances between the geographical centroids of two counties were compiled using the County-to-County Distance Matrix at the Center for Transportation Analysis, Oak Ridge National Laboratory. Rural-urban classification is the same as in the previous new-firm entry analysis. The number of existing firms is aggregated from the firm level data in the National Establishment Time Series (NETS) in year 2000.

Results are reported in Table 7 with elasticities in brackets. Consistent with the analysis of new firm entry, there is a very strong positive relationship between our estimate of the effect of agglomeration measures on firm entry and the probability of in-commuting. Thus, the same agglomeration factors that encourage firm entry also atypically attract workers from other counties. The effect remains strong even when we control for the relative number of firms and the urban designation of the county. The literal interpretation is that a 10% increase in the probability of new firm entry in a county attributable to growth in agglomeration results in a 2% increase in the probability of in-commuting from a county

[56] We used the grouped conditional logit with a STATA command "multin."

within a 100 mile radius. Meanwhile, distance has a strong negative effect on the probability of commuting, whether from a rural or urban county of residence.

The adverse impact of distance on commuting is of like magnitude for urban and rural residences. However, the attraction of greater agglomeration in a neighboring county is 3 times larger (0.47 versus 0.15) for rural county residents compared to urban county residents. As a result, a given incremental return in relative agglomeration will attract rural in-commuters from a greater distance than for urban in-commuters.

8. Discussion: Integration between small and large rural towns

We have shown that the same agglomeration measures that have been associated with urban economic growth are important for rural economic growth as well. The pattern holds for both stand-alone ventures and for multi-plant firms looking to expand into new markets. The relative sizes of the elasticities of the agglomeration measures are consistent between urban and rural areas regardless of industry classification (all industries vs. manufacturing) and establishment ownership (stand-alone vs. expansion). These findings of similar agglomeration elasiticites across rural and urban markets are consistent with the recent results reported by Glaeser and Gottlieb (2008) across metropolitan regions and Kline and Moretti (2014) in the Tennessee Valley Authority. Commuters are attracted to the same agglomeration factors that attract new firm start-ups.

Our results have two useful implications to theories in local agglomeration economies. First, our results can test several theories related to agglomeration economies for rural areas similar to Glaeser (1992) et al. did for urban areas. Our results imply that the most attractive areas for new firm entry are diverse economies with proximate upstream and downstream firms, an available educated workforce and a ready number of firms already in the sector. Our results support the various theories of local economic growth including Jacobs' (1969) importance of diversity, Porter's (1990) emphasis on clusters, Lucas's (1988) and Romer's (1986) endogenous growth based on concentrations of an educated workforce and Marshall's (1920) theory of proximate upstream and downstream firms. But we find no support for Marshall's and Schumpeter's (1942) view that monopoly opportunity invites entry in order to exploit first mover rents; in contrast, more competitive markets seem to induce firm entry.

Second, our results provide a dynamic context for Central Place Theory (CPT) initiated by Christaller (1933). One of the key components in CPT is a persistent urban hierarchy; larger cities have more industries because higher-ordered industries require higher demand thresholds. As mentioned in Mulligan et al. (2012), CPT does not have a sufficient dynamic explanation of how urban hierarchy persists. Our finding that agglomeration attracts new firm entry relative to less agglomerated markets means that the most agglomerated will get the largest annual infusion of new firms. Consequently, agglomeration economies are a key reason that the urban hierarchy persists.

Based on our findings, we expect that rural communities with higher agglomeration economies will serve as regional centers of economic activity with their surrounding communities providing labor and customers who commute in. Glaeser and Gottlieb (2008) and Kline and Moretti (2014) argue that the finding of common agglomeration elasticities across areas cannot justify place-based government intervention. Our results also suggest that there is no additional advantage to agglomeration in urban areas relative to rural areas that have higher agglomeration endowments. However, rural communities that do not have a critical mass of firm clusters, upstream suppliers, downstream customers and educated labor will find it difficult to attract new start-ups. Policies aimed at encouraging new firm entry in dispersed small towns that lack agglomeration externalities would be much more expensive and will likely fail (Barkley and Henry, 1997). In addition, recruiting new firms that do not exist in the rural town would require additional costs because no incumbent firms in the industry would be a signal that the industry has no advantages in the town. The more promising strategy is to target new firm recruitment to the relatively few rural markets that have managed to foster sufficient agglomeration economies that they can attract both firms and commuting workers.[57]

Our findings call into question the efficacy of rural development policies that aim to improve economic growth in all rural towns. A "greatest potential" type approach that targets rural towns with growth potential, as evidenced by the existence of agglomeration economies, and promotes them as local job centers would be economically promising. While that means many small rural towns will continue to experience population loss, overall, a more targeted policy may stabilize regional rural population.

[57] Our approach can be compared with Kilkenny and Johnson (2007) and Olfert and Partridge (2010). Kilkenny and Johnson argued in favor of community-level adjustments to make fewer, larger and more prosperous rural communities. While their goal is similar to ours, their approach is different. They suggest merging small rural towns jurisdictionally or physically while we suggest integration between small rural communities and rural regional job centers, possibly through better roads and improved telecommunications. Olfert and Partridge emphasized the integration between rural towns and urban centers to take advantage of the spillover of urban agglomeration economies to rural towns. Their approach focuses on in-migration from urban areas and out-commuting to urban areas, while we focus on job creation in large rural towns with in-commuting from surrounding smaller rural communities.

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Table 1 Summary statistics of key variables in Iowa and North Carolina

	Urban county				Rural county			
	Mean	Std. Dev.	Min.	Max.	Mean	Std. Dev.	Min.	Max.
CLU_{kc}: Location quotient in *k*, *c*	1.05	1.05	0.00	16.43	0.77	1.39	0.00	36.15
MON_{kc}: No incumbent in *k*	0.17	0.37	0.00	1.00	0.40	0.49	0.00	1.00
UP_{kc}: Proximity to upstream firms	1.27	1.79	0.02	15.93	0.21	0.24	0.00	7.24
$DOWN_{kc}$: Proximity to downstream firms	1.11	1.51	0.00	15.76	0.21	0.26	0.00	8.66
CON_c: Herfindahl Index in county *c*	0.22	0.07	0.14	0.50	0.24	0.08	0.14	0.58
EDU_c: Share of population with 14+ years of schooling	0.28	0.10	0.13	0.57	0.22	0.05	0.14	0.52
INC_c: Median household income in *c*	40.36	5.20	30.98	54.99	34.47	4.20	25.18	42.43
$AMENITY_c$: Natural amenity scale in *c*	-0.45	1.41	-3.07	2.18	-1.31	2.02	-4.86	3.33
TAX_c: Taxes per capita	1.09	0.63	0.5	3.63	0.87	0.31	0.42	2.37
EXP_c: Government expenditure per capita	2.06	0.66	0.71	3.98	2.33	0.61	1.17	4.36
# of counties		45				154		

The first six variables are as of 2000. INC_c is as of 1999; TAX_c and EXP_c are 1997 values. Rural counties are statistically different from urban counties in terms of EXP_c and the other variables at the 5% and 1% significance level in the two-sample z-test, respectively.

Table 2 Location choice of new firms in Iowa and North Carolina counties in 2000-2002

Dependent var.: choice of county	(1)		(2)		(3)	
CLU_{kc}	-		0.28	(<0.01)***	0.28	(<0.01)***
MON_{kc}	-		-0.65	(0.03)***	-0.64	(0.03)***
UP_{kc}	-		0.15	(<0.01)***	0.13	(<0.01)***
$DOWN_{kc}$	-		0.05	(<0.01)***	0.05	(<0.01)***
CON_{c}	-		-1.31	(0.04)***	-1.23	(0.04)***
EDU_{c}	-		2.31	(0.03)***	2.40	(0.03)***
INC_{c}	116.37	(0.41)***	18.58	(0.65)***	25.84	(0.71)***
Urban county	1.00	(0.01)***	1.24	(0.01)***	1.27	(0.01)***
Rural county adjacent to urban county	0.09	(0.01)***	0.41	(0.01)***	0.42	(0.01)***
Rural county with 20,000+ population	-		-		-	
North Carolina	0.65	(0.01)***	0.23	(0.01)***	0.38	(0.01)***
$AMENITY_{c}$	-		-		0.02	(<0.01)***
TAX_{c}	-		-		-0.22	(0.01)***
EXP_{c}	-		-		0.27	(0.01)***
Log likelihood	-885,440.67		-856,743.59		-856,190.59	
Likelihood ratio test between (1) and (2)		p-value<0.01				
Likelihood ratio test between (2) and (3)				p-value<0.01		
# of est. / # of counties			191,191 / 199			

Notes: Estimates are based on the conditional logit estimation. Standard errors are in parentheses. ***: significant at 1%, **: significant at 5%, *: significant at 10%. A dummy variable for rural county with 20,000+ population is excluded to avoid non-convergence in estimation.

Table 3 Location choice of new firms in Iowa and North Carolina counties in 2000-2002

Dependent var.: choice of county	All industries						Manufacturing					
	(1)		(2)		(3)		(4)		(5)		(6)	
CLU_{kc}	0.28	(<0.01)***	0.23	(<0.01)***	0.20	(0.01)***	0.18	(0.01)***	0.14	(0.01)***	0.11	(0.01)***
MON_{kc}	-0.64	(0.03)***	-0.38	(0.04)***	-0.40	(0.05)***	-0.42	(0.05)***	-0.29	(0.06)***	-0.25	(0.08)***
UP_{kc}	0.13	(<0.01)***	1.30	(0.02)***	1.65	(0.04)***	0.08	(0.01)***	0.77	(0.05)***	0.58	(0.10)***
$DOWN_{kc}$	0.05	(<0.01)***	0.29	(0.01)***	0.51	(0.02)***	0.20	(0.01)***	0.34	(0.03)***	0.91	(0.10)***
CON_{c}	-1.23	(0.04)***	-0.32	(0.06)***	-0.66	(0.10)***	-1.42	(0.22)***	-0.68	(0.3)**	-1.03	(0.51)**
EDU_{c}	2.40	(0.03)***	1.48	(0.07)***	2.16	(0.14)***	1.92	(0.18)***	2.60	(0.35)***	3.28	(0.67)***
URB_{c} x CLU_{kc}	-		0.15	(0.01)***	0.18	(0.01)***	-		0.10	(0.01)***	0.13	(0.02)***
URB_{c} x MON_{kc}	-		-0.05	(0.09)	-0.02	(0.09)	-		-0.20	(0.11)*	-0.22	(0.13)*
URB_{c} x UP_{kc}	-		-1.15	(0.02)***	-1.49	(0.04)***	-		-0.68	(0.05)***	-0.49	(0.10)***
URB_{c} x $DOWN_{kc}$	-		-0.25	(0.01)***	-0.46	(0.02)***	-		-0.15	(0.03)***	-0.72	(0.10)***
URB_{c} x CON_{c}	-		-2.49	(0.09)***	-2.11	(0.13)***	-		-1.65	(0.45)***	-1.24	(0.62)**
URB_{c} x EDU_{c}	-		0.40	(0.08)***	-0.25	(0.14)*	-		-0.87	(0.37)**	-1.50	(0.68)**
R_ADJ_{c} x CLU_{kc}	-		-		0.07	(0.01)***	-		-		0.06	(0.02)***
R_ADJ_{c} x MON_{kc}	-		-		0.07	(0.07)	-		-		-0.01	(0.12)
R_ADJ_{c} x UP_{kc}	-		-		-0.37	(0.04)***	-		-		0.27	(0.11)**
R_ADJ_{c} x $DOWN_{kc}$	-		-		-0.24	(0.02)***	-		-		-0.61	(0.11)***
R_ADJ_{c} x CON_{c}	-		-		0.63	(0.13)***	-		-		0.64	(0.62)
R_ADJ_{c} x EDU_{c}	-		-		-1.08	(0.16)***	-		-		-1.01	(0.76)
INC_{c}	25.84	(0.71)***	17.57	(0.72)***	17.56	(0.72)***	18.16	(3.48)***	18.85	(3.51)***	18.42	(3.51)***
$AMENITY_{c}$	0.02	(<0.01)***	0.02	(<0.01)***	0.02	(<0.01)***	0.01	(0.01)	<0.01	(0.01)	>-0.01	(0.01)
TAX_{c}	-0.22	(0.01)***	-0.23	(0.01)***	-0.27	(0.01)***	-0.15	(0.05)***	-0.18	(0.05)***	-0.22	(0.05)***
EXP_{c}	0.27	(0.01)***	0.24	(0.01)***	0.27	(0.01)***	0.14	(0.05)***	0.20	(0.05)***	0.23	(0.05)***
Log likelihood	856,190.59		849,369.14		849,070.34		-34,300.41		-34,146.84		-34,124.25	
# of est. / # of counties			191,191 / 199						7,774 / 196			

Notes: Estimates are based on the conditional logit estimation. Standard errors are in parentheses. ***: significant at 1%, **: significant at 5%, *: significant at 10%. Estimates for control variables (URB_{c}, R_ADJ_{c}, R_BIG_{c}, and NC_{c}) are not reported.

Table 4 Elasticities of agglomeration measures on probability of new firm entry

(a) By all industries and manufacturing

	All industries					Manufacturing				
	Urban	Rural	Urban	Rural: Adjacent to urban	Rural: Remote from urban	Urban	Rural	Urban	Rural: Adjacent to urban	Rural: Remote from urban
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
CLU_{kc}	0.23	0.19	0.21	0.20	0.17	0.15	0.12	0.13	0.12	0.10
MON_{kc}	{-0.37}	{-0.32}	{-0.35}	{-0.33}	{-0.33}	{-0.45}	{-0.27}	{-0.44}	{-0.24}	{-0.24}
UP_{kc}	0.25	0.58	0.31	0.70	0.74	0.15	0.33	0.12	0.28	0.25
$DOWN_{kc}$	0.06	0.12	0.09	0.18	0.20	0.10	0.13	0.18	0.30	0.35
CON_c	-0.20	-0.07	-0.26	-0.10	-0.15	-0.24	-0.16	-0.30	-0.19	-0.24
EDU_c	0.37	0.34	0.48	0.41	0.50	0.54	0.60	0.66	0.68	0.75
INC_c	0.63			0.63		0.67			0.66	
$AMENITY_c$	0.02			0.02		<0.01			>-0.01	
TAX_c	-0.21			-0.25		-0.17			-0.21	
EXP_c	0.53			0.61		0.45			0.52	

Notes: Elasticities based on conditional logit results reported in Table 2. The results reported in brackets for monopoly opportunity MON_{kc} are proportional changes in the probability of firm entry going from the absence of a monopoly to the presence of a monopoly in the county-sector market.

Table 4 continued

(b) By establishment ownership

	Expansion start-up					Standalone start-up				
	Urban	Rural	Urban	Rural: Adjacent to urban	Rural: Remote from urban	Urban	Rural	Urban	Rural: Adjacent to urban	Rural: Remote from urban
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
CLU_{kc}	0.20	0.17	0.19	0.17	0.16	0.23	0.20	0.21	0.19	0.17
MON_{kc}	{-0.53}	{-0.49}	{-0.60}	{-0.32}	{-0.32}	{-0.31}	{-0.28}	{-0.29}	{-0.35}	{-0.35}
UP_{kc}	0.16	0.34	0.20	0.42	0.44	0.28	0.65	0.37	0.86	0.90
$DOWN_{kc}$	0.11	0.21	0.16	0.31	0.34	0.05	0.09	0.08	0.17	0.19
CON_c	-0.18	-0.03	-0.43	-0.19	-0.34	-0.19	-0.07	-0.20	-0.04	-0.09
EDU_c	0.42	0.41	0.30	0.33	0.24	0.35	0.32	0.54	0.49	0.60
INC_c	0.82		0.32			0.59		0.44		
$AMENITY_c$	0.01		0.01			0.02		0.01		
TAX_c	-0.26		-			-0.21		-		
EXP_c	0.91		-			0.52		-		

Notes: Elasticities based on conditional logit results reported in Table 4. The results reported in brackets for monopoly opportunity MON_{kc} are proportional changes in the probability of firm entry going from the absence of a monopoly to the presence of a monopoly in the county-sector market.

Table 5 Location choice of new firms by establishment ownership: Iowa and North Carolina counties in 2000-2002

Dependent var.: choice of county	Expansion start-up (1)		(2)		(3)		Standalone start-up (4)		(5)		(6)	
CLU_{kc}	0.25	(0.01)***	0.20	(0.01)***	0.18	(0.02)***	0.29	(<0.01)***	0.23	(0.01)***	0.19	(0.01)***
MON_{kc}	-0.75	(0.10)***	-0.62	(0.11)***	-0.38	(0.14)***	-0.59	(0.04)***	-0.33	(0.04)***	-0.43	(0.05)***
UP_{kc}	0.11	(0.01)***	0.79	(0.04)***	1.01	(0.08)***	0.14	(<0.01)***	1.45	(0.02)***	2.01	(0.04)***
$DOWN_{kc}$	0.11	(0.01)***	0.52	(0.03)***	0.84	(0.08)***	0.05	(<0.01)***	0.24	(0.01)***	0.48	(0.02)***
CON_c	-1.60	(0.17)***	-0.12	(0.23)	-1.45	(0.43)***	-1.22	(0.05)***	-0.30	(0.06)***	-0.38	(0.11)***
EDU_c	2.23	(0.13)***	1.76	(0.28)***	1.05	(0.54)*	2.39	(0.04)***	1.37	(0.07)***	2.61	(0.14)***
URB_c x CLU_{kc}	-		0.14	(0.02)***	0.16	(0.02)***	-		0.15	(0.01)***	0.19	(0.01)***
URB_c x MON_{kc}	-		-0.05	(0.21)	-0.33	(0.23)	-		-0.03	(0.10)	0.07	(0.10)
URB_c x UP_{kc}	-		-0.67	(0.04)***	-0.87	(0.08)***	-		-1.29	(0.02)***	-1.84	(0.04)***
URB_c x $DOWN_{kc}$	-		-0.42	(0.03)***	-0.74	(0.08)***	-		-0.20	(0.01)***	-0.44	(0.02)***
URB_c x CON_c	-		-3.04	(0.35)***	-1.74	(0.51)***	-		-2.45	(0.10)***	-2.32	(0.13)***
URB_c x EDU_c	-		0.29	(0.30)	0.91	(0.55)*	-		0.51	(0.08)***	-0.90	(0.15)***
R_ADJ_c x CLU_{kc}	-		-		0.05	(0.03)*	-		-		0.08	(0.01)***
R_ADJ_c x MON_{kc}	-		-		-0.51	(0.23)**	-		-		0.10	(0.07)
R_ADJ_c x UP_{kc}	-		-		-0.23	(0.09)**	-		-		-0.42	(0.04)***
R_ADJ_c x $DOWN_{kc}$	-		-		-0.39	(0.08)***	-		-		-0.27	(0.03)***
R_ADJ_c x CON_c	-		-		1.67	(0.51)***	-		-		0.59	(0.13)***
R_ADJ_c x EDU_c	-		-		1.09	(0.61)*	-		-		-1.34	(0.16)***
INC_c	26.88	(2.68)***	23.03	(2.75)***	9.06	(2.48)***	25.53	(0.73)***	16.65	(0.75)***	12.28	(0.69)***
$AMENITY_c$	0.01	(0.01)	0.01	(0.01)	0.01	(0.01)	0.02	(<0.01)***	0.02	(<0.01)***	<0.01	(<0.01)**
TAX_c	-0.21	(0.04)***	-0.28	(0.04)***	-		-0.22	(0.01)***	-0.23	(0.01)***	-	
EXP_c	0.33	(0.03)***	0.41	(0.03)***	-		0.26	(0.01)***	0.23	(0.01)***	-	
Log likelihood	-59,646.70		-59,287.58		-59,351.09		-796,135.48		-789,658.68		-790,526.52	
# of est. / # of counties			14,115 / 199						177,076 / 199			

Notes: Standard errors are in parentheses. ***: significant at 1%, **: significant at 5%, *: significant at 10%. Estimates for control variables (URB_c, R_ADJ_c, R_BIG_c, and NC_c) are not reported. TAX_c and EXP_c are excluded in columns (3) and (6) due to non-convergence in estimation.

Table 6 Robustness check: probability of sectoral employment growth in Iowa and North Carolina counties in 2000-2010

Dependent var.: whether or not employment in industry *k* in a county *c* increases between 2000 and 2010	(1)			(2)			(3)		
CLU_{kc}	-0.39	(0.02)***	[-0.16]	-0.35	(0.02)***	[-0.15]	-0.32	(0.03)***	[-0.16]
MON_{kc}	-1.61	(0.04)***	{-0.73}	-1.66	(0.04)***	{-0.74}	-1.69	(0.06)***	{-1.04}
UP_{kc}	-0.03	(0.03)	[-0.01]	0.04	(0.08)	[0.01]	0.06	(0.12)	[0.01]
$DOWN_{kc}$	-0.01<	(0.03)	[>-0.01]	0.04	(0.07)	[0.01]	0.05	(0.12)	[0.01]
CON_c	-0.51	(0.21)**	[-0.06]	-0.38	(0.23)*	[-0.04]	-0.56	(0.36)	[-0.08]
EDU_c	0.48	(0.29)*	[0.05]	0.52	(0.41)	[0.05]	0.40	(0.61)	[0.05]
URB_c x CLU_{kc}	-			-0.17	(0.05)***	[-0.02]	-0.20	(0.05)***	[-0.03]
URB_c x MON_{kc}	-			0.40	(0.11)***	[0.03]	0.44	(0.11)***	[0.05]
URB_c x UP_{kc}	-			-0.06	(0.09)	[>-0.01]	-0.08	(0.13)	[-0.01]
URB_c x $DOWN_{kc}$	-			-0.03	(0.08)	[>-0.01]	-0.04	(0.13)	[>-0.01]
URB_c x CON_c	-			-0.59	(0.55)	[>-0.01]	-0.40	(0.62)	[-0.01]
URB_c x EDU_c	-			-0.24	(0.52)	[>-0.01]	-0.12	(0.69)	[>-0.01]
R_ADJ_c x CLU_{kc}	-			-			-0.07	(0.04)	[-0.01]
R_ADJ_c x MON_{kc}	-			-			0.07	(0.09)	[0.01]
R_ADJ_c x UP_{kc}	-			-			-0.02	(0.16)	[>-0.01]
R_ADJ_c x $DOWN_{kc}$	-			-			0.00	(0.15)	[-1.04]
R_ADJ_c x CON_c	-			-			0.34	(0.46)	[0.05]
R_ADJ_c x EDU_c	-			-			0.18	(0.74)	[0.02]
INC_c	26.82	(4.77)***	[0.44]	26.25	(4.89)***	[0.43]	26.13	(4.90)***	[0.54]
$AMENITY_c$	0.03	(0.01)***	[0.02]	0.03	(0.01)**	[0.02]	0.03	(0.01)**	[0.02]
TAX_c	-0.07	(0.06)	[-0.03]	-0.08	(0.06)	[-0.03]	-0.08	(0.06)	[-0.04]
EXP_c	0.03	(0.04)	[0.04]	0.04	(0.04)	[0.05]	0.05	(0.04)	[0.06]
Log likelihood	-13,658.98			-13,628.55			-13,625.03		
# of industry-county pairs	21,890			21,890			21,890		

Estimates from the binary logit estimation are reported. Standard errors are in parentheses. ***: significant at 1%, **: significant at 5%, *: significant at 10%. Elasiticities or proportional changes in choice probability are reported in brackets. Estimates for control variables (URB_c, R_ADJ_c, R_BIG_c, and NC_c) are not reported.

Table 7 Effects of agglomeration economies on commuting in 2006-2010

Dependent var.: Choice of county for work	All workers		Workers living in urban counties		Workers living in rural counties	
	(1)	(2)	(3)	(4)	(5)	(6)
AE_d/AE_h	0.29 (<0.01)*** [0.29]	0.22 (<0.01)*** [0.22]	0.27 (<0.01)*** [0.28]	0.15 (<0.01)*** [0.15]	0.62 (<0.01)*** [0.56]	0.52 (<0.01)*** [0.47]
Distance	-0.12 (<0.01)*** [-7.66]	-0.13 (<0.01)*** [-7.78]	-0.12 (<0.01)*** [-7.62]	-0.13 (<0.01)*** [-7.85]	-0.13 (<0.01)*** [-7.84]	-0.13 (<0.01)*** [-7.85]
Urban (=1)	1.32 (<0.01)*** {1.99}	1.09 (<0.01)*** {1.49}	1.29 (<0.01)*** {1.89}	0.90 (<0.01)*** {1.12}	1.38 (<0.01)*** {2.24}	1.30 (<0.01)*** {2.02}
$Firm_d / Firm_h$	-	0.06 (<0.01)*** [0.02]	-	0.14 (<0.01)*** [0.05]	-	0.02 (<0.01)*** [<0.01]
Log likelihood	-5,293,688	-5,228,885	-3,120,020	-3,064,266	-2,134,452	-2,132,170
# of obs.	5,224,042		3,351,099		1,872,943	

Notes: Estimates from the conditional logit are reported. Standard errors are in parentheses. Elasticities or proportional changes in choice probability are in brackets. ***: significant at 1%, **: significant at 5%, *: significant at 10%. Commuters choose one out of counties within 100 miles from their home county. We do not consider cross-state commuting due to data availability.

CHAPTER 4. REFERENCE-DEPENDENT PREFERENCES IN RISK PREFERENCE ELICITATION METHODS WITH A MULTIPLE PRICE LIST FORMAT

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ABSTRACT

A risk preference elicitation method with a Multiple Price List format (MPL method) is widely used in eliciting risk preferences, which has a menu of binary choice questions. We compare subjects' risk attitudes between an MPL method and one question selected from the MPL method. In most MPL methods used in our experiment, we find that elicited risk attitudes are different between the two elicitation procedures. We examines four possible causes for the different risk attitudes: the lack of incentive compatibility in a pay-one-randomly incentive mechanism, decoy effect, imperceptive preferences and reference-dependent preferences. We find that reference-dependent preferences better explain the different risk attitudes, which suggests that MPL methods are not reliable because loss aversion influences elicited risk aversion.

Keywords: *risk preference elicitation, multiple price list format, reference-dependent preferences*

1. Introduction

Elicitation procedures may influence measured risk preferences. For example, different elicitation procedures may lead to elicit different risk aversion (Harbaugh et al. 2010) and different preference rankings for lotteries (Lichtenstein and Slovic, 1971). Understanding effects of elicitation procedures on measured risk preferences is critical to experimental studies testing economic theories and identifying behavioral factors because the choice of elicitation procedure may decide research outcomes.

We test a risk preference elicitation method with a Multiple Price List format (in short, MPL method). As reviewed in Harrison and Rutström (2008) and Holt and Laury (2014), an MPL method is one elicitation procedure commonly used in the literature. An MPL method has a menu of binary choice questions, and subjects are asked to make decisions sequentially. Our test focuses on whether elicited risk attitudes are consistent between an MPL method and one question selected from the MPL method.[58] To illustrate, suppose that there are two groups of randomly drawn subjects. Group A answers which options they would take in the following three questions in an MPL method:

Group A questions:

1. Win $5 if a die number is 1, and win $0 if the die number is 2-10 vs. Get $0.75
2. Win $5 if a die number is 1, and win $0 if the die number is 2-10 vs. Get $1.00
3. Win $5 if a die number is 1, and win $0 if the die number is 2-10 vs. Get $1.25

Group B is only presented with one question selected from the MPL set:

2’. Win $5 if a die number is 1, and win $0 if the die number is 2-10 vs. Get $1.00

[58] MPL methods have been tested in other directions. Some researchers have interest in whether the sequence of questions and the availability of information on subsequent questions influence subjects’ choices in an MPL method (Bruner, 2009; Lévy-Garboua et al. 2012). Some other researchers explore whether omitting some questions and manipulating scale of options influence elicited preferences in an MPL method (Anderson et al. 2006; Bosch-Domènech and Silvestre 2013; Beauchamp et al. 2012), and whether subjects’ low numeracy influences elicited preferences (Dave et al. 2010). These studies are different from our concern on MPL methods.

If subjects' choices are different between groups A and B, procedure invariance would be violated. There are four possible causes for the violation of procedure invariance between an MPL method and one question selected from the MPL method: the lack of incentive compatibility in a pay-one-randomly incentive mechanism, decoy effect, imperceptive preferences, and reference-dependent preferences.

First, the lack of incentive compatibility in a pay-one-randomly mechanism may lead to different risk attitudes between the two elicitation procedures. A pay-one-randomly mechanism compensates subjects for one randomly-chosen question, which is usually used in MPL methods. For a pay-one-randomly mechanism to elicit subjects' true preferences, subjects' preferences should satisfy a set of axioms—in particular, the independence axiom and the reduction-of-compound-lottery axiom (Holt, 1986; Segal, 1988). If those axioms do not hold, elicited risk attitudes between an MPL method and one question selected from the MPL method would be different (Freeman et al. 2012). Several studies investigated whether a pay-one-randomly mechanism is incentive compatible but found mixed results (Starmer and Sugden, 1991; Beattie and Loomes, 1997; Cubitt et al. 1998; Cox et al. 2014a).

Second, the decoy effect may lead to different risk attitudes between the two elicitation procedures. The decoy effect is one violation of the-independence-of-irrelevant-alternatives axiom in decision theory (Huber et al. 1982); the presence of asymmetrically dominated third option increases the choice of the dominating option. Cox et al. (2014b) found evidence that the decoy effect influences subjects' lottery choices. The authors conjectured that the decoy effect might lead to different lottery choices between an MPL method and one question selected from the MPL method. To illustrate their conjecture, we go back to our previous example. Recall that group A chooses between a lottery and $0.75 in the first question, and between the lottery and $1 in the second question. If the decoy effect influences subjects' choices, then group A would choose the $1 more than would group B because the $0.75 would make the $1 look more attractive.

Third, imperceptive preferences may lead to different risk attitudes between the two elicitation procedures. Imperceptive preferences mean that subjects cannot distinguish two options until the options are perceivably different in terms of their utility (Castillo and Eil, 2014). If imperceptive preferences influence subjects' choices, subjects would stick to the status quo option in the MPL method until another option becomes perceivably better than the status quo option. In the previous example, suppose that everyone would rank the lottery as better than the certain $0.75 in question 1, but that everyone would be indifferent between the lottery and the certain $1 in question 2. Everyone in group A would choose the lottery in question 1 and would also select the lottery in question 2. Group A would not switch from the lottery to the certain amount until the certain cash offer rises enough to dominate the lottery. However, group B would only see question 2 and would be divided equally between the lottery and the certain $1. Thus, imperceptive preferences would lead to different risk attitudes between an MPL method and one question selected from the MPL method.

Fourth, reference-dependent preferences may lead to different measured risk attitudes between the two elicitation procedures. Bateman et al. (1997, 2005) found evidence of reference-dependent preferences in MPL methods. In our context, when group A makes choices on the series of questions, they might view one option as a reference point and compare the other option relative to the one option. We construct theoretical predictions on subjects' choices between an MPL method and one question selected from the MPL method by using Kőszegi and Rabin's (2007) reference-dependent preference model. Differently from the other three hypotheses, the hypothesis does not predicts different measured risk attitudes between an MPL method and one question selected from the MPL method for all

MPL methods; the hypothesis predicts different elicited risk attitudes for some MPL methods, not for the other MPL method.

We find different elicited risk attitudes between an MPL method and one question selected from the MPL method for most MPL methods used in our experiment. Among the four potential causes, reference-dependent preferences better explain our results than do the other causes, which suggests that elicited risk attitudes in MPL methods would not be reliable because individual loss aversion influences elicited risk aversion.

Our results provide useful implications to the literature. First, our results do not support Freeman et al. (2012) or Castillo and Eil (2014). Freeman et al. did a similar experiment to ours but explained their results using the lack of incentive compatibility in a pay-one-randomly incentive mechanism. Their argument is not consistent with our results. Castillo and Eil proposed imperceptive preferences, but their preference model is not consistent with our results. Second, our results suggest that reference points in MPL methods are not necessarily the options subjects choose in a few first questions of MPL methods. This is contrasted with Sprenger (2010); he assumed that subjects' reference points in MPL methods are always the options subjects choose in a few first questions of MPL method. Our results call into questions his assumptions on reference points in MPL methods, which requires further investigations. Third, our results provide insights to the literature on procedure invariance. For example, Harbaugh et al. (2010) found a violation of procedure invariance between choice and pricing tasks and conjectured that cognitive load in pricing tasks may cause the violation. Our results provide another explanation to their results; reference-dependent preferences in pricing tasks might contribute to different elicited risk attitudes between choice and pricing tasks.

2. Experimental design and predictions for lottery choices

We describe a basic design of our experiment. Our design is slightly different from the example in the previous section. We ask one more question to group A. Group A answers three questions as well as one more question:

1. Win $5 if a die number is 1, and win $0 if the die number is 2-10 vs. Get $0.75
2. Win $5 if a die number is 1, and win $0 if the die number is 2-10 vs. Get $1.00
3. Win $5 if a die number is 1, and win $0 if the die number is 2-10 vs. Get $1.25

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4. Win $5 if a die number is 1, and win $0 if the die number is 2-10 vs. Get $1.00

Question 4 is identical to the question 2. The dotted line denotes that question 4 is asked separately from the first three questions; question 4 and the first three questions are printed on different survey pages so that subjects cannot see the first three questions when they answer question 4. We add question 4 because, as we will show later, predictions from the lack-of-incentive-compatibility hypothesis do not have specific signs of changes in elicited risk attitudes between an MPL method and one question selected from the MPL method. Comparing subjects' decisions for questions 4 and 2' provides us more cases to test the lack-of-incentive-compatibility hypothesis. Group B answers only one question:

2'. Win $5 if a die number is 1, and win $0 if the die number is 2-10 vs. Get $1.00

Note that questions 2, 4 and 2' have same options but different decision circumstances. Question 2 is asked within the MPL method while question 4 is asked separately from the MPL method. Question 2' is asked solely; subjects answer only the question 2'. We set subjects' decisions on question 2' as a baseline. Comparing subjects' decisions between questions 2 and 2' allows us to test all four hypotheses. Comparing subjects' decisions for questions 4 and 2' allows us to test the lack-of-incentive-compatibility hypothesis.

In our experiment, we test three MPL methods: two MPL methods eliciting certainty equivalents for a low-probability lottery and a middle-probability lottery, and Holt and Laury's (2002) MPL method. We choose those MPL methods because they are widely used in the literature. We call those MPL methods as a CE1, a CE2 and an HL, respectively. Each of those MPL methods has a menu of ten binary choice questions illustrated in Table 1. Subjects choose between a lottery and money in the CE1 and the CE2, and between two lotteries in the HL. In deriving predictions, we focus on subjects' decisions in the shaded questions. As described in the example above, each of the shaded questions is asked in three different ways. First, the shaded question is asked within the MPL method. Second, the shaded question is asked separately from the MPL method. Lastly, only the shaded question is asked.

We derive predictions of four hypotheses for lottery choices. The predictions are summarized in Table 2. Each column has a different question from a different MPL method. For example, column (1) considers subjects' choices in a question from the CE1 in which subjects choose between a lottery and $1. We set subjects' choices when only one question is asked as a baseline. Columns (1)-(4) report expected changes in the probability of choosing a less risky option when the question is asked within the MPL method compared to the baseline. Columns (5)-(7) report expected changes in the probability of choosing a less risky option when the question is asked separately from the MPL method compared to the baseline. Four hypotheses have different predictions on subjects' choices.

First, we consider the lack-of-incentive-compatibility hypothesis. In Table 2, the hypothesis predicts different elicited risk attitudes in all columns because a pay-one-randomly incentive mechanism is assumed to be not incentive compatible. Holt (1986) and Segal (1988) argued that if subjects' preferences do not satisfy the independence axiom and the reduction-of-compound-lotteries axiom, a pay-one-randomly mechanism would not be incentive compatible. If the incentive mechanism is not incentive compatible, subjects' choices would be different in all columns. Note that the hypothesis only predicts differences in subjects' choices in all columns; the hypothesis does not predict a certain sign of differences in subjects' choices.

Second, we consider the decoy effect hypothesis. In columns (1)-(2), the hypothesis predicts that subjects are more likely to choose a money option over a lottery when the question is asked within the MPL method compared to when only the question is asked. This is because a money option in a previous question is asymmetrically dominated to another money option in a current question, as described in the example in the previous section. Recall that money options in the CE1 and the CE2 are in ascending order. However, the hypothesis does not have predictions on subjects' choices in columns (3)-(4) because there are no clear asymmetric-dominance relationships across options in the HL. In columns (5)-(7), the decoy effect hypothesis predicts no changes in subjects' choices because there is no third option that subjects can see when they make decisions.

Third, we consider the imperceptive preference hypothesis. In columns (1)-(2), the hypothesis predicts that subjects choose a money option when the question is asked within the MPL method compared to when only the question is asked. This is because imperceptive preferences makes subjects choose a lottery until a money option becomes perceivably better. Subjects usually choose a lottery in a few first questions in the CE1 and the CE2. Thus subjects are more likely to choose a lottery when the question is asked within the MPL method compared to when only the question is asked. In columns (3)-(4), the imperceptive preference hypothesis predicts that subjects are more likely to choose a less risky lottery when the question is asked within the MPL method compared to when only the question is asked. This is because subjects usually choose a less risky lottery in a few first questions in the HL, and they choose the status quo option until a more risky lottery becomes perceivably better. In columns (5)-(7), the hypothesis predicts no changes in subjects' choices because there are no prior questions influencing subjects' decisions when they make decisions.

Lastly, we consider the reference-dependent preference hypothesis. To derive predictions from the hypothesis, we need to figure out which option is a reference point for subjects in MPL methods. Based on Weber et al.'s (2000) experimental data, we assume that reference points are a lottery in the CE1 but a money option in the CE2 for the majority of subjects. We do not mean that all subjects have a same reference point as we assume. Rather, the majority of subjects have a same reference point as we assume. In Weber et al.'s experiment, they elicited subjects' certainty equivalents, buying prices and selling prices for a low-probability lottery and a high-probability lottery. They found that subjects' certainty equivalents for a low-probability lottery were closer to their selling prices than to their buying prices, which implies that a low-probability lottery would be a reference point in the CE1 for the majority of subjects. On the contrary, the authors found that a certainty equivalent for a high-probability lottery is closer to their buying prices, which implies that money options would be reference points in the CE2 for the majority of subjects.[59] With those reference points, we derive predictions of the reference-dependent preference hypothesis using Kőszegi and Rabin's (2007) reference-dependent preference model. Their model allows us to handle stochastic reference points formed by subjects' beliefs or expectations. In Figure 1, we sketch predictions of their model. In the figure, we assume that losses have twice larger marginal utility than do the same size of gains; a coefficient of loss aversion is 2. The formal

[59] In detail, Weber et al. used a low-probability lottery giving 100 with a probability of 0.25 and 0 with a probability of 0.75; and a high-probability lottery giving 100 with a probability of 0.75 and 0 with a probability of 0.25. WTP, CE and WTA for the former are 22.8, 26.4 and 27.1, respectively. WTP, CE and WTA for the latter are 69.2, 71 and 75.2.

derivation is available in the Appendix. Panel (a) of Figure 1 represents the relative utility of money options to a lottery in questions included in the CE1. The solid line represents the relative utility of money options to a lottery when the question is asked within the MPL method while the dotted line represents the relative utility of money options to a lottery when only the question is asked. At the point where a money option is $1, the solid line is very close to the dotted line, which suggests that subjects' choices when the question is asked within the MPL method would be similar to ones when only the question is asked. Panel (b) represents the relative utility of money options to a lottery in questions included in the CE2. At the point where a money option is $2.50, the solid line is higher than the dotted line, which suggests that subjects are more likely to choose the $2.50 when the question is asked within the MPL method than when only the question is asked. When it comes to the HL, we assume that more risky lotteries are reference points in the HL for the majority of subjects because higher payoffs in more risky lotteries would be more salient to subjects than higher payoffs in less risky lotteries. Those reference points seems to be supported by Kim et al. (2012). The authors found that subjects paid more attention to a high-payoff low-probability lottery than to a low-payoff high-probability lottery in their eye-tracking experiment. Panel (c) of Figure 1 represents the relative utility of a less risky lottery to a more risky lottery in questions included in the HL. At the point where a probability for higher payoffs of lotteries is 0.4, the solid line is very close to the dotted line, which implies that subjects' choices when the question is asked within the MPL method are similar to when only the question is asked. On the contrary, at the point where a probability for higher payoffs of lotteries is 0.7, the solid line is lower than the solid line, which implies that subjects are less likely to choose a less risky lottery when the question is asked within the MPL method than when only the question is asked. In columns (5)-(7), the reference-dependent preference hypothesis predicts no changes in subjects' choices because subjects' reference points are the status quo of $0 in both cases: when the question is asked separately from the MPL method and when only the question is asked.

We used a between-subjects design; we recruited two groups of subjects for each of three MPL methods. In each pair of groups, one group answered 11 binary choice questions; ten questions in an MPL method and one question selected from the MPL method. The latter was asked separately from the MPL method in a way that the question was printed on a different page. Three buffer questions were inserted between the former and the latter.[60] To address order effects, we used two opposite sequences of questions: (MPL method)-(One question) or (One question)-(MPL method). The other group answered only one question selected from an MPL method. All sessions were conducted on paper and pencil. Lotteries were described using a ten-sided die to provide a better understanding to subjects. Experimental instructions are available in the Appendix.

All sessions were conducted in classrooms on a university campus in 2014. We ran sessions in classrooms 10 minutes before a class ended or right after class. We sometimes conducted sessions in a waiting area due to tight classroom schedules. We recruited 329 students. In the beginning of sessions, we briefly explained our experiment, showed a ten-sided die and told that the die would be rolled at the end of experiment to decide compensation. At the end of experiment, we randomly picked one question, rolled the die and paid compensation.

3. Results

We provides an overview on subjects' lottery choices. Table 3 reports the proportions of subjects choosing a less risky option depending on different decision circumstances. Each

[60] The two questions were borrowed from the cognitive reflection test (Frederick, 2005). The third question was to choose the sixth highest out of ten money amounts.

column has a different question from a different MPL method. For example, column (1) reports subjects' choices in a question of choosing between a lottery and \$1. We set subjects' choices when only the question is asked as a baseline. In columns (1) and (3), subjects' choices when the question is asked within the MPL method are similar to the baseline for the CE1 and the first half of the HL. For example, in column (1), subjects choosing a less risky option are 53% in the former and 51% in the baseline. On the contrary, in columns (2) and (4), subjects' choices when the question is asked within the MPL method are much different from the baseline for the CE2 and the second half of the HL. In all columns, subjects' choices when the question is asked separately from the MPL method are somewhat similar to the baseline. Those patterns are confirmed in probit analyses in Table 4.

We conduct a probit estimation of choosing a less risky option in Table 4. The baseline is subjects' choices when only the question is asked. The estimation specification in columns (1)-(4) includes a dummy variable of whether the question is asked within the MPL method and two more dummy variables for male and age. In columns (1) and (3), estimated coefficients for dummy variables of whether the question is asked within the MPL method are not significant at the 10% significance level, which implies that subjects' choices are not different between an MPL method and one question selected from the MPL method for the CE1 and the first half of the HL. On the contrary, in columns (2) and (4), estimated coefficients for the dummy variables are significantly positive and negative, respectively, which implies that subjects' choices are different between an MPL method and one question selected from the MPL method for the CE2 and the second half of the HL. Thus in most MPL methods used in our experiment, procedure invariance between the two elicitation procedures is not valid. Columns (5)-(7) includes a dummy variable of whether the question is asked separately from the MPL method. In all the columns, estimated coefficients for the dummy variable are not significant at the 5% significance level, which suggests that subjects' choices when the question is asked separately from the MPL method are similar to the baseline for all MPL methods.[61]

We test four hypotheses for subjects' decisions in MPL methods. The estimation results from Table 4 are summarized at the bottom row of Table 2. First, we test the lack-of-incentive-compatibility hypothesis. The estimation results do not support the hypothesis in all columns except (2) and (4). Thus, we reject the hypothesis. Second, we test the decoy effect hypothesis. The estimation results do not support the hypothesis only in column (1). However, we cannot test the hypothesis in columns (3) and (4) because the hypothesis does not have predictions for those columns. Thus the hypothesis needs further investigations. Third, we test the imperceptive preference hypothesis. The estimation results do not support the hypothesis in columns (1)-(4). Thus, we reject the hypothesis. Lastly, we test the reference-dependent preference hypothesis. Our results support the hypothesis in all columns. Thus we accept the hypothesis. We conclude that the reference-dependent preference hypothesis better explains subjects' choices in MPL methods.

4. Discussion

In most MPL methods used in our experiment, we have found that elicited risk attitudes are different between an MPL method and one question selected from the MPL method. We have tested four possible causes for the different risk attitudes and found that reference-dependent preferences better explain our results than do the other causes, which suggests that MPL methods are not reliable because loss aversion influences elicited risk aversion.

[61] The p-value for the dummy variable in column (5) is 0.099.

Our results provide useful implications to the literature. First, our results do not support Freeman et al.'s (2012) or Castillo and Eil (2014). Freeman et al. argued that the lack of incentive compatibility in a pay-one-randomly incentive mechanism would lead to different elicited risk attitudes between an MPL method and one question selected from the MPL method. Their argument is not consistent with our results. Castillo and Eil proposed imperceptive preferences, but their preference model is not consistent with our results. Second, our results call into questions Sprenger's (2010) assumption on reference points in MPL methods. He tested whether subjects' decisions in MPL methods follow reference-dependent preferences. He assumed that the options subjects choose in a first few questions in MPL methods are subjects' reference points. Our results suggest that his assumption is not necessarily true. In our results, the options subjects did not choose in a first few questions in MPM methods were subjects' reference points in some MPL methods. Further investigations are required to address which options are reference points in MPL methods. Third, our results provide insights to the literature about procedure invariance. For example, Harbaugh et al. (2010) found a violation of procedure invariance between choice and pricing tasks. They conjectured that cognitive load in pricing tasks may cause the violation. Our results provide a new explanation to their results; the formation of reference-dependent preferences in pricing tasks would lead to different elicited risk attitudes between choice and pricing tasks.

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Table 1 Multiple price list methods used in the experiment

MPL method eliciting a certainty equivalent of a lottery with a low probability: CE1		MPL method eliciting a certainty equivalent of a lottery with a middle probability: CE2		Holt and Laury's (2002) MPL method: HL	
Option 1	Option 2	Option 1	Option 2	Option 1	Option 2
($5, 0.1, $0, 0.9)	$0.25	($5, 0.5, $0, 0.5)	$1.00	($2, 0.1, $1.50, 0.9)	($3.75, 0.1, $0.10, 0.9)
($5, 0.1, $0, 0.9)	$0.50	($5, 0.5, $0, 0.5)	$1.25	($2, 0.2, $1.50, 0.8)	($3.75, 0.2, $0.10, 0.8)
($5, 0.1, $0, 0.9)	$0.75	($5, 0.5, $0, 0.5)	$1.50	($2, 0.3, $1.50, 0.7)	($3.75, 0.3, $0.10, 0.7)
($5, 0.1, $0, 0.9)	$1.00	($5, 0.5, $0, 0.5)	$1.75	($2, 0.4, $1.50, 0.6)	($3.75, 0.4, $0.10, 0.6)
($5, 0.1, $0, 0.9)	$1.25	($5, 0.5, $0, 0.5)	$2.00	($2, 0.5, $1.50, 0.5)	($3.75, 0.5, $0.10, 0.5)
($5, 0.1, $0, 0.9)	$1.50	($5, 0.5, $0, 0.5)	$2.25	($2, 0.6, $1.50, 0.4)	($3.75, 0.6, $0.10, 0.4)
($5, 0.1, $0, 0.9)	$1.75	($5, 0.5, $0, 0.5)	$2.50	($2, 0.7, $1.50, 0.3)	($3.75, 0.7, $0.10, 0.3)
($5, 0.1, $0, 0.9)	$2.00	($5, 0.5, $0, 0.5)	$2.75	($2, 0.8, $1.50, 0.2)	($3.75, 0.8, $0.10, 0.2)
($5, 0.1, $0, 0.9)	$2.25	($5, 0.5, $0, 0.5)	$3.00	($2, 0.9, $1.50, 0.1)	($3.75, 0.9, $0.10, 0.1)
($5, 0.1, $0, 0.9)	$2.50	($5, 0.5, $0, 0.5)	$3.25	($2, 1, $1.50, 0)	($3.75, 1, $0.10, 0)

Notes: ($5, 0.1, $0, 0.9) denotes a lottery giving $5 with a probability of 0.1, and nothing with a probability of 0.9. The shaded questions were asked in three different ways. First, the question was asked within the MPL method. Second, the question was asked separately from the MPL method. Third, only the question was asked. The shaded color was not shown to subjects.

Table 2 Predictions and test results: Changes in the proportion of subjects choosing a less risky option

Hypothesis	When the question is asked within the MPL method compared to when only the question is asked				When the question is asked within the MPL method compared to when only the question is asked		
	CE1: ($5, 0.1, $0, 0.9) vs. $1 (1)	CE2: ($5, 0.5, $0, 0.5) vs. $2.50 (2)	HL: ($2, 0.4, $1.50, 0.6) vs. ($3.75, 0.4, $0.10, 0.6) (3)	HL: ($2, 0.7, $1.50, 0.3) vs. ($3.75, 0.7, $0.10, 0.3) (4)	CE1: ($5, 0.1, $0, 0.9) vs. $1 (5)	CE2: ($5, 0.5, $0, 0.5) vs. $2.50 (6)	HL: ($2, 0.7, $1.50, 0.3) vs. ($3.75, 0.7, $0.10, 0.3) (7)
Lack of incentive compatibility	+ / —	+ / —	+ / —	+ / —	+ / —	+ / —	+ / —
Decoy effect	+	+	Not available	Not available	No change	No change	No change
Imperceptive preference	—	—	+	+	No change	No change	No change
Reference-dependent preference	No change	+	No change	—	No change	No change	No change
Our results	No change	+	No change	—	No change	No change	No change

Table 3 Proportion of subjects choosing a less risky option

	CE1: ($5, 0.1, $0, 0.9) vs. $1 (1)	CE2: ($5, 0.5, $0, 0.5) vs. $2.50 (2)	HL: ($2, 0.4, $1.50, 0.6) vs. ($3.75, 0.4, $0.10, 0.6) (3)	HL: ($2, 0.7, $1.50, 0.3) vs. ($3.75, 0.7, $0.10, 0.3) (4)
When the question is asked within the MPL method	0.53	0.64	0.80	0.19
When the question is asked separately from the MPL method	0.69	0.45	Not available	0.48
When only the question is asked	0.51	0.34	0.76	0.47
Obs.	70 / 70 / 35	64 / 64 / 32	64 / · / 30	64 / 63 / 34

Table 4 Probit analysis of choosing a less risky option

	CE1: ($5, 0.1, $0, 0.9) vs. $1 (1)	CE2: ($5, 0.5, $0, 0.5) vs. $2.50 (2)	HL: ($2, 0.4, $1.50, 0.6) vs. ($3.75, 0.4, $0.10, 0.6) (3)	HL: ($2, 0.7, $1.50, 0.3) vs. ($3.75, 0.7, $0.10, 0.3) (4)	CE1: ($5, 0.1, $0, 0.9) vs. $1 (5)	CE2: ($5, 0.5, $0, 0.5) vs. $2.50 (6)	HL: ($2, 0.7, $1.50, 0.3) vs. ($3.75, 0.7, $0.10, 0.3) (7)
When the question is asked within the MPL method	-0.04 (0.26)	0.78 (0.28)***	0.07 (0.31)	-0.85 (0.29)***	-	-	-
When the question is asked separately from the MPL method	-	-	-	-	0.44 (0.27)*	0.27 (0.28)	-0.01 (0.27)
Male	-0.12 (0.25)	-0.15 (0.27)	0.10 (0.30)	0.28 (0.29)	-0.18 (0.26)	-0.20 (0.26)	-0.38 (0.26)
Older than 20	0.14 (0.27)	-0.18 (0.29)	-0.04 (0.31)	0.09 (0.29)	-0.04 (0.27)	0.21 (0.28)	-0.19 (0.27)
Log-likelihood	-70.45	-61.56	-48.06	-51.91	-65.65	-63.34	-64.29
Obs.	102	95	92	96	102	95	95

The baseline is subjects' choices when only the question is asked. Standard errors are reported in the parentheses. *: p-value<0.10, ***:<0.01. One to three observations are missing across columns due to no responses in gender and age.

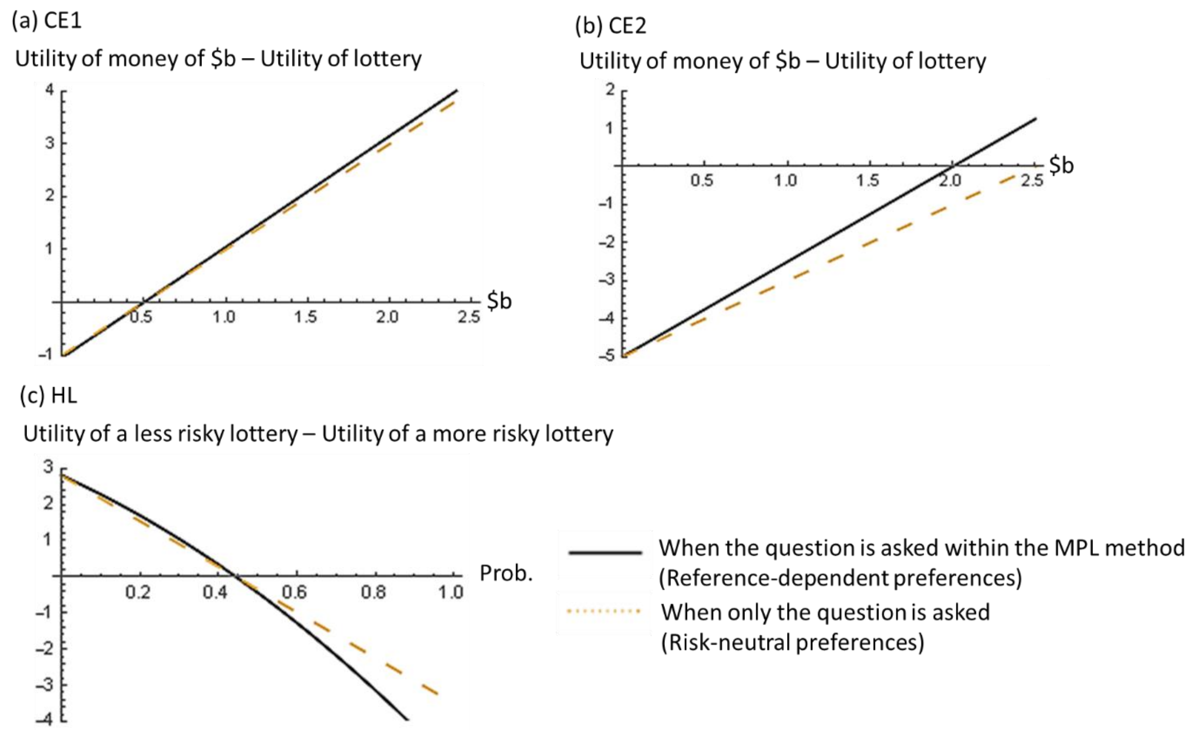

Figure 1 Relative utility of a less risky option to a more risky option

Appendix A: Predictions of the reference-dependent preference hypothesis on subjects' choices

We derive predictions of the reference-dependent preference hypothesis on subjects' choices. We have an interest in subjects' choices between an MPL method and one question selected from the MPL method. We consider three MPL methods: two MPL methods eliciting certainty equivalents of lotteries, and Holt and Laury's (2002) MPL method. We call those MPL methods as a CE1, a CE2 and an HL. We create predictions using Kőszegi and Rabin's (2007; KR hereafter) reference-dependent preference model. The model allows us to handle stochastic reference points.

We explain a utility function used in the KR model. The utility, *u* consists of two components for a given consumption bundle, *c*: consumption utility, *m* and gain-loss utility, μ relative to the reference point, *r*.

$$u(c|r) = m(c) + \mu(m(c) - m(r)) \tag{1}$$

We assume that the consumption utility is linear: $m(x) = x$. We also assume that the gain-loss utility is kinked around zero: $\mu(x) = x$ for $x \geq 0$ and $\mu(x) = -\lambda x$ for $x < 0$ where λ is a loss aversion coefficient. We assume $\lambda = 2$; losses have twice larger marginal utility than do the same size of gains. Thus the utility function in equation (1) becomes:

$$u(c|r) = \begin{Bmatrix} c + (c-r) & if\ c - r \geq 0 \\ c - \lambda(r-c) & if\ c - r < 0 \end{Bmatrix} \tag{2}$$

Prediction for subjects' choices in the CE1 and one question selected from the CE1: Subjects choose between a lottery and a money option in the CE1: $A \equiv (\$a, p, \$0, 1-p)$ and $B \equiv \$b$

where $a > b$. The former denotes a lottery giving \$$a$ with a probability of p and \$0 with a probability of 1-p. Specifically, the lottery in the CE1 is (\$5, 0.1, \$0, 0.9).

First, we consider utilities of the lottery and a money option when subjects answer the CE1. Suppose that subjects' reference points are the lottery, which has been discussed in the text. Subjects' utilities for the lottery and a money option are:

$$U(A|A) = p \cdot u(a|A) + (1-p) \cdot u(0|A)$$

$$= p[pu(a|a) + (1-p)u(a|0)] + (1-p)[pu(0|a) + (1-p)u(0|0)]$$

$$=p[pa + (1-p)(a+a)] + (1-p)[p(-\lambda)a] \quad (3)$$

$$U(B|A) = pu(b|a) + (1-p)u(b|0)$$

$$= p(b - \lambda(a-b)) + (1-p)(b+b) \quad (4)$$

Second, we consider utilities of the lottery and a money option when subjects answer only one question from the CE1. We assume that subjects' reference point is \$0. Subjects' utilities for the lottery and a money option are:

$$U(A|0) = p \cdot u(a|0) + (1-p) \cdot u(0|0)$$

$$= p(a+a) \quad (5)$$

$$U(B|0) = b + b \quad (6)$$

Panel (a) of Figure 1 in the text represents the relative utility of a money option to the lottery. The relative utility is calculated as 'utility of a money option' – 'utility of the lottery'. The solid line represents relative utilities when the question is asked within the CE1. The dotted line represents relative utilities when only the question is asked. We focus on relative utilities for a question of choosing between (\$5, 0.1, \$0, 0.9) and \$1. At the point where the money option is \$1, the solid line is close to the dotted line, which suggests that the relative utility of \$1 to the lottery when the question is asked within the CE1 is similar to one when only the question is asked.

Prediction for subjects' choices in the CE2 and one question selected from the CE2: Subjects choose between a lottery and a money option in the CE2: $A \equiv (\$a, p, \$0, 1-p)$ and $B \equiv \$b$ where $a > b$. Specifically, the lottery in the CE2 is (\$5, 0.5, \$0, 0.5).

First, we consider utilities of the lottery and a money option when subjects answer the CE2. Suppose that subjects' reference points are a money option. Subjects' utilities for the lottery and a money option are:

$$U(A|B) = p \cdot u(a|b) + (1-p) \cdot u(0|b)$$
$$= p[a + (a-b)] + (1-p)[0 + (-\lambda)b] \quad (7)$$

$$U(b|B) = b + (b-b) \quad (8)$$

Second, we consider utilities of the lottery and a money option when subjects answer only one question from the CE2. We assume that subjects' reference point is \$0. Subjects' utilities for the lottery and a money option are:

$$U(A|0) = p \cdot u(a|0) + (1-p) \cdot u(0|0)$$
$$= p(a + a) \quad (9)$$
$$U(B|0) = b + b \quad (10)$$

Panel (b) of Figure 1 represents the relative utility of a money option to the lottery. We focus on relative utilities for a question of choosing between (\$5, 0.5, \$0, 0.5) and \$1. At the point where the money option is \$2.50, the solid line is higher than the dotted line, which suggests that subjects are more likely to choose the \$2.50 when the question is asked within the CE2 than when only the question is asked.

Prediction for subjects' choices in the HL and one question selected from the HL: Subjects choose between two lotteries in the HL: $X \equiv (\$x_1, p, \$x_2, 1-p)$ and $Y \equiv (\$y_1, p, \$y_2, 1-p)$ where (x_1, x_2, y_1, y_2)=(\$2, \$1.50, \$3.75, \$0.10). First, we consider utilities of two lotteries when subjects answer the HL. Suppose that subjects' reference points are a more risky lottery. Subjects' utilities for two lotteries are:

$$U(X|Y) = p \cdot u(x_1|Y) + (1-p) \cdot u(x_2|Y)$$

$$= p[pu(x_1|y_1) + (1-p)u(x_1|y_2)] + (1-p)[pu(x_2|y_1) + (1-p)u(x_2|y_2)]$$

$$= p[p(x_1 - \lambda(y_1 - x_1)) + (1-p)(x_1 + (x_1 - y_2))]$$

$$+(1-p)[p(x_2 - \lambda(y_1 - x_2)) + (1-p)(x_2 + (x_2 - y_2)] \tag{11}$$

$$U(Y|Y) = p \cdot u(y_1|Y) + (1-p) \cdot u(y_2|Y)$$

$$= p[pu(y_1|y_1) + (1-p)u(y_1|y_2)] + (1-p)[pu(y_2|y_1) + (1-p)u(y_2|y_2)]$$

$$= p[py_1 + (1-p)(y_1 + (y_1 - y_2))] + (1-p)\left[p\left(y_2 - \lambda(y_1 - y_2)\right) + (1-p)y_2\right] \tag{12}$$

Second, we consider utilities of two lotteries when subjects answer only one question from the HL. We assume that subjects' reference point is \$0. Subjects' utilities for the lottery and a money option are:

$$U(X|0) = p \cdot u(x_1|0) + (1-p) \cdot u(x_2|0)$$

$$= p(x_1 + (x_1 - 0)) + (1-p)(x_2 + (x_2 - 0)) \tag{13}$$

$$U(Y|0) = p(y_1 + (y_1 - 0)) + (1-p)(y_2 + (y_2 - 0)) \tag{14}$$

Panel (c) of Figure 1 represents the relative utility of a less risky lottery to a more risky lottery. We focus on relative utilities for a question of choosing between ($2, 0.4, $1.50, 0.6) and ($3.75, 0.4, $0.10, 0.6). At the point where a probability for higher payoffs in lotteries is 0.4, the solid line is close to the dotted line, which suggests that subjects' choices when the question is asked within the HL are similar to when only the question is asked. Next, we focus on relative utilities for a question of choosing between ($2, 0.7, $1.50, 0.3) and ($3.75, 0.7, $0.10, 0.3). At the point where a probability for higher payoffs in lotteries is 0.7, the solid line is lower than the dotted line, which suggests that subjects are less likely to choose the former when the question is asked within the HL than when only the question is asked.

Appendix B: Experimental instructions

Experiment Title: Economic Choices

Participant ID (Any word can be your ID. For example, tree or bird.):

____

This is a research experiment about economic decision making. You MUST be at least 18 years old to participate in the experiment. The experiment will last approximately three minutes. Your total earnings will be $2.40 on average.

You will be asked to choose between a lottery and a fixed amount of money in each of 11 questions. When you finish the experiment, you will get paid based on your choice for **one** question, randomly drawn. If you have chosen a lottery for that question, we will throw a ten-sided die to decide your earnings from the lottery. You will also receive $1 for your participation.

You will fill out two forms. Please go to the next page to begin the first form.

Please answer the following questions:

Question	Option A		Option B	
1. Which option would you like to take?	A) You receive $5 if a ten-sided die reads 1, and $0 if the die reads 2-10.	☐	B) You receive $0.25.	☐
2. Which option would you like to take?	A) You receive $5 if a ten-sided die reads 1, and $0 if the die reads 2-10.	☐	B) You receive $0.50.	☐
3. Which option would you like to take?	A) You receive $5 if a ten-sided die reads 1, and $0 if the die reads 2-10.	☐	B) You receive $0.75.	☐
4. Which option would you like to take?	A) You receive $5 if a ten-sided die reads 1, and $0 if the die reads 2-10.	☐	B) You receive $1.00.	☐
5. Which option would you like to take?	A) You receive $5 if a ten-sided die reads 1, and $0 if the die reads 2-10.	☐	B) You receive $1.25.	☐
6. Which option would you like to take?	A) You receive $5 if a ten-sided die reads 1, and $0 if the die reads 2-10.	☐	B) You receive $1.50.	☐
7. Which option would you like to take?	A) You receive $5 if a ten-sided die reads 1, and $0 if the die reads 2-10.	☐	B) You receive $1.75.	☐
8. Which option would you like to take?	A) You receive $5 if a ten-sided die reads 1, and $0 if the die reads 2-10.	☐	B) You receive $2.00.	☐
9. Which option would you like to take?	A) You receive $5 if a ten-sided die reads 1, and $0 if the die reads 2-10.	☐	B) You receive $2.25.	☐
10. Which option would you like to take?	A) You receive $5 if a ten-sided die reads 1, and $0 if the die reads 2-10.	☐	B) You receive $2.50.	☐

Please answer the following three questions:

A. A bat and a ball cost $1.10 in total. The bat costs $1.00 more than the ball. How much does the ball cost? [$.]

B. If it takes 5 minutes for 5 machines to make 5 widgets, how long would it take for 100 machines to make 100 widgets? [ ] minutes

C. There are nine numbers. Please circle the number that is fifth from the largest: [$2.25, $1.00, $1.15, $1.25, $1.50, $1.20, $1.75, $0.75, $2.00]

Please return this form to an experiment facilitator and receive another one.

Experiment Title: Economic Choices
Participant ID that you used in the previous form: ________________________________

Please answer the following question:

11. Which option would you like to take?

- Option A: You receive $5 if a ten-sided die reads 1, and $0 if the die reads 2-10.
- Option B: You receive $1.

Option A	Option B
☐	☐

Your answers for the following questions do <u>not</u> affect your earnings, but please answer carefully.

A. Suppose that you would decide whether or not you would like to play a lottery. The result of the lottery would be determined by flipping a coin.

- Lottery: WIN $3 if the flipped coin is a head, and LOSE $2 if the flipped coin is a tail

Would you like to play the lottery?

Yes	No
☐	☐

B. What is your gender? [Male ☐ / Female ☐]

C. What is your age? [   ]

Thanks! Please let an experiment facilitator know that you have finished. The experiment facilitator will randomly draw one question to decide your earnings.

CHAPTER 5. PRE-PLAY LEARNING AS A REMEDY FOR PREFERENCE REVERSAL

Younjun Kim and Elizabeth Hoffman

ABSTRACT

The preference reversal phenomenon is an iconic empirical puzzle in decision theory, which is inconsistent preference rankings for a low-payoff high-probability lottery and a high-payoff low-probability lottery in terms of choice and pricing. The preference reversal has challenged standard economic theory. We test whether pre-play learning removes the preference reversal. Pre-play learning denotes ex-ante lottery learning, where subjects observe playing lotteries before making decisions. In our experiment, we find that pre-play learning makes subjects' distributions of selling prices for lotteries consistent with their choices, which suggests that pre-play learning removes the preference reversal. We explain our finding with the convergence of probability weight functions toward linearity by pre-play learning.

Keywords: *pre-play learning, preference reversal, response mode*

1. Introduction

The preference reversal phenomenon is an iconic empirical puzzle in decision theory. The preference reversal means inconsistent preference rankings of a low-payoff high-probability lottery compared to a high-payoff low-probability lottery in terms of choice and pricing. The preference reversal has received much attention from economists and psychologists because it challenges standard economic theory such as expected utility theory and Hicksian welfare theory (Lichtenstein and Slovic, 2006). Standard economic theory predicts consistent preference rankings for lotteries between choice and pricing, but the prediction is contradicted by the preference reversal. The preference reversal has a predictable pattern which cannot be explained simply by error or noise. The pattern has been confirmed in many studies (Seidl, 2002). Thus researchers have proposed explanations of why the preference reversal occurs and developed new preference theories to accommodate the preference reversal. Slovic and Lichtenstein (1983), Tversky and Thaler (1990) and Cox (2008) provide comprehensive reviews of these studies. Compared to the large volume of literature explaining the preference reversal, trials to remove the preference reversal have not been successful. Our study fills this gap with simple pre-play learning. Pre-play learning means ex-ante lottery learning, where subjects observe playing lotteries before making decisions. We add pre-play learning on a typical experimental design used in other studies and test whether pre-play learning removes the preference reversal.

In our view, pre-play learning has not received adequate attention in the literature. For example, Seidl's (2002) review does not mention lack of pre-play learning as a possible cause of preference reversals. The four main causes cited are a lack of incentive compatibility in elicitation modes (Holt, 1986), intransitive preferences (Loomes and Sugden, 1983), overpricing and underpricing of lotteries (Tversky et al. 1990), and nonlinear probabilities (Rachlin et al. 1991). Other researchers have suggested that preference reversals can be attributed to imprecise preferences (Butler and Loomes, 2007) or salience (Bordalo et al. 2012).

There are several reasons that pre-play learning has the potential to remove preference reversals. First, preference reversal has been conjectured to be "a product of inexperience and lack of motivation" (p.231) in Plott's (1996) preference discovery hypothesis. Plott emphasized that learning from "repeated choices, practice, incentives (feedback)" and "sobering and refocusing experiences" (p.227) allows to reach rational choices. Braga and Starmer's (2005) review of empirical evidence concluded that consumer experiences can refine their preferences or decision-making abilities sufficiently to eliminate preference reversals. Second, learning promotes subjects' choices under risk consistent with standard economic theory. Van de Kuilen and Wakker (2006) found that lottery feedback decreases the common ratio effect inconsistent with standard economic theory. Myagkov and Plott (1997) found that risk-seeking in losses seemed to disappear with experience. Third, learning helps choice and pricing behaviors consistent with standard economic theory. For example, trade experience removes the endowment effect phenomenon, which is consistent with standard economic theory (List, 2004; Engelmann and Hollard, 2010). Detailed instructions and practice rounds can remove the disparity between willingness-to-pay and willingness-to-accept (Plott and Zeiler, 2005). To the extent that preference reversal is due to similarly unfamiliar decisions, pre-play learning provides an opportunity for agents to become familiar with the nature of stochastic outcomes and the consequences of decisions.[62]

The preference reversal has a typical pattern where inconsistent preference rankings between choice and pricing for lotteries are more likely to be observed in subjects choosing a

[62] For a review on how learning influences lottery choices, see Hertwig and Erev (2009). For a comprehensive review on learning and economic behaviors, see Erev and Roth (2014).

low-payoff high-probability lottery than in those choosing a high-payoff low-probability lottery. However, lottery feedback can remove the typical preference reversal pattern (Cox and Grether, 1996). Unfortunately, the lottery feedback effect is limited. Braga et al. (2009) found that lottery feedback removed the typical preference reversal pattern in the first few rounds but created another form of preference reversal pattern in later rounds. The new preference reversal pattern is a mirror image of the typical preference reversal pattern; inconsistent preference rankings are more likely to be observed in subjects choosing a high-payoff low-probability lottery than in those choosing a low-payoff high-probability lottery.

We explore the role of experience on preference reversal by adding pre-play learning to Grether and Plott's (1979) experiment. We replicate their experimental design and instructions both because their experiment carefully controls for potential concerns raised in previous preference-reversal experiments and because their experiment became the prototype for subsequent studies.[63] In our experiment, subjects undergo a pre-play learning exercise before making decisions in choice and pricing tasks. We play each lottery ten times in front of the subjects and ask them to keep a record of lottery outcomes. We use only one pair of lotteries selected from their experiment. As in Grether and Plott, we use the BDM (Becker, DeGroot and Marschak, 1964) mechanism to elicit selling prices for lotteries. Subjects are compensated for one of their randomly-chosen decisions at the end of experiment. As we will show later, we find that pre-play learning makes subjects' distributions of selling prices for lotteries consistent with their choices, which implies consistent preference rankings between choice and pricing. Thus pre-play learning removes the preference reversal.

Why does pre-play learning remove the preference reversal? It might be because pre-play learning makes a non-linear probability weight function converge toward linearity,

[63] For example, Lichtenstein and Slovic's (1971) experiment was clouded by strategic responses and subject confusion on their experimental instructions,

especially in the high probability range. Our conjecture is empirically supported by Hau et al. (2008) and van de Kuilen (2009). In their experiments, lottery learning made elicited probability weight functions converge to linearity. This line of explanation also seems probable because the majority of people have non-linear probability weight functions. Bruhin et al. (2010) found that about 80% of subjects exhibited non-linear probability weight functions while the rest of them exhibited a linear probability weight function.

2. Experimental design

Our experiment replicated Grether and Plott's (1979) experimental design and instructions except the number of lottery pairs; we used only one pair of lotteries rather than six. The lotteries used in our experiment were a low-payoff high-probability lottery with a 35/36 chance of winning $4 and a 1/36 chance of losing $1, and a high-payoff low-probability lottery with an 11/36 chance of winning $16 and a 25/36 chance of losing $1.50. We call the lotteries a p-bet and a $-bet, respectively. Similarly to Grether and Plott's experiment, in our experiment, subjects joined a choice task and two pricing tasks. In a choice task, subjects were asked to choose between two lotteries. In pricing tasks, subjects were asked to determine selling prices for each lottery. Selling prices for lotteries were elicited using the BDM mechanism.[64] At the end of experiment, one of decisions in choice and pricing tasks was randomly selected for subjects' compensation. A bingo cage was used to decide lottery outcomes. Experimental instructions are available in the Appendix.

We repeated our experiment with a simplified version of the BDM mechanism because subjects tended to be confused and may not have reported their true values (Cason and Plott, 2014). To avoid subjects' confusion on the BDM mechanism, detailed instructions

[64] In the BDM mechanism, a subject was asked to decide a selling price for a lottery, and her selling price was compared with a random price drawn between $0 and $9.99. If her selling price was lower than or equal to the random price, she sold the lottery and received the random price. If her selling price was higher than the random price, she played the lottery.

and repeated practices for the BDM mechanism are required (Plott and Zeiler, 2005). However, in our view, the original instructions of Grether and Plott (1979) on the BDM method were not sufficiently detailed to remove subjects' confusion. Thus, to improve subjects' understanding on the BDM mechanism and maintain the original experimental setting of Grether and Plott, we modified the BDM mechanism using a multiple-price-list format. This kind of elicitation method has been widely used in other studies (e.g. Kahneman et al. 1990; Butler and Loomes, 2007; Loomes et al. 2010). In our experiment, subjects were asked to answer questions of whether they would sell a lottery for varying given prices. Then one price was randomly selected, and their choice for the selected price was implemented. If they chose to sell the lottery at that price, they sold the lottery and received the price in the question. If they decided not to sell the lottery, they played the lottery. The given prices were displayed in a decreasing order from $9.99 to $0 across questions, and this price range was the same as a random price range for the BDM mechanism in our experiment. In our analysis, we used a middle point of prices where subjects switched their decisions because only price intervals were elicited from the BDM mechanism with a multiple price list format. For example, if a subject sold a lottery for $5 but did not sell for $4.50, her selling price was $4.75 in our analysis.

There were four groups of subjects in our experiment. Two groups were treatment groups with pre-play learning while the other two groups were control groups without pre-play learning. In treatment groups, subjects joined a pre-play learning exercise before making any decisions in choice and pricing tasks. In pre-play learning, we played each of lotteries ten times, and subjects kept a record of lottery outcomes.[65] We controlled possible order effects by switching orders of tasks: (pricing tasks)-(choice task) and (choice task)-

[65] Note that subjects were provided lottery information such as outcomes and probabilities prior to pre-play learning. On the contrary, in some psychology studies (e.g. Hertwig et al. 2004), subjects make decisions only relying on sampling from lotteries without specified lottery information.

(pricing tasks).[66] All sessions were conducted in a lab on a university campus in 2015. Each session lasted 30 minutes and was on paper and pencil. We recruited 167 students. Subjects were paid a show-up fee of $10 and additional earnings from their decisions. In treatment and control groups using the BDM mechanism with a multiple price list format, there were nine subjects who showed more than one switching point or who showed unusual choices of keeping a lottery at high prices and selling it at low prices. We included responses of those subjects in our analysis by using a first switching point in the case of multiple-switching points, and using a maximum price of $9.99 in the price range in the case of unusual choice pattern. We assumed that the unusual choice pattern was a kind of multiple switching points. Including those data does not change general findings in our analysis.

3. Results

We check effects of pre-play learning on subjects' preference rankings for lotteries in terms of choice and pricing in Table 1. In particular, we compare distributions of selling prices for lotteries depending on lottery choice. Panel (a) reports mean selling prices for lotteries elicited using the BDM method. Recall that the p-bet denotes a low-payoff high-probability lottery, and that the $-bet denotes a high-payoff low-probability lottery. Likewise, a p-bid denotes a selling price for the p-bet, and a $-bid denotes a selling price for the $-bet. In columns (1) and (2) with pre-play learning, subjects choosing the p-bet assign higher prices to the p-bet ($5.42) than to the $-bet ($4.04), which is weakly supported in a one-sided t-test (p-value: 0.09). On the contrary, in columns (6) and (7) without pre-play learning, subjects choosing the p-bet assign similar prices to the p-bet ($3.57) and the $-bet ($4.02), which is supported in one-sided Wilcoxon rank-sum test and t-test (p-values: 0.81 and 0.73,

[66] In detail, the orders of tasks for treatment groups were [pre-play learning for the $-bet]-(pricing task for the $-bet)-[pre-play learning for the p-bet]-(pricing task for the p-bet)-(choice task) and [pre-play learning for the p-bet and the $-bet]-(choice task)-(pricing tasks for the p-bet and the $-bet). Pre-play learning was omitted for control groups. We do not find evidence for order effects.

respectively). Thus pre-play learning seems to align preference rankings of subjects choosing the p-bet consistent between choice and pricing. This pattern becomes stronger in panel (b) where selling prices are elicited using the BDM method with a multiple price list format. In columns (1) and (2) with pre-play learning, subjects choosing the p-bet value the p-bet ($5.16) higher than the $-bet ($4.41), which is supported in one-sided Wilcoxon rank-sum test and t-test (p-values: 0.02 and 0.03, respectively). In contrast, in columns (6) and (7) without pre-play learning, subjects choosing the p-bet value similarly the p-bet ($3.94) and the $-bet ($4.26), which is supported in the two tests (p-values: 0.76 and 0.74, respectively). Collectively, those results suggest that pre-play learning makes preference rankings of subjects choosing the p-bet consistent between choice and pricing.

Next, we consider effects of pre-play learning on selling prices of subjects choosing the $-bet. Differently from subjects choosing the p-bet, even without pre-play learning, subjects choosing the $-bet show consistent preference rankings between choice and pricing. In panel (a), columns (6) and (7) without pre-play learning, subjects choosing the $-bet assign higher prices to the $-bet ($5.58) than to the p-bet ($3.60), which is supported in one-sided Wilcoxon rank-sum test and t-test (p-values: <0.01 and <0.01, respectively). This pattern carries over to columns (1) and (2) with pre-play learning; subjects choosing the $-bet assign higher prices to the $-bet ($6.44) than to the p-bet ($4.00), which is supported in the two tests (p-values: 0.03 and 0.06). Those patterns are true in panel (b). In columns (6) and (7) without pre-play learning, subjects choosing the $-bet value the $-bet ($6.78) higher than the p-bet ($3.52). In columns (1) and (2) with pre-play learning, subjects choosing the $-bet value the $-bet ($6.34) higher than the p-bet ($3.84). In sum, those results suggest that pre-play learning makes preference rankings consistent between choice and pricing, thereby virtually eliminating the preference reversal.

Another way of testing the consistency of preference rankings between choice and pricing is to compare preference rankings between choice and pricing directly at individual level. Table 2 classifies subjects in terms of their lottery choice and selling price. Selling prices are elicited using the BDM method in panel (a) and the BDM method with a multiple price list format in panel (b). In the left half of panel (a), at the first row, 18 subjects choose the p-bet and value the p-bet higher than the $-bet. 15 subjects choose the p-bet but value the $-bet higher than the p-bet. Four subjects choose the p-bet but value the p-bet and the $-bet equally. In those cases, the 18 subjects have consistent preference rankings between choice and pricing whereas the 15 and 4 subjects have inconsistent preference rankings. In a similar way, at the second row, one subject chooses the $-bet but values the p-bet higher than the $-bet. Seven subjects choose the $-bet and value the $-bet higher than the p-bet. In those cases, seven subjects have consistent rankings between choice and pricing, but one subject has inconsistent preference rankings. We see that the proportion of inconsistent preference rankings in subjects choosing the p-bet is higher than that in subjects choosing the $-bet in the left half of panel (a), which is a typical preference reversal pattern observed in other studies. We see the similar pattern in the other comparisons in the right half of panel (a) and in panel (b). We conduct a hypothesis test of whether distributions of inconsistent preference rankings are the same between subjects choosing the p-bet and those choosing the $-bet. If it is the case, the typical preference reversal pattern no longer exists, and inconsistent preference rankings may be viewed as error and noise. Note that we do not include subjects choosing an indifferent option between the p-bet and the $-bet in the test due to small number of observations. In panel (a), columns (1)-(3) with pre-play learning, inconsistent preference rankings are asymmetric between subjects choosing the p-bet and those choosing the $-bet, which is supported in a two-sided Wilcoxon rank-sum test (p-value: 0.05). In columns (4)-(6) without pre-play learning, inconsistent preference ranks are also asymmetric (p-value: 0.02).

However, those asymmetric patterns disappear with pre-play learning when selling prices are elicited using the BDM method with a multiple price list format as shown in columns (1)-(3) of panel (b), which is supported in the test (p-value: 0.27). On the contrary, in columns (4)-(6) without pre-play learning, inconsistent preference ranks are asymmetric between subjects choosing the p-bet and those choosing the $-bet (p-value<0.01). Those results seem to suggest that the typical preference reversal pattern is removed with pre-play learning when subjects have a better understanding on the BDM method.

We examine how pre-play learning changes subjects' selling prices for the p-bet and the $-bet in Table 3. Columns (1) and (2) report mean selling prices elicited by the BDM method. Pre-play learning increases selling prices for the p-bet from $3.64 to $5.25, which is supported in Wilcoxon rank-sum test and two-sample t-test (p-values: 0.01 and 0.06, respectively) as shown in column (5). Pre-play learning does not change selling prices for the $-bet; $4.42 with pre-play learning and $4.78 without pre-play learning (p-values: 0.48 and 0.52, respectively). These pre-play learning effects are also true when selling prices are elicited using the BDM method with a multiple price list. In columns (3) and (4), pre-play learning increases selling prices for the p-bet but does not change for the $-bet. Our results are consistent with Erev et al. (2010) finding that the effect of lottery learning on lottery choice is stronger when probabilities for higher payoffs in lotteries are very large or very small (in their case, larger than 0.8 or smaller than 0.2). Recall that the p-bet in our experiment has very large probability (35/36) for a higher payoff, but that the $-bet does not have very small probability (11/36) in their sense. Pre-play learning equalizes selling prices for the p-bet and the $-bet. In columns (1) with pre-play learning, selling prices are not statistically different, which is supported by Wilcoxon signed-rank test and paired sample t-test as shown at the second last row (p-values: 0.69 and 0.33, respectively). This is true in column (3) with pre-play learning (p-values: 0.95 and 0.92, respectively). On the contrary, in columns (2) and (4) without pre-play learning, selling prices are different, which are supported in the two tests. Our results without pre-play learning are consistent with Grether and Plott (1979) in that the prices for the $-bet are greater than those for the p-bet.

We check whether pre-play learning affects subjects' lottery choices between the p-bet and the $-bet. Columns (1)-(3) of Table 4 report the number of subjects choosing the p-bet or the $-bet, or an "indifferent" option between the two bets. The first two rows report subjects' choices in groups where selling prices are elicited using the BDM method. At the first row with pre-play learning, 79% of subjects choose the p-bet, and 17% of them choose the $-bet, which is different from equal choices (p-value of Wilcoxon signed-rank test<0.01). On the contrary, at the next row without pre-play learning, 47% of subjects choose the p-bet, and 51% of them choose the $-bet, which is similar to equal choices (p-value: 0.76). Those results are replicated in the next two rows. The next two rows report subjects' choices where selling prices are elicited using the BDM method with a multiple price list format. At the third row with pre-play learning, 71% of subjects choose the p-bet, and 26% of them choose the $-bet, which is different from equal choices (p-value<0.01). In contrast, at the last row without pre-play learning, 57% of subjects choose the p-bet, and 40% of them choose the $-bet (40%), which is similar to equal choices (p-value: 0.30). Thus those results suggest that pre-play learning increases the probability of choosing the p-bet over the $-bet.

4. Discussion

We have shown that pre-play learning makes subjects' distributions of selling prices for lotteries consistent with their choices, which implies consistent preference rankings between choice and pricing. Thus pre-play learning virtually removes the preference reversal. Pre-leaning has removed a typical preference reversal pattern when subjects had better understanding on the BDM method. We have also showed that pre-play learning has different effects on the p-bet and the $-bet; pre-play learning increases selling prices for the p-bet but does not change the prices for the $-bet. Those pre-play learning effects equalize selling prices for the two bets.

Why does pre-play learning remove the preference reversal? Pre-play learning might make probability weight functions converge toward linearity, especially in the high probability range. This hypothesis is empirically confirmed by Hau et al. (2008), Jessup et al. (2008) and van de Kuilen (2009). They found that lottery learning increased underweighted probabilities to their objective probabilities. This explanation seems probable because the majority of people have non-linear probability weight functions. Bruhin et al. (2010) found that around 80% of subjects had inverse S-shaped probability weight functions whereas the rest of them had linear ones. It is likely that people with non-linear probability weight functions would result in most of the preference reversal. There are theoretical explanations about the preference reversal using non-linear subjective probabilities. Rachlin et al. (1991) demonstrated how non-linear hyperbolic subjective probabilities explain the preference reversal. Bordalo et al. (2012) argued that the preference reversal occurs because people pay more attention to salient lottery outcomes. In their salience theory, asymmetric attention would lead to non-linear subjective probabilities and thus the preference reversal.

Does pre-play learning affect the level of individual risk aversion? Our answer is no because pre-play learning increases the probability of choosing the p-bet as well as selling prices for the p-bet. If pre-play learning increases the level of risk aversion, pre-play learning should have increased the probability of choosing the p-bet but decreased selling prices for the p-bet. Thus pre-play learning does not seem to change the coverture of individual utility function.

We compare pre-play learning with lottery feedback. Lottery feedback requires "just enough" lottery feedback to remove a preference reversal pattern typically observed in other studies. Braga et al. (2009) found "too much" lottery feedback backfires on the preference reversal. In other words, lottery feedback removed a typical preference reversal pattern in first few rounds but created another form of preference reversal pattern around ten rounds. On the contrary, pre-play learning does not seem to require "just enough" pre-play learning to remove a typical preference reversal pattern. Recall that lotteries were played ten times in our pre-play learning.

Our results provide testable insight to Cox and Grether (1996). In their experiment, repeating the BDM method with lottery feedback did not remove a preference reversal pattern typically observed in other studies. On the contrary, market-like elicitation modes such as second-price auction removed a typical reversal pattern with lottery feedback. Based on those results, they concluded that using market-like elicitation modes is important in removing a typical preference reversal pattern. However, in our view, subjects' weak understanding on the BDM method may also lead to their result in the BDM method. In our experiment, pre-play learning did not remove a typical preference reversal pattern when the BDM method was used. But pre-play learning removed it when the BDM method with a multiple price list format was used.

Our results are generally supported by the psychology literature called "decisions from experience." Note that this line of studies has somewhat different settings from our pre-play learning. They have an interest in how people make decisions when people do not have lottery information on outcomes and probabilities. In their experiments, subjects usually obtain lottery information through learning process such as sampling or feedback (Herwig and Erev, 2009). Results from sampling is comparable counterparts to ours although sampling does not provide lottery information.[67] Our results are consistent with Hertwig et al. (2004) and Golan and Ert (2015) using sampling. They found that sampling makes subjects underweight rare events in lottery choice and pricing, respectively. In our preference-reversal context, underweighting rare events implies increasing the probability of choosing the p-bet as well as selling prices for the p-bet.

[67] The availability of lottery information does not seem to make notable differences in lottery feedback. Lejarraga and Gonzalez (2011) found that lottery feedback has same effects regardless of providing lottery information.

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Table 1 Subjects' mean selling prices for lotteries by their lottery choice

(a) BDM method

Choice	With pre-play learning					Without pre-play learning				
	Mean p-bid (1)	Mean $-bid (2)	H_0: p-bid=$-bid H_1: p-bid>$-bid (3)	H_0: p-bid=$-bid H_1: p-bid<$-bid (4)	# of subjects (n=47) (5)	Mean p-bid (6)	Mean $-bid (7)	H_0: p-bid=$-bid H_1: p-bid>$-bid (8)	H_0: p-bid=$-bid H_1: p-bid<$-bid (9)	# of subjects (n=43) (10)
p-bet	$5.42	$4.04	0.12 / 0.09	-	37	$3.57	$4.02	0.81 / 0.73	-	20
$-bet	$4.00	$6.44	-	0.03 / 0.06	8	$3.60	$5.58	-	<0.01 / <0.01	22
Indif.	$7.00	$3.50	-	-	2	$6.00	$2.00	-	-	1

Note: P-values of Wilcoxon signed rank test / t-test are reported. The p-bet denotes a high-payoff low-probability lottery while the $-bet denotes a low-payoff high-probability lottery. The p-bid and the $-bid denote selling prices for the corresponding lotteries.

(b) BDM method with a multiple price list format

Choice	With pre-play learning					Without pre-play learning				
	Mean p-bid (1)	Mean $-bid (2)	H_0: p-bid=$-bid H_1: p-bid>$-bid (3)	H_0: p-bid=$-bid H_1: p-bid<$-bid (4)	# of subjects (n=42) (5)	Mean p-bid (6)	Mean $-bid (7)	H_0: p-bid=$-bid H_1: p-bid>$-bid (8)	H_0: p-bid=$-bid H_1: p-bid<$-bid (9)	# of subjects (n=35) (10)
p-bet	$5.16	$4.41	0.02 / 0.03	-	30	$3.94	$4.26	0.76 / 0.74	-	20
$-bet	$3.84	$6.34	-	0.01 / <0.01	11	$3.52	$6.78	-	<0.01 / <0.01	14
Indif.	$4.25	$6.25	-	-	1	$3.75	$6.25	-	-	1

Table 2 Frequencies of subjects by their lottery choice and selling price

(a) BDM method

	Higher selling price: With pre-play learning (n=47)			Higher selling price: Without pre-play learning (n=43)		
Choice	p-bet (1)	$-bet (2)	Equal (3)	p-bet (4)	$-bet (5)	Equal (6)
p-bet	18	**15**	**4**	8	**12**	-
$-bet	**1**	7	-	**5**	17	-
Indifferent	**2**	-	-	**1**	-	-
P-value of Wilcoxon rank-sum test	0.05			0.02		

Note: The number of subjects having inconsistent preference rankings are in bold. A null hypothesis is that inconsistent preference rankings have same distributions between subjects choosing the p-bet and those choosing the $-bet. In the test, subjects choosing an indifferent option between the p-bet and the $-bet are not included.

(b) BDM method with a multiple price list format

	Higher selling price: With pre-play learning (n=42)			Higher selling price: Without pre-play learning (n=35)		
Choice	p-bet (1)	$-bet (2)	Equal (3)	p-bet (4)	$-bet (5)	Equal (6)
p-bet	19	**7**	**4**	7	**9**	**4**
$-bet	**1**	9	**1**	**2**	12	-
Indifferent	-	**1**	-	-	**1**	-
P-value of Wilcoxon rank-sum test	0.27			<0.01		

Table 3 Mean selling prices for lotteries

	BDM method		BDM method with a multiple price list format		P-values of Wilcoxon rank-sum test / two-sample t-test for equality of p-bids ($-bids) between pre-play learning and no pre-play learning for…	
	Pre-play learning (1)	No pre-play learning (2)	Pre-play learning (3)	No pre-play learning (4)	columns (1) & (2) (5)	columns (3) & (4) (6)
p-bid	$5.25	$3.64	$4.80	$3.76	0.01 / 0.06	0.03 / 0.04
$-bid	$4.42	$4.78	$4.74	$5.34	0.48 / 0.52	0.44 / 0.39
P-values of Wilcoxon signed-rank test / paired sample t-test for p-bid=$-bid	0.69 / 0.33	0.03 / 0.04	0.95 / 0.92	<0.01 / <0.01	-	-
# of subjects	47	43	42	35	-	-

Table 4 Frequencies of lottery choices

		Choosing p-bet (1)	Choosing $-bet (2)	Indifferent (3)	# of subjects (4)	P-values of Wilcoxon signed-rank test for equality of proportions choosing p-bet and $-bet (5)
BDM method	Pre-play learning	37 (79%)	8 (17%)	2 (4%)	47 (100%)	<0.01
	No pre-play learning	20 (47%)	22 (51%)	1 (2%)	43 (100%)	0.76
BDM method with a multiple price list format	Pre-play learning	30 (71%)	11 (26%)	1 (2%)	42 (100%)	<0.01
	No pre-play learning	20 (57%)	14 (40%)	1 (3%)	35 (100%)	0.30

Appendix: Experimental instructions based on Grether and Plott (1979)

Study Title: Economic Valuation

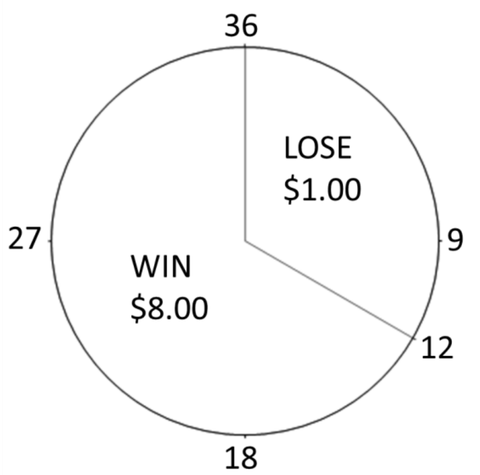
This is a research study about economic valuation. You MUST be at least 18 years old to participate. This lab session is completely anonymous and will take approximately 25 minutes to complete. You will be compensated $10 for your participation. Your final compensation may vary depending on your decisions made on study tasks. If you have questions during the session, please raise your hand and the facilitator will assist you. Please do not talk with other participants or use your smartphone during the session. Please do not go to subsequent pages of the packet until the facilitator asks you to do so.

Please find the Informed Consent document in front of you and sign at the bottom of the second page.

Please do NOT go to subsequent pages of the packet until the facilitator asks you to do so.

Instructions

We are trying to determine how people make decisions. We have designed a simple choice experiment and will ask you to make one decision in each of three items. Each decision you make will involve one or two *bets*. If a bet is played, then one ball will be drawn from a bingo cage that contains 36 balls numbered 1 to 36. Depending upon the nature of the bet, the number drawn will determine whether you lose an amount of money or win an amount of money. The figure below is an example of the type of bets used in the experiment. In the example, if you play the following bet, then you will lose $1 if the number drawn is less than *or equal to* 12, and you will win $8 if the number drawn is greater than 12.



You will be paid in the following fashion. We will first give you $10. After you have made a decision on each item, one item will be chosen at random by drawing a ball from a bingo cage. The bet(s) in the chosen item will then be played. You will be paid an amount depending upon your decisions and upon the outcomes of the bets in the chosen item—any amount you win will be added to the $10, and any amount you lose will be subtracted from the $10. However, the most you can lose on a bet is $1.50, so you will receive at least $8.50. All actual payments will occur after the experiment.

If you have questions, please raise your hand.

Please do NOT go to the next page until the facilitator asks you to do so.

PART 1

For each of the items below, you have been presented a ticket that allows you to play a bet. You will then be asked for the *smallest* price at which you would sell the ticket to the bet.

If an item from this part is chosen at the end of the experiment, we will do the following. First, a bingo cage will be filled with 10 balls numbered 0 to 9. Then 3 balls will be drawn from this cage, with each ball being replaced before the next is drawn. The numbers on these 3 balls will determine the digits of an offer price between $0.00 and $9.99, with the first number being the penny (right) digit, the second number the dime (middle) digit, and the third number the dollar (left) digit. If this offer price is greater than or equal to your minimum selling price for the item's bet, you would receive the offer price. If the offer price is less than your selling price, you would play the bet and be paid according to its outcome.

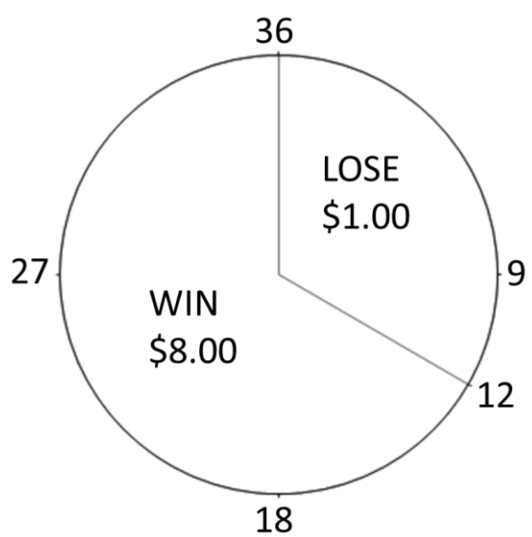
It is in your best interest to be accurate; that is, the best thing you can do is to be honest. If the price you state is too high or too low, then you are passing up opportunities that you prefer. For example, suppose you would be willing to sell the bet for $4 but instead you say that the lowest price you will sell it for is $6. If the offer price drawn at random is between the two (for example $5), you would be forced to play the bet even though you would rather have sold it for $5. Suppose that you would sell it for $4 but not for less, and that you state that you would sell it for $2. If the offer price drawn at random is between the two (for example $3) you would be forced to sell the bet even though at that price you would prefer to play it.

If you have questions, please raise your hand. You will have three practice tasks soon.

Please do NOT go to the next page until the facilitator asks you to do so.

Practice Task 1

What is the *smallest* price for which you would sell a ticket to the following bet? $________



Please wait for other participants to finish their decisions.

Suppose that the offer price is $___ . ___ ___.

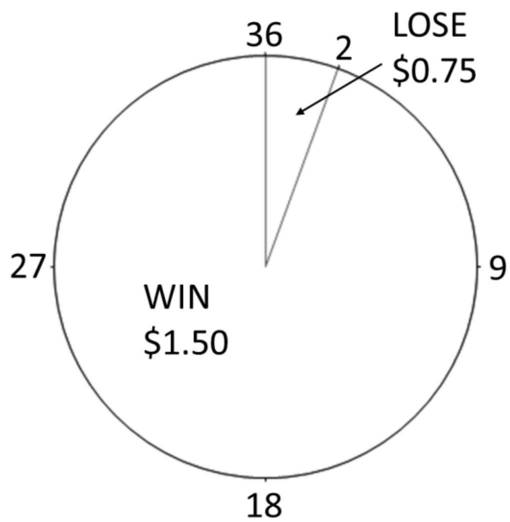
Please fill in ONLY ONE SIDE in the table below.

i) If the offer price is greater than or equal to your selling price, you receive the offer price.	ii) If the offer price is less than your selling price, you play the bet.
I get: $	**The number drawn:**
	I get / lose: (Circle one) $

Please do NOT go to the next page until the facilitator asks you to do so.

Practice Task 2

What is the *smallest* price for which you would sell a ticket to the following bet? $_______



Please wait for other participants to finish their decisions.

Suppose that the offer price is $___ . ___ ___.

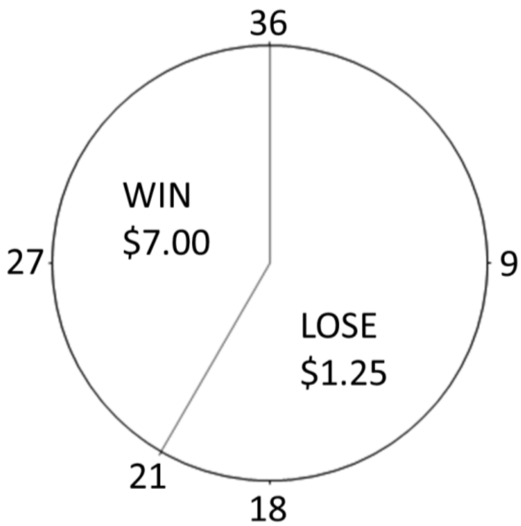
Please fill in ONLY ONE SIDE in the table below.

i) If the offer price is greater than or equal to your selling price, you receive the offer price.	ii) If the offer price is less than your selling price, you play the bet.
I get: $	**The number drawn:**
	I get / lose: (Circle one) $

Please do NOT go to the next page until the facilitator asks you to do so.

Practice Task 3

What is the *smallest* price for which you would sell a ticket to the following bet? $_______



Please wait for other participants to finish their decisions.

Suppose that the offer price is $___ . ___ ___.

Please fill in ONLY ONE SIDE in the table below.

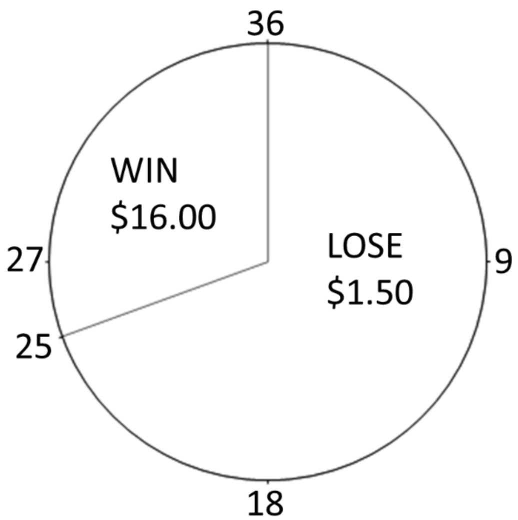
i) If the offer price is greater than or equal to your selling price, you receive the offer price.	ii) If the offer price is less than your selling price, you play the bet.
I get: $	**The number drawn:**
	I get / lose: (Circle one) $

All practice tasks are over. We will give you two items that may influence your compensation.

Please do NOT go to the next page until the facilitator asks you to do so.

Item 1

Consider carefully the following bet shown below:



To give you a sense of outcomes from the bet, we will draw a ball from a bingo cage ten times. Please keep records of the numbers drawn and circle the corresponding money outcome in the table below.

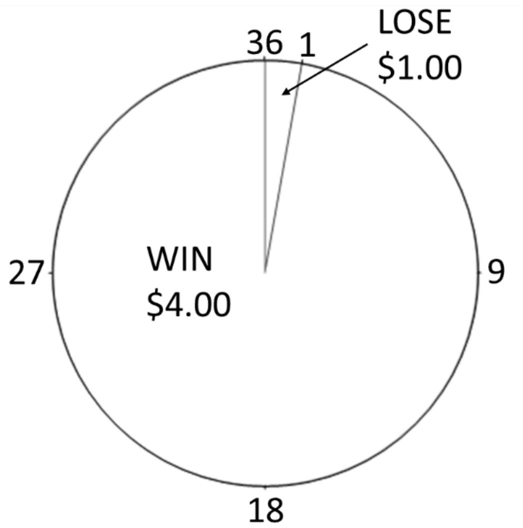
Trial	Write the number drawn	Circle the corresponding money outcome
1		Win $16 / Lose $1.50
2		Win $16 / Lose $1.50
3		Win $16 / Lose $1.50
4		Win $16 / Lose $1.50
5		Win $16 / Lose $1.50
6		Win $16 / Lose $1.50
7		Win $16 / Lose $1.50
8		Win $16 / Lose $1.50
9		Win $16 / Lose $1.50
10		Win $16 / Lose $1.50

Please answer the following question:
What is the *smallest* price for which you would sell a ticket to the bet? $_______

Please do NOT go to the next page until the facilitator asks you to do so.

Item 2

Consider carefully the following bet shown below:



To give you a sense of outcomes from the bet, we will draw a ball from a bingo cage ten times. Please keep records of the numbers drawn and circle the corresponding money outcome in the table below.

Trial	Write the number drawn	Circle the corresponding money outcome
1		Win $4 / Lose $1
2		Win $4 / Lose $1
3		Win $4 / Lose $1
4		Win $4 / Lose $1
5		Win $4 / Lose $1
6		Win $4 / Lose $1
7		Win $4 / Lose $1
8		Win $4 / Lose $1
9		Win $4 / Lose $1
10		Win $4 / Lose $1

Please answer the following question:
What is the *smallest* price for which you would sell a ticket to the bet? $_______

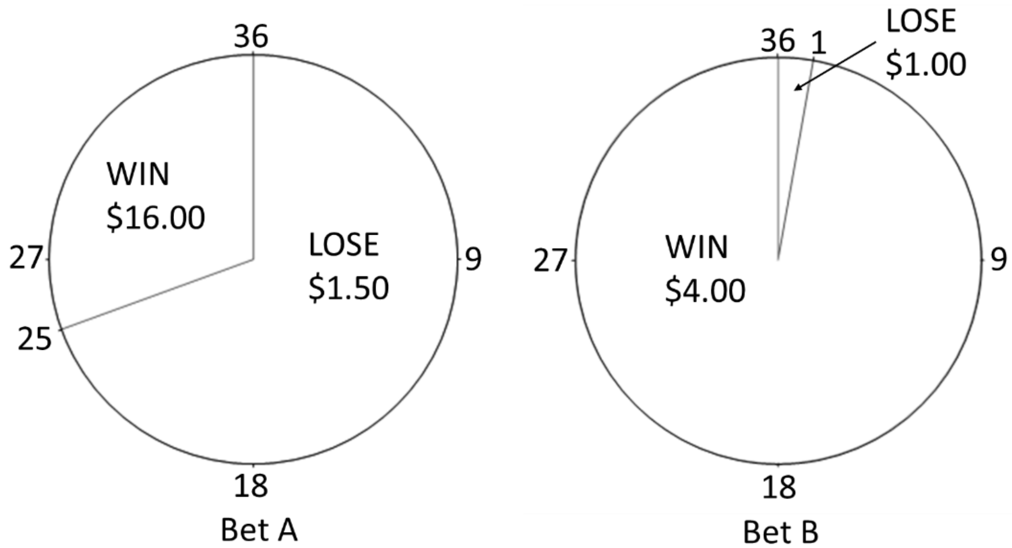
Please do NOT go to the next page until the facilitator asks you to do so.

PART 2

If an item from this part is chosen at the end of the experiment, you will play the bet you select. If you check "Don't care," the bet you play will be determined by a coin toss.

Item 3

Consider carefully the following two bets shown below.



Suppose you have the opportunity to play one of these bets. Make one check below to indicate which bet you would prefer to play:

Bet A	Bet B	Don't care
☐	☐	☐

Please do NOT go to the next page until the facilitator asks you to do so.

Your Earnings

You will calculate your earnings. The facilitator will randomly draw one ball. The number on the ball will decide which item would be considered for your compensation.

The item number drawn: [ ]

1) If the item number is 1 or 2, the facilitator will draw a ball three times to decide the offer price.

The offer price: **$___.___ ___**

Please fill in ONLY ONE SIDE in the table below.

i) If the offer price is greater than or equal to your selling price, you receive the offer price.	ii) If the offer price is less than your selling price, you play the bet.
I get: **$**	**The number drawn:** [ ]
	I get / lose: **(Circle one)** $

2) If the item number is 3, please fill in ONLY ONE SIDE in the table below.

i) If you have chosen bet A in item 1,	ii) If you have chosen bet B in item 1,	iii) If you have chosen "Don't care" in item 1,
The number drawn: [ ]	**The number drawn:** [ ]	**The coin flip:** **(Circle one)** Head / Tail
I get / lose: **(Circle one)** $	**I get / lose:** **(Circle one)** $	Heads mean bet A, and tails mean bet B.
		The number drawn: [ ]
		I get / lose: **(Circle one)** $

My total earnings: $10 plus or minus $_______ = $________
(Circle one)

Please go to the next page.

Please answer the following questions:

A. What is your gender? [Male □ / Female □]

B. What is your age? [   ]

C. What year are you in school?

[Freshman □ / Sophomore □ / Junior □ / Senior □ / Graduate □ / Other □]

Please find a receipt on the next page and fill it out using the total earnings you calculated on the previous page. Then detach it from this packet to keep your responses anonymous. Please submit the receipt and all decision sheets when you receive your compensation. You will receive a debriefing form for this study when you leave the lab.

CHAPTER 6. DIRECTIONS OF FUTURE RESEARCH

My current papers are stepping stones for my future research. I have plans to extend the current studies. First, I would like to explore dynamic patterns of effects of agglomeration factors on rural economy. Because agglomeration economies are related to knowledge spillover and lower transportation costs, changes in information and transportation technologies may influence the sizes of effects of agglomeration factors. Understanding those patterns would be useful to governments because it can improve the efficiency of regional development policies.

Second, I would like to check how agglomeration influences population migration. People tend to follow jobs, and jobs tend to be created in areas with higher agglomeration economies. I expect that agglomeration economies would be an important factor in migration decisions. It would also be interesting to compare sizes of effects between agglomeration factors and other factors such as amenity.

Third, I would like to see how behavioral factors such as risk and time preferences affect entrepreneurial decisions. In particular, I would compare behavioral effects between urban and rural areas. Understanding of who become entrepreneurs in rural areas would be meaningful to regional development policies.

Lastly, I would like to study how framing influences health decisions such as vaccination. For example, under-vaccination for the flu is a concern in the U.S. Some studies found that insurance framing for lottery choices increases more risk-averse choices. Note that getting vaccinated is a more risk averse choice. My research question is whether insurance framing makes more people vaccinated, which is a testable hypothesis.