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CONTRACT, DURATION AND DISCRIMINATION

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Contract, Duration and Discrimination

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Abstract

Racial residential segregation is a crucial aspect of the persisting racial inequality in the United States. We reexamine this enduring problem from a novel perspective, exposing the relationship between segregation and contract duration. In the housing context, the main contract duration decision involves the choice between buying (long duration) and renting (short duration). And this choice can affect, and be affected by, the racial composition of a neighborhood. If, because of discriminatory misperceptions based on mistaken stereotypes or discriminatory preferences, moving into a racially diverse neighborhood is perceived by some white residents to be a riskier or otherwise less preferred alternative, then (i) a white person moving into such a diverse neighborhood would be more likely to rent than buy; and (ii) a white person who is intent on buying, would be likely to choose a less diverse, predominantly white neighborhood. To empirically explore the relationship between contract duration and segregation, we apply two methodological approaches: First, we analyze rich survey data collected by the Bureau of Labor Statistics, which cover 8,984 individuals who were surveyed annually over a period of 17 years, including about their housing decisions. Second, we run online, incentivized trust-game experiments (N=763 across all experiments), where we study the relationship between duration choices and partner choices. Our findings suggest that short-duration, rental contracts may help reduce discriminatory outcomes. The shorter duration and the lower perceived risk of renting may encourage white residents to move into more diverse neighborhoods. And renting in a more diverse neighborhood may help dispel discriminatory misperceptions that are based on mistaken stereotypes or even eradicate discriminatory preferences, such that when the time comes to buy a house (long-duration contract) the search will include more diverse neighborhoods. If short-duration, rental contracts can be more conducive to racial integration, this provides a reason to soften the strong policy preference for homeownership. We also briefly explore the relationship between contract duration and other contractual design choices beyond the housing context.

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Introduction

Racial residential segregation is a crucial aspect of the persisting racial inequality in the United States.[1] A Large body of literature has identified and quantified the patterns of residential segregation, explored its causes and studied its negative implications.[2] In this Article, we reexamine this enduring problem from a novel perspective, exposing the relationship between segregation and contract duration. Moreover, by highlighting the role of contract duration, we are able to formulate new policy proposals to help mitigate the problem of residential segregation.

Contract duration, understood as the length of the contract period or the staying power of the contract's impact, is of obvious importance in many contexts. Longer duration often entails greater risk, thus motivating parties to compensate by reducing other dimensions of (perceived) risk. Consider an employee who is choosing between employer A who is known to be okay and employer B who may turn out to be either great or terrible (the employee simply has less information about employer B). If the employment contracts that A and B are offering are short-term, e.g., because the employee can leave at any time, then the employee may be willing to take the risk and go to work for employer B. But if the employment contracts that A and B are long-term, e.g., because switching jobs is costly, the employee would be more likely to choose the low-risk employer A. Next, considering the reverse causal mechanism, when a contract is perceived to be riskier, parties would be expected to choose a shorter contractual duration. If the employee can choose the duration of the employment relationship in advance, she will be more willing to make a long-term commitment to the low-risk employer A, and quite reluctant to make such a commitment to the risky employer B.

Turning to housing decisions, which are the focus of this Article, the main contract duration question involves the choice between buying (long duration) and renting (short duration).[3] And

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[1] *See*, *e.g.*, Patrick Sharkey, STUCK IN PLACE: URBAN NEIGHBORHOODS AND THE END OF PROGRESS TOWARD RACIAL EQUALITY (2013).

[2] For a review of this literature, *see* Camille Zubrinsky Charles, *The Dynamics of Racial Residential Segregation*, 29 ANN. REV. SOCIO. 167 (2003); John R. Logan, Brian J. Stults & Reynolds Farley, *Segregation of Minorities in the Metropolis: Two Decades of Change*, 41 DEMO. 1 (2004); Jeffrey M. Timberlake, *Separate, But How Unequal? Ethnic Residential Stratification, 1980 to 1990*, 1 CITY & CMTY 251 (2002); Yu Xie & Xiang Zhou, *Modeling Individual-Level Heterogeneity in Racial Residential Segregation*, 109 PROC.NAT'L ACA. SCI. 11646, 11646-11651 (2012); Susan Athey, Billy Ferguson, Matthew Gentzkowand & Tobias Schmidt, *Estimating Experienced Racial Segregation in US Cities Using Large-Scale GPS Data*, 118 PROC.NAT'L ACA. SCI. 251 (2021); David M Cutler & Edward L. Glaeser, *Are Ghettos Good or Bad?*, 112 Q. J. ECON. 827 (1997); Raj Chetty & Nathaniel Hendren, *The Impacts of Neighborhoods on Intergenerational Mobility I: Childhood Exposure Effects*, 133 Q. J. ECON. 1107 (2018); Bryan Leonard & Steven M. Smith, *Individualistic Culture Increases Economic Mobility in the United States*, 118 PROC. ACAD. SCI. 1 (2021); Raj Chetty, Nathaniel Hendren, & Lawrence F. Katz, *The Effects of Exposure to Better Neighborhoods on Children: New Evidence From the Moving to Opportunity Experiment*, 106 Am. Econ. Rev. 855 (2016); Bischoff, Kendra & Sean F. Reardon, *Residential Segregation by Income, 1970–2009*, In DIVERSITY AND DISPARITIES: AMERICA ENTERS A NEW CENTURY 208-234 (2014).

[3] There are, of course, other important duration-related choices, including the length of the rental period. Also, in some cases, a home can be purchased with an intent to resell within a short period of time. Another housing-related contracting decision concerns mortgage financing. In the United States, home buyers can choose between a long-term, e.g., 30-year, mortgage and shorter-term mortgages. (The effective duration is also determined by the magnitude of the prepayment penalty, such that a nominally long-term mortgage can be effectively short-term if the prepayment penalty is low.) In the mortgage context, a major risk factor is the interest rate. Borrowers can choose between riskier

this choice can affect, and be affected by, the racial composition of a neighborhood. If because of discriminatory misperceptions or discriminatory preferences moving into a racially diverse neighborhood is perceived by some white residents to be a riskier or otherwise less preferred alternative, then (i) a white person moving into such a diverse neighborhood would be more likely to rent than buy; and (ii) a white person who is intent on buying, would be likely to choose a less diverse, predominantly white neighborhood.[4] If such a relationship between contract duration and neighborhood segregation holds true, then short-duration, rental contracts may help reduce discriminatory outcomes. The short duration and the lower perceived risk of renting may encourage white residents to move into more diverse neighborhoods. Moreover, renting in a more diverse neighborhood may help dispel discriminatory misperceptions that are based on mistaken stereotypes (under information-based theories of discrimination) or even eradicate discriminatory preferences (under taste-based theories of discrimination), such that when the time comes to buy a house (long-duration contract) the search will include more diverse neighborhoods. If short-duration, rental contracts can reduce race-based segregation, then perhaps housing policy should do more to encourage renting (more on this below).

To empirically explore the relationship between contract duration and segregation, we apply two methodological approaches: First, we analyze rich survey data collected on 8,984 men and women born during the years 1980 through 1984 and living in the United States at the time of the initial survey in 1997. The data we use was collected annually over a period of 17 years by the Bureau of Labor Statistics, and it includes information on rent v. buy decisions as well as demographic and socio-economic information. Second, we run online, incentivized experiments, using a nationally representative Qualtrics sample (N=763 across four distinct experiments). We emphasize that, in the online experiments, we do not attempt to replicate housing decisions or to test how race affects perceived risk. These experiments explore the relationship between duration, (perceived) risk and learning and provide evidence of causality in a different setting that is more susceptible to experimental manipulation. Accordingly, the relationship between the two empirical parts of the Article—the survey data part and the online experiments part—should be understood at this higher level of generality: studying the relationship between duration, (perceived) risk and learning.

Survey data. We analyze survey data and document correlations between contract duration and neighborhood racial segregation in the United States. In particular, we test whether discriminatory tastes or beliefs exert greater influence in long-term contracts as compared to short term contracts. We predict that people will discriminate more in long-term contracts. We first look at respondents'

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variable-rate mortgages and less-risky fixed-rate mortgages. A borrower who takes a long-term mortgage would be more likely to choose the fixed-rate option, and a borrower who takes a short-term mortgage would be more likely to choose the adjustable-rate option.

[4] Part I provides a brief introduction to different theories of discrimination. We note a possible countervailing effect: A person who purchases a house in a diverse neighborhood and thus makes a long-term commitment to the neighborhood will invest more time and effort to better-integrate into the neighborhood. Contract duration can be related to other risk dimensions that are unrelated to race. For example, we would expect to see more long-term contracts in stable neighborhoods, as compared to neighborhoods that are changing/gentrifying. Our focus is on a buyer or lessee who will live in a more- or less-integrated neighborhood for a long v. short period of time. We are not focusing on the seller or lessor. For a seller, the sale transaction is a short-term contract in the relevant sense, since the seller will have no further interaction with the buyer or with the neighborhood. In contrast, a lessor will have a continuing relationship with the lessee and with the neighborhood. This alternative, seller/lessor-side perspective is discussed in Sec. II.D.

initial residential contract choices. Starting with descriptive statistics, we find that white respondents who buy (long-term contract) choose counties with a higher percentage of white residents than white respondents who rent (short-term contract). Since the buy-or-rent decision and the location decision are made simultaneously, causation is difficult to discern. Still, regression analysis shows that white respondents are more likely to buy, rather than rent, when the percentage of white residents in the county is higher; and that white respondents are more likely to choose a whiter, less diverse, location, when they are buying as compared to renting.[5] However, these correlations do not establish the role, if any, of contract duration in this relationship. The people who buy and the people who rent may differ in ways that are not observed in our data but are correlated with the decision to live in more or less integrated neighborhoods. For example, homeowners may be more conservative and renters may be more progressive. The negative correlation between owning and the percentage of white residents in the area could then simply reflect the differences between the population of owners and the population of renters.

We begin to address these selection issues by looking beyond the first housing decision and using fixed-effects regressions to perform a within-person analysis. This allows us to test whether the same person—e.g., a conservative or a progressive—makes different rent vs. buy decisions depending on neighborhood diversity, and whether the same person chooses a more or less diverse location when buying as compared to renting. In these regressions, we confirm that white respondents are more likely to choose a whiter, less diverse, location, when they are buying as compared to renting. However, we are unable to confirm that white respondents are more likely to buy, rather than rent, when the percentage of white residents in the county is larger, although this may be due to the limited statistical power of the fixed-effects regressions.[6]

Next, using the fixed-effects regression models, we follow respondents over time and test for the effects of past experiences in more or less diverse locations on their current location choices. We find that respondents with a history of living in whiter, less diverse places are more likely to move into whiter, less diverse neighborhoods—a finding that suggests a learning effect. (When focusing on the buy-or-rent decision as the dependent variable, we do not find any statistically significant effect of the percentage of white residents in the county into which the respondent moved or of the average percentage of white residents across the counties where the respondent previously lived.) Finally, to further test for learning effects, we focus on respondents' first home-buying decision, and show that respondents with past renting experiences in whiter, less diverse locations are more likely to purchase their first home in whiter, less diverse locations; and that respondents with past renting experiences in more diverse locations are more likely to purchase their first home in more diverse locations.[7]

These results are consistent with the hypothesis that some white individuals associate greater racial diversity with higher risk and thus choose a shorter-duration, rental contract, when relocating into more diverse neighborhoods. The results are also consistent with the hypothesis that, because some

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[5] *See infra* Sec. II.A.

[6] *See infra* Sec. II.B. Even the result that white respondents are more likely to choose a whiter, less diverse, location, when they are buying as compared to renting is significant only at the 10% level. Note that using fixed effects regression models significantly decreases the degrees of freedom and thus leads to reduced statistical power.

[7] *See infra* Sec. II.B. (fixed-effects regressions) and Sec. II.C. (testing for learning effects by studying respondents' first home-buying decision).

white individuals associate greater racial diversity with higher risk, when they choose a longer-duration, purchase contract, i.e., when they buy a home, they tend to do so in less diverse neighborhoods. In addition, these results are consistent with the learning hypothesis, namely, that past experience, usually past rental experience, in more diverse neighborhoods dispels, for some individuals, the association between greater racial diversity and higher risk, thus leading these individuals to buy homes (long-term contracts) in more diverse neighborhoods.[8]

Note that we observe statistically significant correlations between contract duration and racial diversity only for the white respondents in our sample. This rules out some of the alternative explanations for the correlations we observe. For example, if the correlations were driven by house prices, the investment value of homeownership, the supply of housing units for sale v. rentals, school quality or by neighborhood characteristics that are correlated with the neighborhood's racial composition, we would expect to observe similar trends for both white and non-white respondents in our sample.[9]

Online experiments. We view the analysis of the survey data as suggestive in interesting and important ways, but it cannot establish causal links between contract duration and other contracting choices. Therefore, in an attempt to tease out causation, we turn to the second methodology. We use a set of trust-game experiments to study the relationship between contract duration, captured by the number of game rounds, and the characteristics of the partners with whom the participant will play the trust game. Specifically, potential partners vary by age, gender, marital status and the state where they grew up; and the presumption, which is supported by prior work, is that partners who are less similar to the participant are perceived as less trustworthy and thus "riskier."[10] We recognize, of course, that choosing partners in a trust game is not the same as choosing where to live. Still, we believe that our experiments shed some light on the causal relationships between contract duration and other aspects of the contract that can be perceived as more or less risky.

The first experiment (Experiment 1) evaluates whether exogenously manipulated duration affects participants' choice of contracting partners. We find that when duration is longer, participants tend to choose partners who are more similar to them.[11] The second experiment (Experiment 2) tests whether causation operates also in the opposite direction, evaluating whether exogenously manipulated partner characteristics affect participants' choice of contracting duration. We find that participants who are assigned less-similar partners choose to play a smaller number of rounds. The third experiment (Experiment 3) focuses on learning effects. We were hoping to find that short-term (contractual) interactions with a less-similar partner would allow participants to learn that less-similar partners are trustworthy, i.e., behave cooperatively in the trust game, leading these participants to then enter into further (contractual) interactions with the same or another less-similar partner. Our data is not consistent with this optimistic story. But it is consistent with a

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[8] The observed correlations can also be attributed to a related psychological mechanism: People might perceive their place of residence as more reflective of their identity when they own a home, as compared to when they rent a home. *See* Sec. II.D.

[9] These alternative explanations are discussed in Sec. II.E.

[10] *See*, *e.g.*, Bruno Abrahao et al., *Reputation Offsets Trust Judgments Based on Social Biases Among Airbnb Users*, 114 PNAS 9848 (2017). Following Abrahao et al (and most other recent work in this area), we do not include race as one of the partner characteristics, since subjects would then infer that the experiment is about race.

[11] We ran two versions of Experiment 1 (*see* Part III). Therefore, together with Experiment 2 and Experiment 3 (described below), we have four experiments.

different, though still optimistic, story: Once a person interacts with a less-similar partner, he or she will be more likely to interact with less-similar partners in the future, regardless of how trustworthy the first less-similar partner was.

Policy Implications. These findings suggest the need to reevaluate a main tenant of US housing policy. For decades, the idea of homeownership has been a central aspect of the American dream.[12] Accordingly, policymakers have sought to encourage homeownership through tax incentives (including the mortgage-interest tax deduction), by supporting the mortgage credit market (e.g., by insuring lenders against the risk of borrower default and through the work of the government-sponsored enterprises, Fannie Mae and Freddie Mac), by providing flexible, block grants to state and local governments that can be used to help homebuyers, and more. Our findings suggest, albeit tentatively, that short-term, rental contracts can be more conducive to racial integration, thus providing a reason to soften the strong policy preference for homeownership. Our findings also support innovative rent-to-buy policies that encourage renting initially and support a transition to homeownership after a minimal rental period. Such policies rely on an optimistic version of the learning effect that we identified, namely, that households who start as renters will be more likely to rent in diverse neighborhoods, realize that diversity is not a cost but rather a benefit, and then when they transition to homeownership, these households will be more likely to buy in a diverse neighborhood. A rent-to-buy approach could be implemented by retaining many of the existing programs that support homeownership but making eligibility for these programs conditional on a prior period of renting. A rent-to-buy policy could also be implemented by providing rental assistance for an initial period and then allowing for easy transition into a homeownership support program.

To be clear: We acknowledge the benefits of homeownership,[13] and we are not arguing against policies that promote homeownership. Still, our findings identify a relative cost of homeownership, and a corresponding relative benefit of renting, in terms of promoting racial integration. This relative cost, and corresponding relative benefit, should, on the margin, shift some of the resources that are currently dedicated to promoting homeownership to supporting renters, at least for an initial rental period.

Beyond housing. The question of duration is paramount in the world of contract, also beyond the housing context. In the employment context, do I make a long-term commitment (with for-cause termination or even tenure, as in universities) or opt for short-term, at will employment? In a joint venture, should the partners commit to long-term cooperation or settle for only a short-term commitment? Should I license my intellectual property rights for two years or five years? What is

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[12] *See*, *e.g.*, Carolina Reid, *To Buy or Not to Buy? Understanding Tenure Preferences and the Decision-Making Processes of Lower-Income Households*, In HOMEOWNERSHIP BUILT TO LAST: LESSONS FROM THE HOUSING CRISIS ON SUSTAINING HOMEOWNERSHIP FOR LOW-INCOME AND MINORITY FAMILIES 143-171 (Eric S. Belsky, Christopher E. Herbert, & Jennifer H. Molinsky, eds., 2014) (Homeownership has become an "aspirational" goal in American culture—something viewed as a requirement for achievement of the "American dream"); Rachel B. Drew, *Believing in Homeownership: Behavioral Drivers of Housing Tenure Decisions* (JCHS Working Paper, W14-3, 2014) (calls homeownership "a social practice viewed as being as central to American life as voting."); ANAT BRACHA & JULIAN C. JAMISON, SHIFTING CONFIDENCE IN HOMEOWNERSHIP: THE GREAT RECESSION (2012) (the housing crisis during the 2008 recession did little to hamper the cultural aspiration towards homeownership).

[13] On the benefits of homeownership, for homeowners and for communities, *see* infra notes 76-77 and accompanying text.

the optimal duration of a treaty—the contractual underpinning of significant parts of international law? What about the duration of personal, intimate relationships? Some of the insights that we develop in the context of housing contracts can be generalized and applied more broadly—in these other contractual domains. Specifically, if longer duration increases risk (or perceived risk), parties entering long-duration contracts would be expected to try and reduce other dimensions of contractual risk. And, if a contract is riskier (or perceived to be riskier), parties would be expected to set a shorter duration. Also, if the parties expect to learn significant new information during a contract's initial periods, they would be more likely to choose a short-duration contract that would allow them to act on this new information. We leave it to future work to study these effects, and their policy implications, beyond the housing context.

The Article proceeds as follows. Part I elaborates on the theoretical links between duration, risk, learning and discrimination. Part II contains our analysis of the survey data. Part III reports the results from our trust-game experiments. Policy implications are discussed in Part IV. And Part V concludes.

I. Theory

Part I lays out the theoretical foundations for the empirical investigation that we undertake in Parts II and III. We begin, in Section A, with a few definitional and conceptual observations about contractual duration. Then, in Section B, we derive the basic link between risk and duration. Section C lays out the relationship between risk, learning and duration. And Section D adds diversification to the risk-duration connection. Finally, Section E discusses different theories of discrimination and how they relate to notions of perceived risk and similarity vs. difference and to contracting choices.

A. Defining Duration

As noted above, we view contract duration broadly, covering both the length of the contract period and the staying power of the contract's impact. Consider the housing example. In rental contracts, the contract period and the duration of the contract's impact (roughly) coincide. Not so with a house purchase contract, where the contract period may be short—shorter than in most rental contracts, but the contract's impact is long-term—longer than the impact of most rental contracts. When the contract period and the duration of the contract's impact diverge, we focus on impact. This follows from our interest in exploring the relationship between duration and risk, and risk is a function of impact.

Another clarification about duration is in order: Long-term contracts do not create immutable commitments. Contracts can be broken. The standard remedy, at least under US law, is damages, not specific performance (although specific performance is more common in real-estate contracts).[14] Also, long-term contracts can be renegotiated. In addition, long-term impacts can

[14] *See*, *e.g.*, Restatement (Second) of Contracts § 359 (inadequacy of money damages as a condition for specific performance); *Curtice Brothers Co. v. Catts*, 72 N.J.Eq. 831 (1907) (in contracts for the sale of land equitable relief, i.e., specific performance will generally be granted, since money damages are more difficult to ascertain).

sometimes be avoided. For example, a house that was purchased can be put on the market and resold. And yet the choice between short-term and long-term contracts matters. The cost of breaching the long-term contract—the damages that would need to be paid—can be substantial. The terms of any renegotiated contract will be affected by the baseline, long-term contract. And the transaction costs of selling a house and moving to another neighborhood can be high.[15]

B. Risk and Duration

Longer duration magnifies risk. Consider a decision to move into a certain neighborhood. The benefit from living in the neighborhood is uncertain, because of imperfect information about noise levels, school quality, availability and reliability of public transportation, etc. For example, the per-year benefit could be either 15 with a high-benefit realization or 5 with a low-benefit realization, each with a probability of 50%. Using payoff variance, a common measure of risk, a one-year rental contract entails a risk of: $v^R = \frac{1}{2}[(15-10)^2 + (5-10)^2] = 25$, namely, we start with the difference between each possible payoff (15 and 5) and the average payoff (10) and find the average of these differences squared. Or, if we use the standard deviation, another common measure of risk: $\sigma^R = \sqrt{v^R} = 5$. What would be the risk of a longer duration contract, e.g., of a home-purchase contract when the buyer expects to live in the neighborhood for 10 years? With a high benefit realization, the buyer would enjoy a benefit of 15 per-year for a 10-year period, or $10 \times 15 = 150$; and with a low benefit realization, the buyer would get $10 \times 5 = 50$. The variance would then be: $v^B = \frac{1}{2}[(150-100)^2 + (50-100)^2] = 2500$. And the standard deviation would be: $\sigma^B = \sqrt{v^B} = 50$. The risk of the long-duration, buy contract is 10 times larger, if we use standard deviation, or 100 times larger if we use variance, than the risk of a short-duration, rental contract. Intuitively, the risk associated with an uncertainty range between 5 and 15 (15-5=10) is much smaller than with an uncertainty range between 50 and 150 (150-50=100).[16]

Since longer duration magnifies risk, decisions about contract duration affect, and are affected by, other important contracting decisions. Specifically, contracting parties who choose a longer duration commitment would be expected to reduce other dimensions of contractual risk (or perceived risk, like that which might be associated with more diverse neighborhoods). Also,

[15] Of course, there are also transaction costs involved in moving from one rented apartment to another, but these are generally smaller. *See* Pablo Casas-Arce & Albert Saiz, *Owning Versus Renting: Do Courts Matter?*, 53 J. L. & ECON. 137 (2010); Steven F. Venti and David A. Wise, *Moving and Housing Expenditure: Transaction Costs and Disequilibrium*, 23 J. PUB. ECO. 207 (1984); John M. Quigley, *Transactions Costs and Housing Markets*, (U. C. Berkley, Working Paper No. W02-005, 2004).

[16] We acknowledge that rental contracts might entail other types of risk that could influence the buy v. rent decision. Specifically, renters might be concerned that the landlord will not renew their lease, thus forcing them to move. On the relationship between risk and duration in another context, *see* Jo Anna Gray, *On Indexation and Contract Length,* 86 J. POL. ECON. 1 (1978) (studying the relationship between risk and duration in the context of labor contracts); Robert Rich & Joseph Tracey, *Uncertainty and Labor Contract Durations*, 86 Rev. ECON. & STAT. 270 (2004) (empirically testing the Gray model).

considering the reverse causal mechanism, when a contract is perceived to be riskier, parties would be expected to choose a shorter contractual duration.[17]

C. Risk, Learning and Duration

When entering into a contract, a party will often be uncertain about some aspects of the transaction. For example, a person who is considering whether to buy or rent a house in a specific neighborhood may be uncertain about the characteristics of the neighborhood, such as noise levels, school quality, availability and reliability of public transportation, etc. The person will learn the relevant information after moving into the neighborhood. Specifically, she will learn whether a certain quality dimension is high or low. In this context, risk and learning are closely related. Returning to the question of duration, both the owner who purchased the house and the tenant who rented the house will learn about the characteristics of the neighborhood. But only the tenant will have an opportunity to act on the new information, e.g., by not renewing the lease. For this reason, a short-duration, rental contract is less risky than a long-duration, purchase contract.[18]

The opportunity to learn reduces the risk in short-duration contracts, when the learned information can result in exit from, or non-renewal of the contract. In some cases, though, exit or non-renewal are irrelevant. Consider a person who is choosing between two vacation contracts—one with a shorter duration and one with a longer duration. The person is uncertain about some aspects of the vacation, e.g., the amenities at the hotel or the quality of tourist attractions in the area. But even if the amenities or attractions are somewhat disappointing, the person will not cut a long-duration vacation short and extending a short-duration vacation is not an option. In such cases, there is no opportunity to act on the learned information, and thus the link between learning and risk is severed. And yet the link between risk and duration remains. With a longer vacation, the risk of subpar amenities or attractions is greater. Accordingly, when uncertainty and risk are greater, a shorter-duration contract will be preferred.

In the housing context it is more difficult to separate the learning-related and non-learning-related connections between duration and risk. When an owner moves into a neighborhood and learns negative information, she will be stuck with the lower value for a longer period of time, thus increasing the risk. When a renter moves into a neighborhood and learns negative information, she will be stuck with the lower value for a shorter period of time (e.g., the 1-year rental period), thus reducing the risk. The risk, for the tenant is smaller, because the contract period is shorter, and because she can act on the learned information and exit at the end of the shorter contract period.

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[17] Of course, there are good reasons to bear the risk of a longer contract duration. Specifically, a long-term commitment gives the other party confidence to invest in the relationship, knowing that you will not bail at the first sign of trouble. This basic trade-off between risk and investment has been studied extensively in the contract theory literature. *See*, *e.g.*, Steven Shavell, *The Design of Contracts and Remedies for Breach*, 99 Q. J. ECON. 121 (1984); Richard E. Speidel, *The Characteristics and Challenges of Relational Contracts*, 94 NW. U. L. REV. 823 (2000); Keith J. Crocker and Scott E. Masten, *Mitigating Contractual Hazards: Unilateral and Contract Length*, 19 RAND J. ECO. 327 (1988).

[18] The role of learning in this context relates to the famous explore vs. exploit dilemma: should I stay in a neighborhood that I know well and enjoy the known, certain benefit (exploit), or should I move to a new, unknown neighborhood where there is a chance of a higher benefit but also a risk of a lower benefit (explore)? On the explore vs. exploit dilemma, *see*, *e.g.*, James G. March, *Exploration and Exploitation in Organizational Learning*, 2 ORG. SCI. 71 (1991).

D. Risk, Duration and Diversification

There are cases, including housing, where the decisionmaker must secure the service for a long period of time, and the only choice is between one long-duration contract (buying a house) and several short-duration contracts (consecutive rental contracts). In such cases, short-duration contracts reduce risk also through the mechanism of diversification. It is well known that risk-averse investors should diversify their portfolio. In a similar way, a series of short-term rentals—in different neighborhoods and even in the same neighborhood—provides a diversification benefit as compared to a long-duration purchase contract.

When studying the relationship between risk, diversification and duration, it is important to distinguish between consumption value and investment value. Our focus in this Article is on consumption value and the associated risk. A person "consumes" a vacation and incurs the risk of lower consumption value, if the hotels' amenities or the tourist attractions in the area turn out to be subpar. Similarly, an owner or renter "consumes" housing services and incurs the risk of lower consumption value, if the quality of the neighborhood, on different dimensions, turns out to be less-than-expected.

In some cases, a contractual choice creates investment value in addition to consumption value. Housing is a key example. Short-duration, rental contracts inherently focus on the consumption value of housing. But long-duration, purchase contracts combine both a consumption value and an investment value.[19] This complicates the comparison between renting and buying. Still, the general observation that long-term contracts are riskier holds: buying entails a higher risk from a possible reduction in the consumption value (e.g., if a noisy neighbor moves in next door) and also from a possible reduction in the investment value, i.e., from a reduction in real-estate prices. Moreover, owning a house is considered to be risky from an investment perspective, since many owners put much of their wealth into a single, undiversified asset—their home.[20,21]

E. Perceived Risk, Discrimination and Contracting

Leading theories of discrimination distinguish between discrimination based on discriminatory beliefs and discrimination based on discriminatory tastes. Belief-based discrimination, or statistical discrimination,[22] arises from beliefs about specific social groups. These beliefs tend to be shaped by perceptions of others' capabilities or warmth. Members of certain social groups are seen as more capable or warm than members of other groups in specific contexts. Such beliefs may

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[19] Indeed, some people buy a house only for the investment value and do not live in the house. But, in our data, buyers actually live in the house that they purchased.

[20] It would be more troubling, from the perspective of this study, if there were other risks associated with short-duration, rental contracts, e.g., the risk of an increase in rental prices.

[21] With respect to investment value, it is generally believed that long-term investing reduces risk, as it allows for smoothing of short-term fluctuations in asset prices. This risk-reducing advantage of long-term investing, as compared to short-term investing, is unrelated to the question of contractual duration that is the subject of this Article. Many long-term investors are able to exit, i.e., sell their assets, at low cost, and thus their investment contracts would be considered short-duration contracts under the terminology that we use in this Article.

[22] *See*, *e.g.*, Kenneth Arrow, *The Theory of Discrimination* (Prin. Uni., Indus. Rel. Sec., Working Paper No. 30A, 1971); Edmund Phelps, *Inflation Policy and Unemployment Theory*, 2 J. SOC. POL. 4 (1972).

be correlated with group averages or based on erroneous stereotypes. Studies have shown that Black men in America tend to be stereotypically and mistakenly viewed as threatening, hostile, aggressive and associated with criminal activities.[23] In the housing context, such beliefs would explain the reluctance of white residents to live in more diverse neighbourhoods—a reluctance that increases with contract duration.

With this form of discrimination, concrete information about specific people, or about a social group decreases discrimination.[24] A recent study explored the effects of summer internships on the gender gap in the initial salaries of graduates from an elite management program. On average, women received lower initial salaries than men. But women received higher salary offers from companies where they previously interned (as compared to offers from other companies); there was no such internship effect for men.[25] Similarly, renting in a diverse neighbourhood could provide an opportunity to learn that prior risk perceptions were unjustified. Such learning would reduce discrimination and increase the likelihood of buying (or renting) in the specific neighbourhood or in other more diverse neighbourhoods in the future.

'Taste discrimination' arises from discriminators' negative or positive preferences that are generated by attitudes toward certain social groups. Examples of such attitudes might include admiration, envy, affection, fear, pity, disgust, etc. Taste discriminators are willing to forego monetary gains in their contractual interactions in order to cater to their discriminatory tastes.[26] In the context of race in America, taste discrimination would entail a preference to avoid living in racially diverse neighborhoods, especially for a long duration (and a willingness to pay more for otherwise similar housing in less diverse neighborhoods). The theory of taste discrimination allows for a dynamic effect akin to learning, as people's preferences evolve over time and are affected by their interactions with others.

The 'contact hypothesis' in the study of prejudice suggests that under conditions of equal status, shared goals, authority sanction, and the absence of competition, interactions between groups might reduce prejudice.[27] In one study that was conducted in the 1960s in the American South, interracial workplace contact was stimulated by inviting racially prejudiced white research participants to "work" on a railroad company management task with one white tester and one

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[23] For the stereotypical perceptions of Black people in America, *see* Neil Hester & Kurt Gray*, For Black Men, being Tall Increases Threat Stereotyping and Police Stops*, 115 PROC. ACAD. SCI. 2711 (2018); Bernd Wittenbrink, Charles M. Judd & Bernadette Park, *Evidence for Racial Prejudice at the Implicit Level and its Relationship With Questionnaire Measures*, 72 J. PER. & SOC. PHYS. 262 (1997); Devine, Patricia G. & Andrew J. Elliot, *Are Racial Stereotypes Really Fading? The Princeton Trilogy Revisited*, 21 PER. & SOC. PHYS. BULL. 1139 (1995); Jon Hurwitz & Mark Peffley, *Public Perceptions of Race and Crime: The Role of Racial Stereotypes,* 41 AM. J. POL. SCI. 375 (1997); Samuel L. Gaertner & John P. McLaughlin, *Racial Stereotypes: Associations and Ascriptions of Positive and Negative Characteristics*, 46 SOC. PSYCH. Q. 23 (1983).

[24] *See*, *e.g.*, Joseph G. Altonji & Charles R. Pierret, *Employer Learning and Statistical Discrimination*, 116 Q. J. ECON. 313 (2001).

[25] *See* Adina D. Sterling & Roberto M. Fernandez, *Once in the Door: Gender, Tryouts, and the Initial Salaries of Managers*, 64 MGMT. SCI. 5444 (2018).

[26] *See*, *e.g.*, GARY S. BECKER, THE ECONOMICS OF DISCRIMINATION (1957); David Neumark, *Wage Differentials by Race and Sex: The Roles of Taste Discrimination and Labor Market Information*, 38 INDUS. REL. 414 (1999).

[27] *See*, *e.g.*, Thomas F. Pettigrew & Linda R. Tropp, *A Meta-Analytic Test of Intergroup Contact Theory*, 90 J. PERS. SOC. PSYCH. 751 (2006). We note that the empirical studies on the 'contact hypothesis' do not fully distinguish between belief-based discrimination and taste discrimination.

Black tester (- the testers were associated with the research team). Participants were led to believe that they were employed in a real part-time job for a month. At the end of the month, participants were asked to rate the coworkers. The Black tester was rated highly in attractiveness, likeability, and competence. In addition, in a survey that was taken several months later and presented as unrelated, the participants in the study expressed less racial prejudice compared to a control group of participants who did not participate in the original study.[28] In another study, researchers interviewed white female residents across different public housing projects that varied by whether they were segregated or desegregated. The white female residents in the desegregated housing projects expressed more esteem for their Black neighbors and tended to support interracial housing more than the women in the segregated housing project.[29] This body of work on the 'contact hypothesis' suggests that renting a house in a more racially diverse neighborhood may alter people's preferences and affect their willingness to buy houses in more diverse neighborhoods in the future.

Under both belief-based and taste-based theories, we would expect duration to be correlated with discrimination. We would expect white people to prefer less diverse neighborhoods when buying as compared to renting; and to choose shorter-duration, rental contracts when moving into a more diverse neighborhood. We would also expect a learning effect, namely, that home buying decisions would be influenced by prior renting experiences.

II. Survey Data

The data that we use is taken from the National Longitudinal Survey of Youth, 1997 (NLSY97). This longitudinal dataset was collected annually over a 17-year period by the Bureau of Labor Statistics on 8,984 men and women born between 1980 and 1984 and living in the United States at the time of the initial survey in 1997. We merged the NLSY97 data with several data sources for county-year level information.[30] We begin, in Section A, by evaluating respondents' initial residential contract choices. Then, in Section B, we consider all housing decisions, using fixed-effects models. Finally, in Section C, we test for the effects of past experiences on respondents' first decision to buy a house (possibly after several renting experiences). We collect and discuss our findings in Section D. Finally, in Section E, we consider alternative explanations for our empirical findings.

[28] *See* Stuart W. Cook*, Interpersonal and Attitudinal Outcomes in Cooperating Interracial Groups,* J. RSCH. & DEV. in EDU. (1978); STUART W. COOK, THE EFFECT OF UNINTENDED INTERRACIAL CONTACT UPON RACIAL INTERACTION AND ATTITUDE CHANGE 222-244 (1971).

[29] *See* MORTON DEUTSCH & MARY EVANS COLLINS, INTERRACIAL HOUSING: A PSYCHOLOGICAL EVALUATION OF A SOCIAL EXPERIMENT (1951).

[30] Specifically, data regarding the population size and the percentage of white and non-white residents in the county was taken from the Census Intercensal County Population Data (United States Census Bureau, County Intercensal Datasets: 2000-2010 available at: https://www.census.gov/data/datasets/time-series/demo/popest/intercensal-2000-2010-counties.html); crime rate data was taken from the *nacjd* Uniform Crime Reporting Program (available at: https://www.icpsr.umich.edu/web/pages/NACJD/guides/ucr.html); and data on the value of houses in the county was taken from Zillow.com (Zillow, Housing Data, available at https://www.zillow.com/research/data/). Given the size of many US counties, it would have been better to use zip-code-level data, rather than county-level data. Unfortunately, we did not have the necessary zip-code-level data for all the control variables.

A. The First Housing Decision

We evaluate the initial residential contract choices made by the respondents. The first housing decision for respondents who do not attend college occurs when they leave their parents' home, whereas for those who attend college, it occurs after their college graduation. Table 1 presents the descriptive statistics for these initial housing decisions.

Table 1: Initial Housing Choice – Descriptive Statistics

	Mean	SD	Min.	Max.	N
Own	0.11	0.31			4845
Median value of house in county ($ per sqft)	130.59	104.6	23.92	1167.5	5120
Number of residents in county (in millions)	1.11	2.13	0	19.65	5909
Percentage of white residents in county	0.65	0.21	0.07	0.99	5909
Married	0.18	0.38			5896
College education	0.26	0.44			5873
Household income (in thousands of dollars)	40.05	56.78	0	421.37	4884
Number of children in household	0.31	0.66	0	6	5963
Female	0.49	0.5			8625
Age	22.23	3.25	14	35	5974
White respondent	0.5	0.5			8625
Black respondent	0.26	0.44			8625
Latino/Latina respondent	0.11	0.31			8625
Asian	0.02	0.13			8625
Other	0.12	0.32			8625

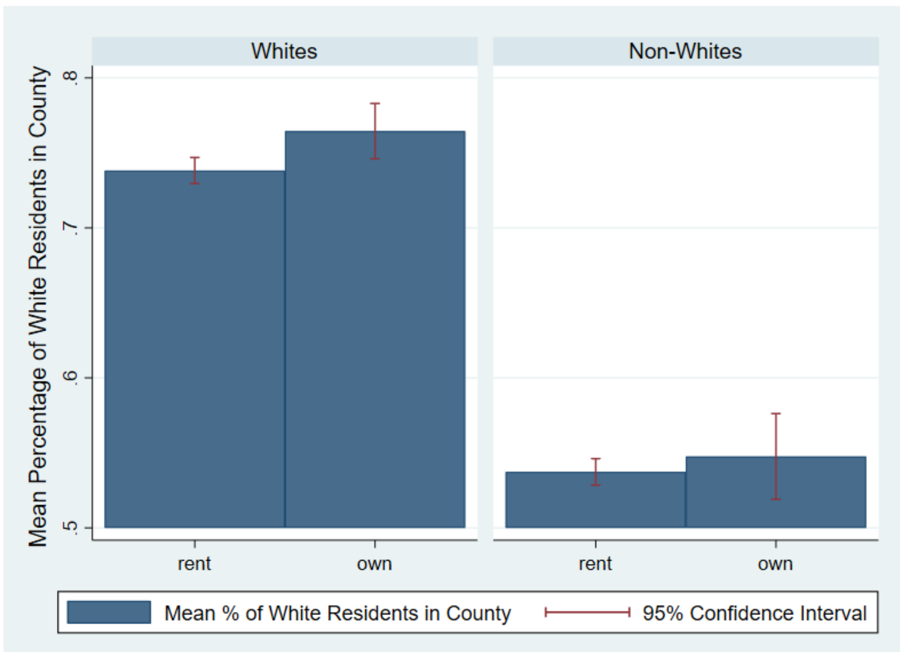
As expected, most respondents, when they make their first housing decision, choose to rent. The ownership rate is only 11%. Table 1 also provides a general overview of our respondents. They are 49% female. They are 50% White, 26% Black, 11% Latinx, 2% Asian and 12% who are listed as "other." When the first housing decision is made, the average respondent is 22 years old, and her household income is $40k; 18% are married and 26% have already earned a college degree.[31]

[31] Table 1 does not include all of the variables in our data. Less important variables were excluded from Table 1, even though they were included in the regression analysis (below). Specifically, the variables that were excluded from Table 1 are: an index of the violent crime rate in the county, HPI2000base (a Housing Price Index from the Federal Housing and Finance Agency, with the year 2000 taken as the base year), the unemployment rate in the county, indicator of town/city size (small town, large town, medium city, large city and mega city) and respondent's income (which was excluded because we already have household income). The variable "household worth" was excluded from Table 1 and from the regression analysis, because the data include "household worth" information on a relatively small minority of respondents and even this information is quite noisy. Table A1, in the Appendix, includes descriptive statistics for all variables.

Note that the variable "Percentage of white residents in county" includes only non-Hispanic white respondents and is measured on a 0-1 scale.

Since we are interested in the rent vs. own decision, it is helpful to divide our sample into renters and buyers.[32] Relative to renters, among buyers we observe more women, more white respondents and fewer Black respondents, higher income, significantly more married couples, and significantly more college degrees. These additional descriptive statistics are relegated to the Appendix (Table A1a). We are especially interested in investigating the effects of race, and of neighborhood racial diversity, on the rent v. buy decision. For this purpose, we focus on white respondents. Appendix Table A1b reports descriptive statistics, for white renters and buyers when they make their first housing decision. The main observation is depicted in Figure 1: Within the subgroup of white respondents, owners choose counties with a higher percentage of white residents than renters.

Figure 1: Initial Housing Choice – Mean Percentage of White residents in County – Renters vs. Buyers, by Race of Participants



We now move beyond these descriptive statistics. The analysis is complicated by the simultaneity of the rent v. buy decision and the location decision (i.e., where to rent or buy). We begin by reporting the following correlations: In the first set of regression models, ownership (the rent or buy decision) is the dependent variable and county characteristics and respondent characteristics are the independent variables. In the second set of regression models, a county characteristic, percentage of white residents in the county, is the dependent variable and ownership (the rent or buy decision), respondent characteristics and other county characteristics are the independent variables.

[32] The "rent" category is defined as those respondents who do not own, which may include a few respondents who do not actually rent (e.g., respondents who live at the home of their spouse's parents; respondents who still live with their own parents are excluded since they have not yet made their first housing decision).

We start with logistic regression models, where ownership (the rent or buy decision) is the dependent variable and county characteristics and respondent characteristics are the independent variables. Table 2 reports the correlations, for the full sample ("All"), for "White respondents," and for "Non-White respondents".[33] White respondents are more likely to buy, rather than rent, when the percentage of white residents in the county is higher.

Table 2: Initial Housing Choice — Logistic Regression Models Predicting Rent vs. Buy Decisions

	All	White respondents	Non-White respondents
Percentage of white residents in county	0.121**	0.282**	0.03
	(0.06)	(0.11)	(0.06)
Median value of house in county (USD per sqft)	0	0	-0.000**
	(0)	(0)	(0)
Crime rate index in the county	-0.54	9.316**	-5.137**
	(2.3)	(4.01)	(2.43)
Housing price index in the county	0	-0.001**	0.000*
	(0)	(0)	(0)
Number of residents in county (in millions)	0.001	-0.02	0.002
	(0)	(0.01)	(0)
Unemployment rate in county	-0.083	-0.646	0.131
	(0.24)	(0.44)	(0.23)
Married (d)	0.154***	0.166***	0.128***
	(0.02)	(0.03)	(0.03)
College education (d)	0.061***	0.069***	0.053**
	(0.02)	(0.03)	(0.02)
Household income (in thousands of dollars)	0.000***	0.000***	0.000***
	(0)	(0)	(0)
Number of children in household	-0.004	-0.028	0.004
	(0.01)	(0.02)	(0.01)
Average % of white residents in county, Past residences	-0.021	-0.063	0.008
	(0.05)	(0.1)	(0.06)
First move was for college (d)	0.011	0.016	0.009
	(0.01)	(0.02)	(0.02)
Observations	2610	1272	1338

Marginal effects; Standard errors in parentheses
(d) for discrete change of dummy variable from 0 to 1
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

[33] In the "All" models (in Tables 2-5), we controlled for race (with Black respondents as the omitted category). The regression models reported in Tables 2-5 do not include gender and age as controls, because the effects of these variables are not significant. Regression models that include gender and age are provided in the Appendix, Tables A2-A5.

We next turn to regression models, where the percentage of white residents in the county is the dependent variable and ownership (the rent or buy decision), respondent characteristics and other county characteristics are the independent variables. Table 3 reports the correlations. White respondents are more likely to choose a whiter and less diverse location when they buy, as compared to when they rent.

Table 3: Initial Housing Choice — OLS Regression Models Predicting Percentage of White Residents in the County

	All	White respondents	Non-White respondents
Own	0.015**	0.021***	0.007
	(0.01)	(0.01)	(0.01)
Median value of house in county (USD per sqft)	-0.000***	-0.000***	0
	(0)	(0)	(0)
Crime rate index in the county	-14.220***	-20.606***	-8.880***
	(0.64)	(0.96)	(0.82)
Housing price index in the county	-0.000***	0	-0.000***
	(0)	(0)	(0)
Number of residents in county (in millions)	-0.008***	-0.019***	-0.005***
	(0)	(0)	(0)
Unemployment rate in county	-0.487***	-0.174	-0.647***
	(0.08)	(0.12)	(0.1)
Married	-0.006	-0.006	-0.01
	(0)	(0.1)	(0.1)
College education	-0.008	-0.015**	0.001
	(0.1)	(0.1)	(0.1)
Household income (in thousands of dollars)	0	0	0
	(0)	(0)	(0)
Number of children in household	0	0.009*	-0.003
	(0)	(0.1)	(0)
Average % of white residents in county, Past residences	0.696***	0.598***	0.741***
	(0.1)	(0.02)	(0.02)
First move was for college	0.003	0.011*	-0.004
	(0)	(0.1)	(0.1)
Constant	0.379***	0.44***	0.314***
	(0.02)	(0.03)	(0.03)
Observations	2610	1272	1338

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Counties in our sample vary in the density of their population, in their size and in the nature of the living experience that they offer. These factors might correlate with the tendency to buy rather than rent as well as with the percentage of white residents in the county. We account for such variation in county type by controlling for the number of residents in the county (see Tables 2-6). Results remain qualitatively similar, when we add a specific control for county type (town, city, mega-city).[34]

B. Beyond the First Housing Decision: Fixed-Effects Regressions

We now look beyond the first housing decision—to all housing decisions. We note, however, that since respondents are relatively young (in 2019, the average age of respondents is 37), the number of housing decisions made by the average respondent is only 2.7. We use fixed-effects regressions to perform a within-person analysis. Specifically, in our panel data each respondent appears several times, and our models include a dummy variable for each respondent. The unit of analysis is therefore a respondent in a given year (each respondent appears in the data once for every year in which she was interviewed). These person-specific fixed-effects models hold constant all the unchanging characteristics of the respondents in our data.[35] This eliminates many possible selection effects. The people who buy and the people who rent may differ in ways that are not observed in our data but are correlated with the decision to live in more or less integrated neighborhoods. For example, homeowners may be more conservative and renters may be more progressive. The (statistically significant) negative correlation between owning and the percentage of white residents in the area could then simply reflect the differences between the population of owners and the population of renters. The person-specific fixed-effects models allow us to test whether the same person makes a different rent vs. buy decision depending on neighborhood diversity, and whether the same person chooses a different location—a more or less diverse location—when she buys compared to when she rents.

As before, we start with regression models where ownership (the rent or buy decision) is the dependent variable and county characteristics and respondent characteristics are the independent variables. We follow respondents over time and test for the effects of past experiences in more or less diverse locations on their rent vs. buy decision. Specifically, we are trying to predict the decision to buy by the percentage of white residents in the county into which the respondent

[34] Regression models that control for the type of residential area (town, city, mega-city) are provided in the Appendix, Tables A2-A5. Residential area type classifications were taken from the Census Intercensal County Population Data (United States Census Bureau, County Intercensal Datasets: 2000-2010, available at: https://www.census.gov/data/datasets/time-series/demo/popest/intercensal-2000-2010-counties.html). Appendix Tables A2-A5 also control for the rate of owner-occupied v. rental units in the county (through the 'percentage of house ownership in the county' control variable), which also captures differences between types of residential areas. (We also added interactions between the type of residential area—town, city, mega-city —and the own v. rent variable in the Table A3 regressions. Interaction effects were not statistically significant, which provides further evidence that the correlations we observe are not driven by differences in county size, population density or the nature of the living experience that the county offers. These regression results, with the interactions, are available from the authors upon request.)

[35] The coefficients in these person-specific fixed-effects models represent the average affects within person in our data.

moved,[36] as well as by the average percentage of white residents across the counties where the respondent previously lived. We find no statistically significant correlations. (The regression models are reported in Appendix Table A3a.)

We next turn to regression models, where the percentage of white residents in the county is the dependent variable and ownership (the rent or buy decision), respondent characteristics and other county characteristics are the independent variables. We follow respondents over time and test for the effects of past experiences on their current location choices. Table 4 reports the correlations. White respondents are more likely to choose a whiter, less diverse, location, when they are buying as compared to renting, although the effect is only marginally significant. We also find that respondents with a history of living in whiter, less diverse places are more likely to move into whiter, less diverse neighborhoods. This finding is particularly interesting, given our fixed-effect, within-person specification. It suggests a learning effect.[37]

Table 4: Fixed Effects OLS Regression Models Predicting the Percentage of White Residents in the County

	All	White respondents	Non-White respondents
Own	0.008***	0.006*	0.006
	(0)	(0)	(0)
Median value of house in county (USD per sqft)	-0.000***	-0.000***	0
	(0)	(0)	(0)
Crime rate index in the county	-21.593***	-24.275***	-19.027***
	(0.77)	(1.12)	(1.09)
Housing price index in the county	0.000***	0.000***	0
	(0)	(0)	(0)
Number of residents in county (in millions)	-0.014***	-0.022***	-0.010***
	(0)	(0)	(0)
Unemployment rate in county	-0.073***	-0.013	-0.114***
	(0.02)	(0.04)	(0.03)
Married	0.002	0	0.004
	(0)	(0)	(0)
College education	-0.007**	-0.012***	-0.001

[36] If a respondent moved several times, we have several different observations, and several different values of the percentage-of-white-respondents-in-county variable, for that respondent.
[37] It could be thought that this finding—that respondents with a history of living in whiter, less diverse neighborhoods are more likely to move into whiter, less diverse neighborhoods—simply captures a mechanical inertia effect (rather than a learning effect): If a respondent is living in a diverse neighborhood and not moving, then over time there will be a stronger correlation between the current percentage of white residents in the neighborhood and the average percentage of white residents in the respondent's residences during previous years. To rule out this possibility, we added to the regression model a "recently moved" variable and an interaction between this new variable and the "percentage of whites in past residences" variable. This robustness analysis appears in the Appendix, in Table A4a. We find that the coefficient for the interaction is negative and significant (-0.076), but it is also much smaller than the main effect of the "percentage of whites in past residences" variable (0.272). This implies a small mechanical-inertia effect (0.076, which is not present, i.e., subtracted, for respondents who have "recently moved") and a large learning effect (0.272 – 0.076 = 0.196). (The coefficient for the "recently moved" variable is positive and significant (0.052), suggesting that people—both white and Black people—tend to move into white neighborhoods.)

	(0)	(0)	(0.01)
Household income (in thousands of dollars)	0	0	-0.000**
	(0)	(0)	(0)
Number of children in household	0.003***	0.003**	0.002
	(0)	(0)	0
Average % of white residents in county, Past residences	0.231***	0.277***	0.188***
	(0.04)	(0.05)	(0.06)
Constant	0.774***	0.754***	0.766***
	(0.03)	(0.04)	(0.04)
Observations	37045	18121	18924

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

C. Buying Your First House: Learning Effects

Here, we test for the effects of past experiences on current housing decisions. Specifically, we focus on respondents' first decision to buy a house (possibly after several renting experiences) and test whether the diversity of the locations where a respondent previously rented affects her tendency to choose a more or less diverse location when she buys her first home. The dependent variable is the percentage of white residents in the county into which the respondent is moving. A key independent variable is the average percentage of white residents across the counties where the respondent previously lived (before buying her first home).[38] The "years renting" variable is also important, measuring the length of the respondent's overall renting experiences, as is the interaction between this variable and the average percentage of white residents across the counties where the respondent previously lived. Table 5 reports the correlations.[39] We find that past experiences are correlated with the first home-buying decision. Respondents with past experiences in whiter, less diverse locations are more likely to purchase their first home in whiter, less diverse locations.

We acknowledge that the analysis reported in Table 5 does not control for selection effects (unlike the preceding person-specific fixed-effects models). For example, an underlying racial bias can explain the respondent's decision to buy a home in a non-diverse neighborhood, as well as her prior decisions to rent in non-diverse neighborhoods. Accordingly, the correlations that we find may not represent a causal learning effect. As with prior results, we view the correlation between respondents' home-buying decisions and their past experiences as suggestive of a possible learning effect, not as proof that learning occurs.

[38] This a weighted average, accounting for number of years in each location.
[39] As a robustness check, Table A5a, in the Appendix, distinguishes between years during which the respondent lived with his/her parents and years during which the respondent made independent housing decisions. The results are qualitatively similar to those reported in Table 5.

Table 5: OLS Regression Models Predicting Percentage of White Residents in County When Respondents Buy Their First House

	All	White respondents	Non-White respondents
Average % of white residents in county, Past residences	0.626^{***}	0.579^{***}	0.656^{***}
	(0.02)	(0.03)	(0.03)
Years renting	-0.001	0	-0.005^{**}
	(0)	(0)	(0)
% White residents in county X years renting	0.004^{***}	0.002	0.010^{***}
	(0)	(0)	(0)
Median value of house in county (USD per sqft)	-0.000^{**}	-0.000^{***}	0
	(0)	(0)	(0)
Crime rate index in the county	-19.707^{***}	-22.071^{***}	-14.604^{***}
	(1.01)	(1.21)	(1.79)
Housing price index in the county	0	0	0
	(0)	(0)	(0)
Number of residents in county (in millions)	-0.013^{***}	-0.021^{***}	-0.008^{***}
	(0)	(0)	(0)
Unemployment rate in county	-0.047	0.197	-0.322^{*}
	(0.01)	(0.13)	(0.17)
Married	-0.001	-0.001	-0.003
	(0.01)	(0.01)	(0.01)
College education	-0.004	-0.003	0.002
	(0.01)	(0.01)	(0.01)
Household income (in thousands of dollars)	0	0	-0.000^{***}
	(0)	(0)	(0)
Number of children in household	0.002	0.002	0.002
	(0)	(0)	(0)
Constant	0.565^{***}	0.593^{***}	0.539^{***}
	(0.04)	(0.05)	(0.06)
Observations	1593	1010	583

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

D. Summary

We can now summarize our analysis of the survey data. Starting with respondents' initial residential contract choices, we find that white respondents are more likely to buy, rather than rent, when the percentage of white residents in the county is larger; and that white respondents are more likely to choose a whiter, less diverse, location, when they are buying as compared to renting. This last result is confirmed when we look beyond the first housing decision and use fixed-effects regressions to perform a within-person analysis and thus control for a range of selection effects.

Next, using the fixed-effects regressions, we follow respondents over time and test for the effects of past experiences in more or less diverse locations on their current location choices. We find that respondents with a history of living in whiter, less diverse places are more likely to move into whiter, less diverse neighborhoods—a finding that suggests a learning effect. (When focusing on the buy-or-rent decision as the dependent variable, we find no statistically significant effect of the percentage of white residents in the county into which the respondent moved or of the average percentage of white residents across the counties where the respondent previously lived.) Finally, to further test for learning effects, we focus on respondents' first home-buying decision, and show that respondents with past renting experiences in whiter, less diverse locations are more likely to purchase their first home in whiter, less diverse locations; and that respondents with past renting experiences in more diverse locations are more likely to purchase their first home in more diverse locations.

These results are consistent with the hypothesis that some white individuals associate greater racial diversity with higher risk and thus choose a shorter-duration, rental contract, when relocating into more diverse neighborhoods. The results are also consistent with the hypothesis that, because some white individuals associate greater racial diversity with higher risk, when they choose a longer-duration, purchase contract, i.e., when they buy a home, they tend to do so in less diverse neighborhoods. In addition, these results are consistent with the learning hypothesis, namely, that past experience, usually past rental experience, in more diverse neighborhoods dispels, for some individuals, the association between greater racial diversity and higher risk, thus leading these individuals to buy homes (long-term contracts) in more diverse neighborhoods.

We acknowledge that there might be an additional qualitative psychological difference between homeownership and renting that may have contributed to the trends we observe in the data: People might perceive their place of residence as more reflective of their identity when they own a home, as compared to when they are "merely" renting. We view this mechanism as complementary to contract duration: Because buying is perceived to be more reflective of identity than renting, the effect of the discriminatory preferences of white residents on their location choices is greater when buying, relative to renting.

Finally, while we focus on a demand-side story, emphasizing how discriminatory perceptions or preferences might affect the housing decisions of buyers and tenants, we should note the potential effects of supply-side discrimination. Specifically, landlords who are renting out their houses or apartments might manifest more racial bias than sellers. Such a difference in supply-side bias would follow from a duration-centered approach: sellers have a limited, short-term interaction with buyers, whereas landlords have longer-term interactions with their tenants. If true, this supply-side story would imply that, in less diverse neighborhoods, whites face a larger supply of rental units than non-whites, whereas both whites and non-whites face a similar supply of housing units for purchase. We would thus expect whites to be over-represented as tenants, rather than buyers, in less diverse neighborhoods, which is not what the data show. In other words, the data suggest that the demand-side effect, on which we focus, is stronger than this alternative supply-side effect.

E. Alternative Explanations

While the survey data are consistent with our '(perceived) risk-duration' explanation, they do not provide conclusive evidence for the causal mechanism that we propose. It is, therefore, important to consider alternative explanations for the observed correlations.

Schools. Perhaps the most obvious alternative explanation has to do with family formation and schools. According to this explanation, many families decide to buy a home when their children approach school age, and the choice of location (where to buy the home) is determined by the quality of schools in the county. If less diverse counties tend to have higher-quality schools, then the correlations that we observe between the buy v. rent decision and racial diversity can be driven by the 'schools' explanation, rather than by our '(perceived) risk-duration' explanation.

We cannot rule out the 'schools' explanation. Still, we argue that our '(perceived) risk-duration' explanation is more consistent with the data or, at least, that the 'schools' explanation does not crowd out our '(perceived) risk-duration' explanation. First, our regression models control for marital status and for the number of children in the household. This means that, even when focusing on households at the same family formation stage, we still observe a correlation between contract duration (buy v. rent) and neighborhood diversity. Second, the correlations between the buy v. rent decision and neighborhood diversity are observed only for the white respondents in our sample. If the 'schools' explanation was driving the results, we would expect to see similar correlations for non-white respondents.[40] Third, the evidence of a learning effect is not consistent with the 'schools' explanation. While we maintain that the data are more consistent with our '(perceived) risk-duration' explanation than with a narrow understanding of the 'schools' explanation, we suggest that a broader understanding of the schools explanation can be integrated into our '(perceived) risk-duration' explanation. One dimension of the risk, or perceived risk, associated with a neighborhood has to do with the schools in that neighborhood. If, justifiably or not, parents are less certain about their children's educational experience—both in terms of instruction quality and in terms of the classroom environment—in a diverse-neighborhood school, then they would be less likely to buy a house in that neighborhood.

Housing stock. Another possible explanation is based on the cross-county variation in housing stock. Specifically, some counties have more rental units, whereas in other counties most units are offered for sale (and not for rental). If more diverse counties tend to have more rental units and in less diverse units more units are offered for sale, then the correlations that we observe between the buy v. rent decision and racial diversity can be driven by the 'housing stock' explanation, rather than by our '(perceived) risk-duration' explanation.

Again, we cannot rule out this alternative explanation. And, again, we argue that our '(perceived) risk-duration' explanation is more consistent with the data or, at least, that the 'housing stock' explanation does not crowd out our '(perceived) risk-duration' explanation. As with the 'schools'

[40] We acknowledge that the 'schools' explanation would apply less forcefully to poorer households who cannot afford to buy a home in neighborhoods with good schools. And, if socio-economic status is correlated with race, the 'school' explanation would apply less forcefully to non-white households. Still, because the regression models control for household income and for average house prices in the county, we do not believe that the 'schools' explanation can fully account for the observed correlations.

explanation, if the 'housing stock' explanation was driving the results, we would observe correlations between the buy v. rent decision and neighborhood diversity for all the respondents in our sample and not only for the white respondents. And, as with the 'schools' explanation, the evidence of a learning effect is not consistent with the 'housing stock' explanation. But we can also address the 'housing stock' explanation directly by adding a control variable that measures the percentage of rental units in the county. Adding this control variable does not significantly alter our results.[41]

Investment value. A third possible explanation focuses on the investment value of owning a house, rather than on the consumption value on which our '(perceived) risk-duration' explanation is based.[42] If the investment risk associated with homeownership tends to be lower in less diverse neighborhoods, this could explain the correlations that we observe between the buy v. rent decision and racial diversity. Once again, we cannot rule out this alternative explanation, but argue that our '(perceived) risk-duration' explanation is more consistent with the data or, at least, that the 'investment value' explanation does not crowd out our '(perceived) risk-duration' explanation. As with the other alternative explanations, if the 'investment value' explanation was driving the results, we would observe correlations between the buy v. rent decision and neighborhood diversity for all the respondents in our sample and not only for the white respondents. And, the evidence of a learning effect is not consistent with the 'investment value' explanation.[43]

III. Experiments

The Part II analysis of the survey data is suggestive, but it cannot convincingly identify causal relationships between contract duration (rent v. buy) and other contractual dimensions (where to rent or buy). Therefore, in Part III, we report findings from four experiments, designed to assess the causal relationship between contract duration and other contractual dimensions that affect the perceived contractual risk. We also test for learning effects. Section A offers a general description of our experimental design. In Sections B-D, we describe the design of, and report the results from, each of the four experiments—trust game experiments, where the identity of the contracting

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[41] More precisely, we use the percentage of house ownership in the county as our control variable, which is simply one minus the percentage of rental units in the county. This robustness check is relegated to the Appendix, where Tables A2-A5 rerun the Part II regressions with the percentage of rental units in the county as an additional control variable. (This control variable is constructed using ACS data, which includes (i) the number of owner-occupied housing units in the county, and (ii) the number of renter-occupied housing units in the county.)

[42] On consumption value vs. investment value, *see supra* Sec. I.D.

[43] Ideally, we would control for investment risk in our regression models. Unfortunately, we do not have good data on investment risk. The best we could do is use the variance of house prices in the county, measured over the survey period, as a proxy for investment risk. (We used the housing price index from the Federal Housing and Finance Agency: Federal Housing Finance Agency, Data Sets, available at: https://www.fhfa.gov/DataTools/Downloads.) We add this control variable, 'housing price volatility in county,' as a robustness check in Appendix Tables A2-A5, and show that our results do not change significantly, but we acknowledge that this variable is probably not a very good proxy for investment risk. (The volatility variable has a significant and negative coefficient, when the dependent variable is the percentage of white residents in the county, suggesting that respondents who generally move to less diverse neighborhoods because they are less risky are less inclined to choose a less-diverse neighborhood when house prices in that neighborhood are more volatile. But the volatility variable is not significant, when the dependent variable is the buy v. rent decision; here we would expect a negative coefficient on the volatility variable.)

partner is the contracting dimension that affects perceived risk. We collect and discuss our findings in Section E.[44]

A. Experimental Design: General

Using Qualtrics' online sample, we recruited 763 participants: 184 for Experiment 1, 173 for Experiment 1a, 143 for Experiment 2, and 210 for Experiment 3.[45] The samples for all studies include American adults who were recruited directly by Qualtrics—an online survey company—to participate in our studies. The experiments were conducted in 2021. (The Appendix presents the demographic characteristics of the participants in each of the experiments as well as descriptive statistics of the outcome variables that we use and analyze).

Each participant received $3 for participating in each of our experiments. In addition, each participant had a 1% probability of receiving additional compensation that varied across our experiments. All experiments were pre-registered at OSF and approved by Harvard University's Internal Review Board.[46]

In these experiments, we tested the relationship between risk, learning and contract duration using a trust game. The risk factor concerns the identity of the counter-party and how trustworthy this counter-party is perceived to be. Participants played the following trust game (with variations in the number of rounds played and in the selection of partners for the game, as detailed below). A Participant was given $10, placed in their virtual, experimental wallet. The Participant was then asked to choose an amount between $0 to $10 that would be transferred to a Partner. The transferred sum was then tripled by the experimenters. For example, if a Participant transferred $5, then her Partner would receive $15. Finally, the Partner decided how much of the tripled sum to transfer back to the Participant (to be added to the Participant's virtual wallet). In this game, the amount transferred by the Participant to the Partner depends on how much the Participant trusts the Partner (specifically, on how much the Participant trusts the Partner to transfer back a fair amount).[47]

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[44] We ran a second set of experiments—investment game experiments, where the subject-matter of the contract, i.e., the type of investment, is more- or less risky. The results were ambiguous, likely due to suboptimal design. Specifically, we did not find statistically significant effects of duration on investment choices, but we did find that riskier investment options lead to fewer investment rounds, i.e., to shorter duration. In terms of learning, we found an asymmetric learning effect: Favorable investment outcomes in early investment rounds lead to more risk-taking in subsequent rounds, but unfavorable outcomes in early rounds do not lead to less risk-taking in subsequent rounds. A full description of, and results from, these experiments are available from the authors upon request.

[45] These numbers reflect only the participants who answered all of the questions (i.e., participants who failed to answer one or more questions were excluded).

[46] For the OSF pre-registration, *see* Tamar K. Katz & Oren Bar-Gill, Contract Duration. Retrieved (2021, February 9) from osf.io/t6hby. For the IRB approval, *see* letter sent by Harvard IRB Administrator, Elizabeth Parsons, Notification of Initial Study Exemption Determination, January 29, 2021 (available from authors).

[47] The trust game has been used extensively in the experimental literature. *See*, *e.g.*, Joyce Berg, John Dickhaut & Kevin McCabe, *Trust, Reciprocity, and Social History*, 10 GAMES ECON. BEHAV. 122 (1995); Daniel Houser, Daniel Schunkb & Joachim Winter, *Distinguishing Trust From Risk: An Anatomy of the Investment Game*, 74 J. ECON. BEHAV. ORGAN. 72 (2010); Abrahao et al, *surpa* note 10. In the standard game theoretic solution to the trust game, the Participant transfers $0 to the Partner; the welfare maximizing outcome is for the Participant to transfer the maximal amount to the Partner.

The structure of the game was fully explained to all participants in advance. In addition to the basic compensation for participating in the experiment, participants had a 1% chance of receiving a bonus equal to the amount in their virtual wallet (i.e., the amount that appears in their virtual wallet at the end of the game). This additional (probabilistic) compensation provides incentives that increase the external validity of our experiments. The game is designed to measure trust. Trust is important for contract duration decisions. And, as explained below, trust interacts with perceived risk and learning—the theoretical factors that influence duration decisions.

Pre-experiment Questions. At the beginning of the experiment, after providing general information, Participants are asked a series of questions about their age, gender, marital status and the state where they grew up.

Partners. Participants play the game with one or more Partners. The identity of the Partner and how similar the Partner is to the Participant play a key role in trust game experiments. Studies have shown that people tend to trust people who are more similar to them.[48] We quantify the notions of "similarity" and "difference" using a homophily score that measures the "distance" between the Participant and the Partner, in terms of age, gender, marital status and the state where they grew up. Possible scores range from zero (when the Participant and Partner share none of the four characteristics) and four (when Participant and Partner share all four characteristics).[49]

Partners are either "assigned" by the experimenters or "chosen" by the Participant:

- Assigned: The experimenters match the Participant with a randomly generated, simulated Partner. In some treatments, the simulated Partner is "very similar" to the Participant (homophily score = 4). In other treatments, the simulated Partner is "very different" from the Participant (homophily score = 0).[50]
- Chosen: By answering a series of questions, the Participant describes her preferred Partner—in terms of age, gender, marital status and the state where the Partner grew up. (A Participant could indicate that she is happy to engage with Partners from more than one age group, gender group, marital status group or state of origin.) The experimenters then match the Participant with a randomly generated, simulated Partner with the chosen traits. (Note that the Partner is chosen based on the Participant's characterization of her preferred Partner, not based on the Participant's personal identification in the pre-experiment questions.)

The Partners are simulated; they are not actual people. But Participants are told that Partners are actual people who are also playing the game. This deception does not harm Participants. A debriefing paragraph at the end of the experiment states: "The Partners with whom you played the

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[48] *See* Abrahao et al, *surpa* note 10 ("Each individual occupies a position in the social space whose coordinates are a function of his or her sociodemographic characteristics. The more features two individuals share in common, the more likely they are to form relationships based on mutual trust.")

[49] Similar scores have been used in Abrahao et al, *id*. This score, like any other scoring technique, obscures potentially relevant variation. For example, individuals who differ on gender (only) are scored similarly to individuals who vary on marital status (only). Also, our homophily score does not distinguish between individuals with a small vs. large age difference. Following Abrahao et al, *surpa* note 10, we did not use race in the main experiments, because we did not want participants to think that this was a study about race.

[50] For example, we randomly choose a Partner from all possible Partners with a homophily score of 4.

game were not actual people, rather they were computer-simulated Partners. This deception was necessary for the study. And it will not harm you: If you were selected to receive the bonus, you will receive the maximal possible bonus amount (i.e., the amount that you would have received, if you transferred the full amount to your Partner and your Partner transferred back the full, tripled sum.)" [51]

Risk perceptions. After Participants learn the structure of the game (and the specific treatment to which they are assigned), and before they make a decision—about Partners, duration or the amount to be transferred to a Partner—we elicit the level of risk that they associate with the game.[52] We were hoping that, through these risk-perception questions, we would be able to confirm that different treatments are perceived as more- or less risky. But we did not find statistically significant differences in risk perceptions across treatments in any of the experiments. It seems like our risk-perception question evoked general reactions to the type of game, rather than the specific treatment. Still, as described below, it is useful to distinguish between Participants who view the type of game as more vs. less risky.

Post-experiment questions. At the end of the experiment (after participants finished playing all rounds of the game), we asked participants about their race, income, education and political party affiliation.[53] We use these demographic and socio-economic variables as controls in (some of) the regressions (as described below).

B. Experiment 1: Effect of Duration on Risk

1. Design and Hypotheses

The first experiment evaluates whether contract duration affects Participants' choice between options that are perceived to be more or less risky. Here, perceived risk is measured by Partner choice.[54] Participants *choose* Partners (as described above). Participants were randomly divided into three groups:

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[51] In Experiment 3, which is the only experiment where the Participant receives information about the amount that the Partner transferred back, we add (after "… they were computer-simulated Partners"): "and the amount that they transferred back to you was also simulated, or chosen by the researchers." Using simulated Partners does not raise internal validity concerns. It could be thought that, in Experiment 3, when the computer immediately informs the Participant of the amount that the Partner transferred back, the Participant would infer from the speed of the response that the Partner is not a human being. We solve this problem by explaining to Participants that Partner responses were elicited in advance for all possible scenarios.

[52] We use the following question: "On a scale of 1-100 (ranging from not at all to very risky), how risky is it to transfer money from your virtual wallet to a partner in this game?" We realize that asking about perceived risk might focus participants' attention on the risk issue, including for participants who would not otherwise focus on risk.

[53] We also included a question that measures risk preferences: "A lottery ticket costs $100 and people win with 50% chance. How much should the prize be for you to choose to buy a ticket?" This is a standard method for assessing risk preferences. Compare: Abrahao et al, *surpa* note 10. A prize value of $200 has expected value of net gain equal to zero (after paying off the ticket) and represents risk neutrality. Higher prize values measured risk aversion proportional to their magnitude. Finally, we included a general open-ended question ("Do you have any thoughts about the study you want to share?").

[54] We note that, unlike in other trust game experiments, Participants are not informed about the amount that the Partner transferred back.

- Group 1 – Long Duration: Participants chose one Partner and played the game with that same Partner for 10 rounds.
- Group 2 – Medium Duration: Participants chose two Partners – one for rounds 1-5 and another for rounds 6-10.
- Group 3 – Short Duration: Participants chose ten Partners – one for each round. [55]

We can thus state our first hypothesis.

Hypothesis 1: When the duration is shorter and thus the game is perceived as less risky, Participants will choose a more "risky" Partner, i.e., the average homophily score of the chosen Partners will be smaller.[56]

In Experiment 1, the more- and less- risky choices are two dimensional: Participants chose both the Partner and the amount transferred in each round. To isolate the Partner choice effect, we also run Experiment 1a, which is identical to Experiment 1, except that we fix the amount transferred – from the Participant to the Partner – to the full $10.

2. Results

Summary statistics for Experiment 1 are relegated to the Appendix. Here, we note that for the Homophily Score of the chosen partner, we have: Mean=9.04, Min=4, Max=12. Recall that we have four traits: gender, age, marital status and state of residence. For each trait, Participant chooses between: (1) only different on the relevant trait; (2) either similar or different on the relevant trait; or (3) only similar on the relevant trait. For example, if Participant chooses 'only different' for all four traits, the homophily score is $4 \times 1 = 4$; and if Participant chooses 'only similar' for all four traits, the homophily score is $4 \times 3 = 12$.

In Table 6, we report the results of OLS regression models predicting the homophily score by experimental condition, controlling for demographic characteristics. We first report the results on the full sample (model 1, without controls). We then report results on the subsample of Participants

[55] In this experimental design, duration is varied together with the number of partners. There is a concern that the number-of-partners effect will conflate the duration effect. Alternative designs that keep the number of partners constant would necessarily vary the overall number of rounds played, and thus introduce a different conflating effect. (Another possible design would have all Participants play with 10 different Partners, such that: in Group 1, all 10 Partners are randomly chosen according to a set of characteristics that the Participant determines before round 1; in Group 2, the first 5 Partners are randomly chosen according to a set of characteristics that the Participant determines before round 1, and the remaining 5 Partners are randomly chosen according to a set of characteristics that the Participant determines before round 6; and in Group 3, each Partner is randomly chosen according to a set of characteristics that the Participant determines before each round. We have decided against this design for external validity reasons: In the real-world, long-duration contracts imply the same partner for the duration of the contract.)

[56] Another possible reason for the prediction in Hypothesis 1 has to do with notions of diversification. In the treatments with shorter duration and multiple Partners (Group 2 and especially Group 3), the Participant may wish to diversify the risk by inviting a larger range of Partners, i.e., by allowing for both "similar" and "different" Partners. To be sure, when a Participant chooses a "similar" Partner, this still allows for diversification, as the algorithm selects *different* Partners who share certain traits—age, gender, marital status and the state where they grew up—with the Participant. For example, in Group 3 the algorithms would select 10 different Partners, one for each round. Still, by inviting both "similar" and "different" Partners, the Participant arguably achieves greater diversification in terms of the traits of potential Partners. This alternative reason is not supported by our data: shorter duration is not correlated with a higher likelihood of choosing the 'either similar or different' option.

in the top three quartiles in terms of risk perceptions (model 2 without controls and model 3 with controls). We consider these subsamples, because risk perceptions play a central role in our theoretical framework. (Participants varied in their perceptions of how risky the game was; mean = 58.23, median = 60, lower end of 2nd quartile = 40, SD = 29.97, on a 1-100 scale.) In all models, playing 10 rounds with the same partner (the longest length) is the reference category.

Table 6: OLS Regression Models Predicting the Homophily Score by Experimental Condition

	Model 1 Full sample	Model 2 "this game is risky" (top 75%)	Model 3 "this game is risky" (top 75%)
5 rounds per partner*	-0.043	-0.586*	-0.625*
	(0.304)	(0.331)	(0.329)
1 round per partner*	-0.500*	-0.899***	-0.984***
	(0.285)	(0.327)	(0.324)
N	184	137	137

*the omitted category is 10 rounds per partner

* p<0.1, **p<0.05, ***p<0.01

For the full sample, playing only one round with the same partner is associated with a decrease of 0.5 in the homophily score, compared to playing ten rounds with the same partner. This suggests that on average, participants who were asked to play ten rounds with the same partner sought out partners who are more similar to them, compared to participants who were asked to play only one round with the same partner. For the sub-sample of participants who felt that transferring money in the game was relatively risky (top 75% of the sample), playing one round or five rounds with the same partner was associated with a significantly lower homophily score, compared to participants who were asked to play ten rounds with the same partner. Thus, for example, in model 2, playing one or five rounds was associated with a 0.9 or 0.6 decrease in the homophily score, respectively. These results are consistent with Hypothesis 1.

As explained above, Experiment 1a replicates Experiment 1, only that the amount that participants transfer to their partners is fixed. Summary statistics for Experiment 1a are relegated to the Appendix. On average, participants in this experiment viewed transferring money in the game as more risky than the participants in the previous experiment, perhaps because the amounts they were asked to transfer were fixed at the highest possible level. In the Appendix, we report the results of OLS regression models predicting the homophily score by experimental condition, controlling for demographic characteristics. As in Experiment 1, for the full sample, playing only one round with the same partner is associated with a significant decrease (of about 0.68 (p=0.05)) in the homophily score, compared to playing ten rounds with the same partner. Playing five rounds with the same partner was associated with a decrease of about 0.45 in the homophily score, compared to playing ten rounds with the same partner (but effects are only marginally significant).

These differences become statistically significant when we focus on the sub-sample of participants who viewed transferring money in the game as relatively risk (top 75% of the sample) and controls are included (model 3).[57] We view these results as providing additional support for Hypothesis 1.

C. Experiment 2: Effect of Risk on Duration

1. Design and Hypotheses

Whereas Experiment 1 (and Experiment 1a) explored the causal effect of contract duration on Participants' choice of Partners (i.e., between more and less similar Partners who may be perceived as less and more risky, respectively), Experiment 2 explores the causal effect of risk on Participants' duration choices. In this experiment, Participants are *assigned* a Partner (as described above).[58] The experiment proceeds as follows:

- The Participant is assigned a Partner and is told who the algorithm matched for them, e.g., "You were randomly assigned to play with a female partner, who is 40 years old. She is married and is from Colorado." The algorithm randomly assigns Partners who are "very similar" to the Participant (homophily scores = 4) or "very different" from the Participant (homophily scores = 0).[59]
- The Participant is then asked how many rounds (0 – 10) they would like to play with this Partner. And, in Stage 1 of the experiment, the game is played for the selected number of rounds.
- To allow for a valid comparison, between Participants that select a smaller v. larger number of rounds, we add Stage 2, including no-game rounds, in which the Participant gets $11.[60] For example, if a Participant chose to play 6 rounds with her assigned Partner in Stage 1, this means that there are 4 rounds remaining for Stage 2. The game is not played in these 4 rounds, but the Participant still gets 4 * $11 deposited in her virtual wallet.[61]

We can thus state the following hypothesis.

[57] The differences are statistically significant even without controls, when we focus on the top 50% of the sample in terms of risk perception.

[58] We note that, as in Experiment 1 (and Experiment 1a), Participants are not informed about the amount that the Partner transferred back.

[59] In choosing a "very similar" Partner, the algorithm randomly chooses among all possible trait-combinations that produce a homophily score of 4. In choosing a "very different" Partner, the algorithm randomly chooses among all possible trait-combinations that produce a homophily score of 0.

[60] If Participants would get $10 (or less) in the no-game rounds, then the rational strategy would be to play the game in all 10 rounds, regardless of Partner identity, and simply choose to transfer zero (or an amount close to zero). For this reason, we chose an amount above $10, specifically $11, for the no-game rounds. We didn't go above $11, so that Participants will still have an incentive to play the game in a significant number of rounds.

[61] We provide such an example in the survey questionnaire – to make sure that participants understand their options. We recognize that the numbers in the example – 6 game rounds and 4 no-game rounds – can serve as an anchor and skew results. But since the same anchor applies across all treatments, the only concern is that it will dampen the treatment effects, such that the effects we identify will be underestimates of the true effects.

Hypothesis 2: When the assigned Partner is less similar to the Participant and thus perceived to be "riskier," Participants will choose a shorter duration, i.e., they will choose to play fewer rounds with that Partner.

2. Results

Summary statistics for Experiment 2 are relegated to the Appendix. In OLS regression models we predict the number of rounds that participants choose to play by whether the partners assigned to the participants were presented as similar or different to them, controlling for demographic characteristics. A "similar" partner is a partner with homophily score 4 (i.e., similar to the respondent on all four dimensions) and a "different" partner is a partner with homophily score 0 (i.e., different from the respondent on all 4 dimensions). In Table 7, we first report results on the full sample (model 1, without controls). We then report results on the subsample of Participants who are in the top three quartiles in terms of risk perceptions (model 2 without controls and model 3 with controls). Participants varied in their perceptions of how risky the game was; mean = 58.105 median = 51, lower end of 2nd quartile = 41, SD = 29.205, on a 1-100 scale.) In all models, 'different partner assigned' is the reference category.

Table 7: OLS Regression Models Predicting the Number of Rounds by Experimental Condition

	Model 1 Full sample	Model 2 "this game is risky" (top 75%)	Model 3 "this game is risky" (top 75%)
Partner assigned: similar	0.758	1.325**	1.219*
	(0.560)	(0.659)	(0.698)
N	143	107	107

* p<0.1, **p<0.05, ***p<0.01

In the full sample (model 1), we find that participants who were assigned to a "similar" partner chose to play 0.76 more rounds, on average, with their partner, compared to participants who were assigned to a "different" partner, but this difference is only marginally significant. In the subsample of Participants who are in the top 75% in terms of risk perceptions (models 2 and 3), the difference increases in magnitude—to approximately 1.2-1.3 rounds—and becomes statistically significant. These results support Hypothesis 2: When assigned a "different" partner, Participants choose a shorter duration, i.e., a smaller number of rounds.

D. Experiment 3: Learning

1. Design and Hypotheses

While the preceding experiments focused on the static relationship between duration and perceived risk, Experiment 3 studies the dynamic learning effect. A Partner is assigned for rounds 1-5; then the behavior of the Partner—how much money the Partner transferred back to the Participant—is revealed to the Participant; finally, the Participant is asked to choose traits for a new Partner for rounds 6-10. In the control group, a Partner is assigned for rounds 1-5 and then the Participant is asked to choose a new Partner for rounds 6-10, but without the intermediate 'learning' stage i.e., the behavior of the assigned Partner in rounds 1-5—how much money the Partner transferred back to the Participant—is not revealed to the Participant. Specifically, we have the following treatment groups:

- Treatment 1: For rounds 1-5, the Participant was assigned a "very similar" Partner (homophily score = 4), the same Partner for all five rounds, and was told who the algorithm matched for them, e.g., "You were randomly assigned to play with a female partner, who is 40 years old. She is married and is from Colorado." After the first 5 rounds, the Participant was told: "Your partner transferred back to you $Y, which is X% of the money that you transferred to him/her." The parameter X gets three possible values: (a) 50%, (b) 100%, or (c) 150%. For rounds 6-10, the Participant chose one Partner and played the game with that same Partner for those five rounds.

- Treatment 2: Same as Treatment 1, only that for rounds 1-5, the Participant was assigned a "very different" Partner (homophily score = 0).

We thus have six sub-treatments, which are summarized in Table 8.

Table 8: Experiment 3 Treatments

	X = 50%	X = 100%	X = 150%
Similar Partner	Treatment 1(a)	Treatment 1(b)	Treatment 1(c)
Different Partner	Treatment 2(a)	Treatment 2(b)	Treatment 2(c)

Let $H(Partner,\ X)$ denote the homophily score that the Participant chose for the Partner in rounds 6-10, given the identity of the Partner ("similar" or "different") in rounds 1-5 and the amount of money that this Partner transferred back (X).[62] We can now state the following hypothesis.[63]

[62] To be more precise, the probability P_d is also a function of the amount that the Participant transferred to the Partner.
[63] Beyond Hypothesis 3, we were also interested in comparing the magnitudes of different learning effects that run counter to Participants' prior beliefs. Specifically, assume that Participants come into the experiment with a prior belief that "similar" Partners are more trustworthy and that "different" Partners are less trustworthy. Is the learning effect when a "similar" Partner behaves in an untrustworthy way (transfers back only 50%) equal to the learning effect when a "different" Partner behaves in a trustworthy way (transfers back 150%)? Or is there more learning when a "similar" partner behaves in an untrustworthy way, because (i) a negative signal ("similar" partner transfers back only 50%) triggers more learning than a positive signal ("different" partner transfers back 150%), or (ii) it is more difficult to counter a negative (untrustworthy) prior. We have no clear hypothesis either way, and thus present these questions for exploration, rather than state specific hypotheses. We also recognize that the comparison is "noisy"; if Participants do not perceive a 50% reduction (from 100% to 50%) as equivalent to a 50% increase (from 100% to 150%), then we would be comparing apples and oranges. In any event, the results, as described below, did not allow for a meaningful exploration of these questions.

Hypothesis 3:

(a) Learning about a “different” Partner: For a Participant who was assigned a “different” Partner for rounds 1-5, $H(\text{"different"},\ 150\%) < H(\text{"different"},\ 100\%) < H(\text{"different"},\ 50\%)$. And adding the Control: $H(\text{"different"},\ 150\%) < H(\text{"different"},\ Control) < H(\text{"different"},\ 50\%)$.[64]

(b) Learning about a “similar Partner”: For a Participant who was assigned a “similar” Partner for rounds 1-5, $H(\text{"similar"},\ 150\%) > H(\text{"similar"},\ 100\%) > H(\text{"similar"},\ 50\%)$. And adding the Control: $H(\text{"similar"},\ 150\%) > H(\text{"similar"},\ Control) > H(\text{"similar"},\ 50\%)$.[65]

2. Results

Summary statistics for Experiment 3 are relegated to the Appendix. In Table 9, we report OLS regression models that predict the homophily score by whether the partners assigned in the previous five rounds were relatively similar (model 1 without controls and without the amounts that the partners transferred back, and model 2 with controls and with the amounts that the partners transferred back).

Table 9: OLS Regression Models Predicting the Homophily Score by Experimental Condition

	Model 1 Homophily score	Model 2 Homophily score
Similar partner assigned	1.104*** (0.276)	1.0567*** (0.288)
100% back		-0.517 (0.394)
150% back		-0.570 (0.333)

* p<0.1, **p<0.05, ***p<0.01

We see that participants who were assigned a “similar” partner in rounds 1-5 were more likely to select a “similar” partner (i.e., higher homophily score) in rounds 6-10; and participants who were assigned a “different” partner in rounds 1-5 were more likely to select a “different” partner (i.e., lower homophily score) in rounds 6-10. This is true regardless of the amount of money that the assigned partner transferred back (interactions were statistically non-significant).

These results do *not* confirm Hypothesis 3. In particular, we did not confirm the optimistic hypothesis that a short-term (contractual) interaction with a “different” Partner would allow

[64] We do not have a clear hypothesis w.r.t. the relationship between $H(\text{"different"}, Control)$ and $H(\text{"different"}, 100\%)$.

[65] We do not have a clear hypothesis w.r.t. the relationship between $H(\text{"similar"}, Control)$ and $H(\text{"similar"}, 100\%)$.

Participant a chance to learn that the "different" Partner is trustworthy, i.e., transfers back a large amount of money, and then enter into further (contractual) interactions with this or another "different" Partner. However, our data is consistent with a different optimistic story: Once a person interacts with a partner who is 'different,' he will be more likely to interact with a person who is "different" in the future, regardless of how trustworthy the "different" person was, i.e., regardless of the amount that the Partner transferred back.[66]

E. Summary

We find that when duration is longer, participants tend to choose partners who are more similar to them, and that participants who are assigned less-similar partners choose to play a smaller number of rounds. Causation seems to run in both directions. In terms of learning, we were hoping to find that a short-term (contractual) interaction with a less-similar partner would allow participants to learn that less-similar partners are trustworthy, i.e., behave cooperatively in the trust game, leading these participants to then enter into further (contractual) interactions with the same or another less-similar partner. Our data is not consistent with this optimistic story. But it is consistent with a different, though still optimistic, story: Once a person interacts with a less-similar partner, he or she will be more likely to interact with less-similar partners in the future, regardless of how trustworthy the first less-similar partner was. These findings complement the results we obtained with the survey data (Part II) and provide evidence for a causal relationship between duration and (perceived) risk.

IV. Policy Implications

Our findings suggest the need to reevaluate a main tenant of US housing policy—the almost undisputed goal of encouraging homeownership. If short-term, rental contracts can be more conducive to racial integration, then policymakers should reconsider the strong policy preference for homeownership. We begin, in Section A, by describing the main homeownership-promoting federal policies—the policies that may need to be reevaluated. Next, in Section B, we turn to federal policies that support the rental option. Unlike the pro-homeownership policies, rent-assistance programs are largely focused on low-income individuals and families, for whom homeownership is a distant dream. To promote racial integration, based on this Article's findings, federal policy should expand rental-assistance programs beyond its current focus on low-income renters, funding this expansion by scaling down homeownership assistance programs. Finally, Section C considers more innovative, and more nuanced, policies that combine homeownership-assistance with rental assistance in a dynamic program designed to encourage the learning effect

[66] Our data suggest that a Participant who received a large amount of money (150%) back from any Partner, "similar" or "different," in rounds 1-5 is more likely to choose a different Partner for rounds 6-10 (as compared to a Participant gets only 50%, which is the omitted category in our analysis). This particular learning effect seems to be indifferent to type. Perhaps Participants learn, or think they learn, something about the pool of Partners in the experiment. Perhaps Participants who have more money, having just received a large amount in rounds 1-5, are less anxious about losing money and worry less about the risks associated with a "different" Partner. Or perhaps Participants who received a large amount of money in rounds 1-5 are in a good, trusting mood, and are thus willing to trust both "similar" and "different" Partners.

that we identified. Specifically, such a Rent-to-Buy program may induce renting for an initial period and then support first-time homeowners who have completed the initial renting period.[67]

Before we proceed, it is important to acknowledge the history of residential segregation in the United States and its negative implications for the lives of Black people and of people of color more generally. Critics have accused federal housing policy of creating and perpetuating residential segregation.[68] There have also been attempts to correct course and promote racial integration. One prominent example is the Fair Housing Act (FHA) of 1968, which prohibits racial discrimination in housing, including both sale and rental contracts.[69] The FHA allows plaintiffs to make explicit housing discrimination claims as well as discriminatory effects claims, which have been interpreted by some courts to include segregative-effect claims.[70] Another important attempt to promote integration targets credit contracts, especially mortgage credit contracts, which are critical for homeownership. Specifically, the Equal Credit Opportunity Act, enacted in 1974, prohibits discrimination on the basis of race.[71] And, moving from the individual level to the neighborhood level, the Community Reinvestment Act, enacted in 1977, rates banking institutions on the extent to which they meet the credit needs of borrowers, especially in low- and moderate-income neighborhoods.[72]

And yet, despite these and similar efforts and some positive indicators of desegregation, the overall pattern of desegregation in the US has been uneven, and many metropolitan areas are still as segregated as they were in 1968.[73] Obviously, there is much more to do. Recently, the Poverty and Race Research Action Council has called for a "re-envisioning of our national housing policy" that will include "intentional measures to expand geographic choice and foster diverse communities."[74] The policy proposals discussed in this Part can contribute, together with other initiatives, to this goal of fostering diverse communities.

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[67] The policy implications that we discuss presume that buying and renting differ in how they discourage or encourage racial integration. But policymakers can also attempt to reduce the differences between buying and renting in a way that would encourage integration. Our analysis suggests that buying is less conducive to integration, because of the contract's long-term duration. If we can reduce the duration of home-buying contracts, e.g., by reducing the transaction costs that accompany the buying and selling of real-estate (including the costs of moving), then perhaps buying may no longer be associated with more segregation than renting.

[68] *See*, *e.g.*, SHERYLL CASHIN, FAILURES OF INTEGRATION: HOW RACE AND CLASS ARE UNDERMINING THE AMERICAN DREAM (New York, PublicAffairs, 2005); Poverty and Race Research Action Council (PRRAC), *A Vision for Federal Housing Policy in 2021 and Beyond*, 2 (July 15, 2020) (https://www.prrac.org/a-vision-for-federal-housing-policy-in-2021-and-beyond/). For example, PRRAC criticized the rental voucher programs for "effectively steer[ing] many families into lower-opportunity, higher-poverty communities." *Id*.

[69] Fair Housing Act, Pub. L. No. 90-284, 82 Stat. 81 (1968) (codified as amended at 42 U.S.C. §§ 3601–3631 (2012)).

[70] *See* Huntington Branch, NAACP v. Town of Huntington, 844 F.2d 926, 937 (2d Cir. 1988); Robert G. Schwemm, *Segregative-Effect Claims Under the Fair Housing Act*, 20 N.Y.U. J. LEGIS & PUB POL'Y. 709, 714 (2017). Such segregative-effect claims require housing segregation in the relevant area as well as a policy that "creates, increases, reinforces, or perpetuates" the segregation. *Id*.

[71] 5 U.S.C. §§ 1691-1691f.

[72] 12 U.S.C. §§ 2901 et seq.

[73] *See* Douglas S. Massey, *The Legacy of the 1968 Fair Housing Act*, 30 SOCIO. F. 571 (2015).

[74] *See* Megan Haberle & Philip Tegeler, *A Call to Remedy Segregation and Advance Housing Justice: Federal Strategies for 2021 and Beyond*, 29 POVERTY AND RACE 7 (2020).

A. The Hidden Cost of Promoting Homeownership

For decades, the idea of homeownership has been a central aspect of the American dream.[75] Homeownership is believed to generate real economic benefits, as a high-return investment and an engine of upward mobility.[76] It also carries powerful psychological and emotional benefits. In a June 2001 speech, President George W. Bush declared: "[H]omeownership lies at the heart of the American Dream. It is a key to upward mobility for low- and middle-income Americans. It is an anchor for families and a source of stability for communities. It serves as the foundation of many people's financial security. And it is a source of pride for people who have worked hard to provide for their families."[77] Moreover, because of this view that homeownership can serve as an engine of upward mobility, President Bush and others have sought to frame the federal support for homeownership as a way to reduce racial inequality.[78] There is reason to doubt the effectiveness of pro-homeownership policies in reducing racial wealth gaps.[79] And as long as most people of color, especially Black and Latinx families, are tenets and not homeowners, the policies and legal doctrines that systematically favor homeownership systematically harm people of color.[80]

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[75] *See*, *e.g.*, Reid, *supra* note 12, at 143-171. Homeownership has become an "aspirational" goal in American culture—something viewed as a requirement for achievement of the "American dream." *See*, *e.g.*, Drew, *supra* note 12; Bracha & Julian, *supra* note 12; Laurie S. Goodman & Christopher Mayer, *Homeownership and the American Dream*, 32 J. ECON. PERP. 31, 31-32 (2018) ("For decades, it was taken as a given that an increased homeownership rate was a desirable goal…. But after the financial crises and Great Recession, in which roughly eight million homes were foreclosed on and about $7 trillion in home equity was erased, economists and policymakers are re-evaluating the role of homeownership in the American Dream." Still, Goodman & Mayer conclude that homeownership should continue to play a central role in the American Dream.) Similar questions about the post-crisis future of homeownership-promoting policies were raised by Maggie McCarty, Libby Perl & Katie Jones, CONG. RSCH. SERV., RL34591, OVERVIEW OF FEDERAL HOUSING ASSISTANCE PROGRAMS AND POLICY (2019) (available at https://crsreports.congress.gov/product/pdf/RL/RL34591) (observing, in the Abstract, that "given the severe downturn in U.S. housing markets that began in 2007 and the resulting high foreclosure rate, it is unclear to what degree federal policy will continue to focus on increasing access to homeownership.") The support for policies that encourage homeownership crosses party lines. For example, in May 1995, President Bill Clinton released the National Homeownership Strategy with the goal that it would "boost homeownership in America to an all-time high by the end of the century." And in 2003, President George W. Bush signed the American Dream Downpayment Initiative to assist first-time homebuyers with obtaining a down payment. *see* Goodman & Mayer, *id*.

[76] *See* Goodman & Mayer, *id*. For an overview of the literature on the benefits and risks of homeownership, *see* William M. Rohe & Mark Lindblad, *Reexamining the Social Benefits of Homeownership after the Housing Crisis*, JOINT CENTER FOR HOUSING STUDIES OF HARVARD UNIVERSITY (2013), available at http://www.jchs.harvard.edu/sites/default/files/hbtl-04.pdf.

[77] The White House Archives – President George W. Bush, Home Ownership Policy Book (2002), https://georgewbush-whitehouse.archives.gov/infocus/homeownership/homeownership-policy-book-background.html.

[78] *See* Goodman & Mayer, *supra* note 75, at p. 31; McCarty, Perl & Jones, *supra* note 75, Abstract ("In the past, lagging homeownership rates among low-income and minority households have prompted several Presidents to promote homeownership-based housing policies.").

[79] The extent that homeownership is promoted as a vehicle for reducing racial inequality by building the wealth of minority homeowners, the likelihood of success might be quite small. *See* Goodman & Mayer, *supra* note 75, at 32. As Goodman & Mayer explain: "the internal rate of return to homeownership is quite favorable compared to alternative investments…. However, the ability to build wealth through homeownership is dependent on holding on to the home during downturns; lower-income and minority borrowers are less likely to maintain homeownership through the cycle, and thus benefit less from homeownership."

[80] *See*, *e.g.*, Sarah Schindler & Kellen Zale, *The Anti-Tenancy Doctrine,* 171 U. PA. L. REV. (forthcoming in 2023).

In any event, our focus here is different (though not unrelated). We seek to reconsider the effect of housing policy on residential segregation. Our findings suggest that policies promoting homeownership might have a hidden cost, if the long-duration aspect of homeownership discourages racial integration. We begin by surveying these policies.

Policymakers have sought to encourage homeownership through a broad range of policies and legal doctrines, ranging from land use law, housing law, constitutional law, safety law, consumer protection and contract law, and tax law.[81] Perhaps the most prominent policy is the mortgage-interest tax deduction, which allows homeowners to deduct the interest paid on their mortgage (subject to caps) from their taxable income, thus reducing their tax liability.[82] The deduction is one of the largest tax expenditures in the United States.[83] It amounts to approximately 7 percent of total personal income tax payments.[84] Interestingly, it is not clear that this prominent "pro-homeownership" policy actually increases homeownership. Indeed, economists have concluded that the mortgage interest deduction increases home prices—as the reduced cost of borrowing inflates the demand for housing—and thus reduces homeownership rates.[85] Moreover, the mortgage interest deduction is considered regressive: high-income households are the primary beneficiaries of the deduction in its current form, because these households are more likely to buy houses and to take larger mortgages, and because they are subject to higher marginal tax rates under our progressive tax system.[86]

Beyond the mortgage interest deduction, homeowners benefit from the ability to deduct property taxes.[87] They also benefit from the absence of a capital gains tax on the increase in the value of their homes.[88] Another important tax benefit helps homeowners indirectly: The federal government authorizes state and local governments to issue mortgage revenue bonds (MRBs),

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[81] *See* Schindler & Zale, *id*.
[82] *See* Maggie McCarty, Libby Perl & Katie Jones, *supra* note 75, at 28; Goodman & Mayer *supra* note 75, at 32 ("the internal rate of return to homeownership is quite favorable compared to alternative investments…. While this result does not depend only on favorable tax treatment, tax subsidies certainly help increase the financial benefits of homeownership….").
[83] *See* Kamila Sommer & Paul Sullivan, *Implications of US Tax Policy for House Prices, Rents, and Homeownership*, 108 AM. ECON. REV. 241 (citing a 2012 Joint Committee on Taxation report: JOINT COMM. ON TAXATION, Report JCS-1-12, ESTIMATES OF FEDERAL TAX EXPENDITURES FOR FISCAL YEARS 2011– 2015 (2012)); Maggie McCarty, Libby Perl & Katie Jones, *supra* note 75 : "In FY2018, the Joint Committee on Taxation estimated that the mortgage interest deduction would result in a $33.7 billion tax expenditure.") *See also* Henderson J. Vernon & Yannis M. Ioannides, Owner Occupancy: Investment vs Consumption Demand, 21 J. UR. ECON. 228 (1987). *See* Publication 936 (2022), Home Mortgage Interest Deduction, Internal Revenue Service (2020), https://www.irs.gov/publications/p936#:~:text=The%20interest%20you%20pay%20on,interest%20and%20isn't%20deductible.
[84] *See* Sommer & Sullivan, *id*.
[85] *See* Sommer & Sullivan, *id*, at 242, estimate that eliminating the mortgage interest deduction would result in lower house prices and higher homeownership rates; they explain that "the repeal of the mortgage interest deduction decreases house prices because, ceteris paribus, the after-tax cost of occupying a square foot of housing has risen." *See also* David E Rappoport*, Do Mortgage Subsidies Help or Hurt Borrowers?* BOARD OF GOVERNORS OF THE FEDERAL RESERVE SYSTEM FINANCE AND ECONOMICS DISCUSSION SERIES, 2016-081 (2016) (showing that the mortgage interest deduction increases house prices).
[86] *See* Sommer & Sullivan, *supra* note 83, at 242-243.
[87] *See* Michael S. Carliner, *Development of Federal Homeownership "Policy"*, 9 HOUSING POL'Y DEBATE 299, 301 (1998).
[88] *See* Goodman & Mayer, *supra* note 75, at 32. Another benefit is the lack of taxation on imputed rent. *See* Goodman & Mayer, *id*. at 32.

which are exempt from federal taxes. As explained by McCarty, Perl and Jones, in their Congressional Research Service report: "State or local governments sell MRBs to investors. Because the interest earned by bondholders is exempt from federal (and sometimes state) taxation, the bonds can be marketed at lower interest rates than would be required for similar taxable instruments. The proceeds of the bond sales [] are used to finance home mortgages to eligible (generally first-time) homebuyers. In effect, the tax exemption on the bonds provides an interest rate subsidy to homebuyers."[89] The federal government further reduces mortgage interest rates by supporting the mortgage market through the work of the government-sponsored enterprises, Fannie Mae and Freddie Mac, which buy mortgages from banks and sell mortgage-based securities to investors.[90]

Another policy that reduces mortgage costs is the Federal Housing Administration's (FHA's) mortgage insurance program. The FHA insures private lenders against the risk of borrower default, which provides an incentive for these lenders to make loans to borrowers who might not otherwise be served by the private market.[91] In addition, the federal government provides flexible, block grants to state and local governments that can be used to help homebuyers.[92] Our findings suggest that these policies entail an unappreciated cost—they might discourage residential integration. This cost provides a reason to reevaluate the range and magnitude of policies that encourage homeownership.[93]

B. Another Benefit of Rental Housing Assistance

The many pro-homeownership policies notwithstanding, there are also important government programs that support the main alternative to homeownership, namely, renting. The federal government provides flexible, block grants that can be used to help low-income renters. Perhaps the most prominent program funded by such grants is the Housing Choice Vouchers program. This program includes tenant-based assistance, whereby tenants receive vouchers that help cover rent payments and can choose where to apply them, i.e., where to rent an apartment, among participating properties. The program also includes project-based rental assistance that subsidizes rent payments in specific housing projects.[94] The federal government also provides tax incentives for the development of affordable rental housing.[95]

The existing policies that provide rental assistance are designed mainly to help low-income households, for whom homeownership is an unlikely alternative. Our findings suggest that the

[89] *See* Maggie McCarty, Libby Perl & Katie Jones, *supra* note 75, at 17. MRBs are codified at 26 U.S.C. §143.
[90] *See* McCarty, Perl & Jones, *supra* note 75, at 23.
[91] *Id*. at Abstract.
[92] *Id*. at 1, 9.
[93] Other policies that support homeownership, in addition to the homeownership promoting policies discussed above, include: policies that lower interest rates in the economy and thus reduce the cost of mortgages (*see* James R. Follain & David C. Ling*, Another Look at Tenure Choice, Inflation, and Taxes*, 16 REAL EST. ECON. 207 (1988)); regulation of lenders and of mortgage products that affects the availability of credit; and planning and zoning laws that can affect the supply of owner occupied vs. rental units (*see* Fionnuala Earley, *What Explains the Differences in Homeownership Rates in Europe?* 19 HOUS. FIN. INT'L. 25 (2004)).
[94] *See* McCarty, Perl & Jones, *supra* note 75, at 1, 9, 10; The vouchers program is codified at 42 U.S.C. §1437f(o).
[95] *See* McCarty, Perl & Jones, *supra* note 75, at 16; The LIHTC program was enacted as part of the Tax Reform Act of 1986 (P.L. 99-514); it is codified at 26 U.S.C. §42.

renter status of these households, which is associated with shorter-duration contracts, facilitates residential integration. But what about the higher-income households who face an actual choice between renting and buying? It is for these households that government policy can shift the decision from renting to buying, or vice versa. Current policy pushes higher-income households to buy, with substantial pro-homeownership policies and limited rental assistance. Our findings provide a reason to rethink the heavy thumb on the scale in favor of homeownership. Specifically, shifting resources from homeownership-encouraging programs to rental assistance may contribute to residential integration.

C. Dynamic Rent-to-Buy Policies

Thus far we have considered policies that support homeownership and policies that provide rental assistance, and we have argued that our empirical findings suggest a need to reevaluate existing policies and possibly rebalance towards less support for homeownership and more rental assistance. We now consider a more nuanced approach that relies on the learning effect that we have identified. The optimistic version of this learning effect implies that households who start as renters will be more likely to rent in diverse neighborhoods and realize that diversity is not a cost but rather a benefit; when these households later choose to buy a home, they will be more likely to buy in a diverse neighborhood. Accordingly, policymakers should encourage renting initially, and support a transition to homeownership after a minimal rental period.

In practice, such a rent-to-buy policy can be implemented in several ways. Perhaps most straightforward is a policy that retains many of the existing programs that support homeownership but makes eligibility for these programs conditional on a prior period of renting.[96] (The eligibility condition can be made even stricter, insisting on a prior period of renting in a diverse neighborhood.) It should be noted that many existing policies already make the benefit conditional;[97] and so it should not be difficult, from an implementation perspective, to add a prior-rental condition. Another way to implement a rent-to-buy policy is to provide rental assistance for an initial period and then allow for easy transition into a homeownership support program.

V. Conclusion

In this Article, we explored the interaction between contractual duration and other important contractual choices. Focusing on housing contracts, we presented evidence—survey evidence and experimental evidence—suggesting that parties entering shorter-duration, rental contracts are

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[96] We thank Shay Lavie for suggesting this reform.

[97] *See* McCarty, Perl & Jones, *supra* note 75, at 35 ("Some of these programs and activities [that support homeownership] benefit a broad range of homebuyers (e.g., the favorable tax treatment of homeownership, secondary market institutions that support the mortgage market) while others focus specifically on homebuyers who face certain barriers to homeownership (e.g., federal mortgage insurance and guaranty programs, grant programs that can be used for down payment or closing cost assistance)." Perhaps the most relevant example is the conditioning of the lower-interest-rate benefit provided by MRBs. *Id*, at 17 ("To qualify for the benefit, a borrower must not have been a homeowner in the past three years, the mortgage must be for the principal residence of the borrower, the purchase price may not exceed 90% (110% in targeted areas) of the average purchase price in the area, and the income of the borrower may not exceed 110% (140% in targeted areas) of the median income for the area.")

willing to take on greater perceived risk and move into more diverse neighborhoods, whereas parties entering longer-duration, home-buying contracts tend to choose less diverse neighborhoods. These findings, we argued, require a reevaluation of the strong preference, in U.S. housing policy, for buying over renting, at least to the extent that our housing policy is, or should be, designed to promote racial desegregation.

The insights that we develop about the interaction between contractual duration and other important contractual choices can be generalized and applied beyond the housing context. For example, in the employment context, we could compare short-duration "at will" employment or gig-work to long-duration employment with "for cause" termination or with long-term union contracts. If short-duration arrangements promote the hiring of a more diverse work force, then this should inform ongoing debates about the definition of "employee" (as compared to "independent contractor"), the rights of gig-workers, etc. We leave it to future work to study the positive and normative implications of contractual duration beyond the housing context.

Appendix

The Appendix collects additional results from our analysis of the survey data (Section A) and from the online, trust-game experiments (Section B).

A. Survey Data: Additional Results

Tables A1, A1a and A1b provide additional descriptive statistics. Tables A2-A5 present regression results similar to those reported in Tables 2-5, but with additional controls. (The regressions reported in Tables A2-A5 include all of the controls discussed in the text. Similar results obtained, when each control is added separately.)

Table A1: Initial Housing Choice – Descriptive Statistics, All variables

	Mean	SD	Min.	Max.	N
Own	0.11	0.31	0.00	1.00	4845
Median value of house in county (USD per sqft)	130.59	104.60	23.92	1167.50	5120
Housing price index in the county	129.30	33.35	71.31	367.17	5778
Number of residents in county (in millions)	1.11	2.13	0.00	19.65	5909
Percentage of white residents in county	0.65	0.21	0.07	0.99	5909
Average % of white residents in county, Past residences	0.67	0.21	0.09	0.99	8625
Married	0.18	0.38	0.00	1.00	5896
College education	0.26	0.44	0.00	1.00	5873
Household income (in thousands of dollars)	40.05	56.78	0.00	421.37	4884
Respondent's income (in thousands of dollars)	14.99	14.01	0.00	180.33	4779
Number of children in household	0.31	0.66	0.00	6.00	5963
Female	0.49	0.50	0.00	1.00	8625
Age	22.23	3.25	14.00	35.00	5974
White	0.50	0.50	0.00	1.00	8625
Black	0.26	0.44	0.00	1.00	8625
Latino/Latina	0.11	0.31	0.00	1.00	8625
Asian	0.02	0.13	0.00	1.00	8625
Other	0.12	0.32	0.00	1.00	8625
Crime rate index in the county	0.01	0.00	0.00	0.02	5190
Unemployment rate in county	0.06	0.02	0.02	0.20	5783
Small town	0.01	0.09	0.00	1.00	8625
Large town	0.14	0.35	0.00	1.00	8625
Medium city	0.16	0.36	0.00	1.00	8625
Large city	0.20	0.40	0.00	1.00	8625
Mega city	0.49	0.50	0.00	1.00	8625

Table A1a: Initial Housing Choice – Descriptive Statistics – for Renters and Buyers Separately, All variables[98]

	Rent				Own					
	Mean	SD	Min.	Max.	Mean	SD	Min.	Max.	Diff.	
Median value of house in county (USD per sqft)	131.58	106.10	23.92	1142.0	123.08	93.64	31.83	1167.5	8.500	(5.45)
Housing price index in the county	130.89	33.41	71.31	367.17	131.75	33.73	77.46	274.84	-0.859	(1.64)
Number of residents in county (in millions)	1.16	2.21	0.00	19.65	0.85	1.86	0.00	19.65	0.311^{***}	(0.10)
Percentage of white residents in county	0.63	0.22	0.07	0.99	0.69	0.21	0.10	0.99	-0.055^{***}	(0.01)
Average % of white residents in county, Past residences	0.66	0.21	0.09	0.99	0.70	0.21	0.10	0.99	-0.047^{***}	(0.01)
Married	0.16	0.36	0.00	1.00	0.43	0.50	0.00	1.00	-0.278^{***}	(0.02)
College education	0.23	0.42	0.00	1.00	0.46	0.50	0.00	1.00	-0.228^{***}	(0.02)
Household income (in thousands of dollars)	38.23	53.28	0.00	421.37	59.01	69.40	0.00	343.97	-20.775^{***}	(2.86)
Respondent's income (in thousands of dollars)	14.98	13.62	0.00	180.33	21.97	18.53	0.00	146.00	-6.989^{***}	(0.74)
Number of children in household	0.35	0.70	0.00	6.00	0.38	0.72	0.00	5.00	-0.033	(0.03)
Female	0.50	0.50	0.00	1.00	0.58	0.49	0.00	1.00	-0.074^{***}	(0.02)
Age	22.44	3.19	14.00	35.00	23.41	3.48	16.00	35.00	-0.975^{***}	(0.15)
White	0.45	0.50	0.00	1.00	0.62	0.49	0.00	1.00	-0.169^{***}	(0.02)
Black	0.30	0.46	0.00	1.00	0.17	0.37	0.00	1.00	0.131^{***}	(0.02)
Latino/Latina	0.11	0.32	0.00	1.00	0.11	0.31	0.00	1.00	0.003	(0.01)
Asian	0.02	0.13	0.00	1.00	0.02	0.12	0.00	1.00	0.002	(0.01)
Other	0.12	0.33	0.00	1.00	0.09	0.28	0.00	1.00	0.033^{**}	(0.01)
Crime rate index in the county	0.01	0.00	0.00	0.02	0.00	0.00	0.00	0.02	0.001^{***}	(0.00)
Unemployment rate in county	0.06	0.02	0.02	0.19	0.06	0.03	0.02	0.18	-0.003^{***}	(0.00)
Small town	0.01	0.09	0.00	1.00	0.02	0.14	0.00	1.00	-0.011^{***}	(0.00)
Large town	0.18	0.38	0.00	1.00	0.24	0.43	0.00	1.00	-0.063^{***}	(0.02)
Medium city	0.19	0.39	0.00	1.00	0.27	0.44	0.00	1.00	-0.078^{***}	(0.02)
Large city	0.27	0.44	0.00	1.00	0.22	0.41	0.00	1.00	0.052^{**}	(0.02)
Mega city	0.36	0.48	0.00	1.00	0.26	0.44	0.00	1.00	0.101^{***}	(0.02)
Observations	4314				531				4845	

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

[98] Note that, in Table A1a, the number of observations is 4,845 (4,314 renters and 531 buyers). From Table A1, we see that ownership (i.e., rent v. buy) information is available for only 4,845 respondents, out of the 8,625 respondents in the data. A similar relationship exists between.

Table A1b: Initial Housing Choice – Descriptive Statistics – for White Renters and Buyers Separately, All variables

	Rent				Own					
	Mean	SD	Min.	Max.	Mean	SD	Min.	Max.	Diff.	
Median value of house in county (USD per sqft)	123.14	101.58	23.92	1142.0	109.90	63.40	31.83	549.75	13.238**	(6.54)
Housing price index in the county	128.16	31.21	71.31	367.17	126.18	27.87	77.46	243.61	1.981	(1.93)
Number of residents in county (in millions)	0.72	1.42	0.01	19.65	0.55	0.99	0.00	5.36	0.172**	(0.08)
Percentage of white residents in county	0.74	0.18	0.12	0.99	0.76	0.17	0.14	0.99	-0.026**	(0.01)
Average % of white residents in county, Past residences	0.78	0.16	0.15	0.99	0.80	0.15	0.33	0.99	-0.017*	(0.01)
Married	0.16	0.37	0.00	1.00	0.45	0.50	0.00	1.00	-0.290***	(0.02)
College education	0.29	0.46	0.00	1.00	0.50	0.50	0.00	1.00	-0.209***	(0.03)
Household income (in thousands of dollars)	42.61	59.02	0.00	421.37	56.30	65.52	0.00	343.97	-13.693***	(3.94)
Respondent's income (in thousands of dollars)	14.79	12.83	0.00	104.35	20.37	16.39	0.00	88.00	-5.583***	(0.88)
Number of children in household	0.17	0.47	0.00	6.00	0.24	0.54	0.00	4.00	-0.071**	(0.03)
Female	0.48	0.50	0.00	1.00	0.59	0.49	0.00	1.00	-0.106***	(0.03)
Age	22.15	2.80	15.00	35.00	23.12	3.23	17.00	35.00	-0.970***	(0.17)
White	0.00	0.00	0.00	0.02	0.00	0.00	0.00	0.02	0.000	(0.00)
Black	0.06	0.02	0.02	0.17	0.06	0.02	0.02	0.17	-0.001	(0.00)
Latino/Latina	0.01	0.11	0.00	1.00	0.03	0.16	0.00	1.00	-0.015**	(0.01)
Asian	0.23	0.42	0.00	1.00	0.29	0.45	0.00	1.00	-0.062**	(0.03)
Other	0.25	0.43	0.00	1.00	0.29	0.45	0.00	1.00	-0.039	(0.03)
Crime rate index in the county	0.24	0.43	0.00	1.00	0.23	0.42	0.00	1.00	0.012	(0.03)
Unemployment rate in county	0.27	0.44	0.00	1.00	0.17	0.37	0.00	1.00	0.104***	(0.03)
Small town	123.14	101.58	23.92	1142.00	109.90	63.40	31.83	549.75	13.238**	(6.54)
Large town	128.16	31.21	71.31	367.17	126.18	27.87	77.46	243.61	1.981	(1.93)
Medium city	0.72	1.42	0.01	19.65	0.55	0.99	0.00	5.36	0.172**	(0.08)
Large city	0.74	0.18	0.12	0.99	0.76	0.17	0.14	0.99	-0.026**	(0.01)
Mega city	0.78	0.16	0.15	0.99	0.80	0.15	0.33	0.99	-0.017*	(0.01)
Observations	1953				330				2283	

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A2: Initial Housing Choice – Logistic Regression Models Predicting Rent vs. Buy Decisions

	All	White respondents	Non-White respondents
Percentage of whites in county	0.250***	0.337**	0.221***
	(0.08)	(0.15)	(0.08)
Median value of house in county (USD per sqft)	-0.000	0.000	-0.000
	(0.00)	(0.00)	(0.00)
Crime rate index in the county	2.933	11.230*	0.778
	(3.15)	(5.88)	(3.35)
Housing price index in the county	0.000	-0.001	0.000*
	(0.00)	(0.00)	(0.00)
Number of residents in county (in millions)	0.000	-0.015	0.001
	(0.00)	(0.02)	(0.00)
City (d)	-0.022	-0.032	-0.014
	(0.02)	(0.04)	(0.02)
Unemployment rate in county	-0.097	-0.737	0.141
	(0.33)	(0.63)	(0.32)
Married (d)	0.130***	0.084**	0.153***
	(0.03)	(0.04)	(0.04)
College education (d)	0.041**	0.055*	0.031
	(0.02)	(0.03)	(0.02)
Household income (in thousands of dollars)	0.000***	0.000	0.000**
	(0.00)	(0.00)	(0.00)
Number of children in household	-0.004	-0.014	-0.001
	(0.01)	(0.03)	(0.01)
Female (d)	0.026*	0.059**	0.005
	(0.01)	(0.03)	(0.02)
White (d)	0.019		
	(0.02)		
Other (d)	0.003		-0.008
	(0.02)		(0.02)
Age	0.009***	0.019***	0.003
	(0.00)	(0.01)	(0.00)
Average % of whites in county, Past residences	-0.129*	-0.162	-0.126*
	(0.07)	(0.13)	(0.07)
First move was for college (d)	0.003	-0.025	0.030
	(0.02)	(0.03)	(0.02)
Housing price volatility in county	-0.000	0.001	-0.001
	(0.00)	(0.00)	(0.00)
house ownership % in county	-0.112	-0.246**	-0.020
	(0.07)	(0.12)	(0.09)
Observations	1339	539	800

Marginal effects; Standard errors in parentheses
(d) for discrete change of dummy variable from 0 to 1
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A3: Initial Housing Choice — OLS Regression Models Predicting Percentage of White Residents in the County

	All	White respondents	Non-White respondents
Own	0.028***	0.024**	0.030***
	(0.01)	(0.01)	(0.01)
Median value of house in county (USD per sqft)	-0.000	-0.000	0.000
	(0.00)	(0.00)	(0.00)
Crime rate index in the county	-11.796***	-18.562***	-6.856***
	(1.05)	(1.69)	(1.25)
Housing price index in the county	-0.000***	-0.001***	-0.000***
	(0.00)	(0.00)	(0.00)
Number of residents in county (in millions)	-0.003***	-0.008***	-0.003***
	(0.00)	(0.00)	(0.00)
Town	-0.036	0.034	
	(0.07)	(0.07)	
City	0.014**	0.042***	-0.004
	(0.01)	(0.01)	(0.01)
Unemployment rate in county	-0.598***	-0.276	-0.752***
	(0.11)	(0.19)	(0.13)
Married	-0.016**	-0.013	-0.024***
	(0.01)	(0.01)	(0.01)
College education	-0.012*	-0.011	-0.004
	(0.01)	(0.01)	(0.01)
Household income (in thousands of dollars)	0.000	-0.000	-0.000
	(0.00)	(0.00)	(0.00)
Number of children in household	-0.000	0.010	-0.001
	(0.00)	(0.01)	(0.00)
Female	0.004	0.001	0.004
	(0.01)	(0.01)	(0.01)
White	-0.019***		
	(0.01)		
Other	-0.030***		
	(0.01)		
Age	-0.001	-0.002	0.000
	(0.00)	(0.00)	(0.00)
Average % of whites in county, Past residences	0.662***	0.478***	0.735***
	(0.02)	(0.03)	(0.02)
First move was for college	0.005	0.019**	-0.009
	(0.01)	(0.01)	(0.01)
Housing price volatility in county	-0.000	-0.001	-0.000
	(0.00)	(0.00)	(0.00)
house ownership % in county	-0.203***	-0.274***	-0.149***
	(0.03)	(0.04)	(0.03)
Observations	1341	541	800

Marginal effects; Standard errors in parentheses
(d) for discrete change of dummy variable from 0 to 1
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A3a: Logistic Fixed Effects Regression Models Predicting Rent vs. Buy Decisions

	All	White respondents	Non-White respondents
Percentage of whites in county	0.012	-1.495	0.218
	(1.37)	(2.28)	(1.84)
Median value of house in county (USD per sqft)	-0.004	-0.004*	-0.008**
	(0.00)	(0.00)	(0.00)
Crime rate index in the county	30.970	-62.069	33.559
	(58.75)	(99.92)	(74.45)
Housing price index in the county	0.007	0.009	0.009
	(0.00)	(0.01)	(0.01)
Number of residents in county (in millions)	-0.001	0.010	-0.010
	(0.05)	(0.09)	(0.06)
Town (d)	-1.387		-2.634
	(1.86)		(2.35)
City (d)	0.441*	0.671*	0.245
	(0.24)	(0.38)	(0.33)
Unemployment rate in county	-0.211	0.329	-0.701
	(4.79)	(8.03)	(6.16)
Married (d)	1.174***	0.459	1.301***
	(0.23)	(0.39)	(0.29)
College education (d)	0.087	-0.425	0.624
	(0.33)	(0.50)	(0.47)
Household income (in thousands of dollars)	0.001	0.000	0.003
	(0.00)	(0.00)	(0.00)
Number of children in household	0.095	0.181	-0.030
	(0.13)	(0.23)	(0.16)
Age	-0.133	-0.318	-0.018
	(0.21)	(0.35)	(0.28)
Average % of whites in county, Past residences	0.644	2.004	2.647
	(3.11)	(4.85)	(4.41)
Housing price volatility in county	-0.016*	-0.006	-0.024**
	(0.01)	(0.02)	(0.01)
house ownership % in county	-1.363	-0.985	-1.707
	(0.96)	(1.58)	(1.25)
Observations	3880	1891	1982

Marginal effects; Standard errors in parentheses
(d) for discrete change of dummy variable from 0 to 1
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Fixed Effects OLS Regression Models Predicting the Percentage of White Residents in the County

	All	White respondents	Non-White respondents
Own	0.004	0.001	0.004
	(0.00)	(0.00)	(0.00)
Median value of house in county (USD per sqft)	0.000	-0.000	0.000**
	(0.00)	(0.00)	(0.00)
Crime rate index in the county	-19.815***	-22.949***	-18.053***
	(1.24)	(1.93)	(1.57)
Housing price index in the county	-0.000**	-0.000	-0.000***
	(0.00)	(0.00)	(0.00)
Number of residents in county (in millions)	-0.006***	-0.008***	-0.006***
	(0.00)	(0.00)	(0.00)
Town	-0.012	-0.058***	0.013
	(0.03)	(0.01)	(0.02)
City	0.027***	0.038***	0.018***
	(0.00)	(0.01)	(0.01)
Unemployment rate in county	-0.081*	-0.084	-0.075
	(0.04)	(0.07)	(0.06)
Married	-0.000	-0.003	0.001
	(0.00)	(0.01)	(0.00)
College education	-0.012***	-0.014**	-0.010
	(0.00)	(0.01)	(0.01)
Household income (in thousands of dollars)	0.000	0.000	-0.000
	(0.00)	(0.00)	(0.00)
Number of children in household	0.001	0.003	-0.000
	(0.00)	(0.00)	(0.00)
Age	-0.007***	-0.006***	-0.007***
	(0.00)	(0.00)	(0.00)
Average % of whites in county, Past residences	0.183***	0.205***	0.138**
	(0.05)	(0.08)	(0.06)
whether R moved in current round	0.030***		
	(0.01)		
% white residents in past residences X move	-0.045***		
	(0.01)		
Housing price volatility in county	0.000	0.000	0.000
	(0.00)	(0.00)	(0.00)
house ownership % in county	-0.160***	-0.169***	-0.149***
	(0.02)	(0.03)	(0.02)
Observations	18293	7304	10989

Marginal effects; Standard errors in parentheses
(d) for discrete change of dummy variable from 0 to 1
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A5: OLS Regression Models Predicting Percentage of White Residents in County When Respondents Buy Their First House

	All	White respondents	Non-White respondents
Average % of whites in county,	0.430*	0.378	0.673
Past residences	(0.22)	(0.30)	(0.43)
% White residents while living	0.040	0.109	-0.065
with parents	(0.11)	(0.15)	(0.22)
Years living with parents	0.000	0.005	0.004
	(0.01)	(0.01)	(0.01)
% White residents with parents	-0.005	-0.014	-0.010
X years with parents	(0.01)	(0.02)	(0.02)
% White residents while renting	0.111	-0.032	0.379**
	(0.09)	(0.11)	(0.18)
Years renting	-0.020**	-0.024**	-0.001
	(0.01)	(0.01)	(0.01)
% White residents while renting	0.031**	0.036**	-0.001
X years renting	(0.01)	(0.02)	(0.02)
% White residents while in	-0.021	0.025	-0.077
college	(0.11)	(0.17)	(0.17)
Years in college	0.006	0.012	-0.001
	(0.01)	(0.02)	(0.02)
% White residents while in	-0.016	-0.021	-0.013
college X years in college	(0.02)	(0.03)	(0.03)
Median value of house in county	0.000	-0.000	0.000
(USD per sqft)	(0.00)	(0.00)	(0.00)
Crime rate index in the county	-18.937***	-24.648***	-12.713**
	(2.74)	(3.39)	(4.98)
Housing price index in the	-0.001***	-0.001***	-0.000
county	(0.00)	(0.00)	(0.00)
Number of residents in county	-0.006**	-0.008***	-0.007*
(in millions)	(0.00)	(0.00)	(0.00)
City	0.020	0.027	0.011
	(0.01)	(0.02)	(0.02)
Unemployment rate in county	-0.145	-0.157	0.072
	(0.28)	(0.35)	(0.49)
Married	-0.006	-0.008	-0.015
	(0.01)	(0.02)	(0.02)
College education	-0.044	-0.050	-0.073
	(0.06)	(0.09)	(0.09)
Household income (in thousands	0.000	0.000	0.001*
of dollars)	(0.00)	(0.00)	(0.00)
Number of children in household	0.007	-0.009	0.022**
	(0.01)	(0.01)	(0.01)
Female	-0.005	-0.003	-0.001
	(0.01)	(0.01)	(0.02)
White	0.009		
	(0.01)		
Age	-0.006*	-0.003	-0.006
	(0.00)	(0.00)	(0.01)
% White residents in past	-0.000	-0.000	-0.002**
residences X household income	(0.00)	(0.00)	(0.00)
% White residents in past	0.078	0.058	0.206
residences X college education	(0.08)	(0.12)	(0.15)
Housing price volatility in	-0.000	-0.000	-0.000
county	(0.00)	(0.00)	(0.00)
house ownership % in county	-0.247***	-0.265***	-0.188*
	(0.06)	(0.07)	(0.11)
Observations	385	233	152

Marginal effects; Standard errors in parentheses
(d) for discrete change of dummy variable from 0 to 1
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

B. Trust-Game Experiments: Additional Results

Tables B1, B2, B4 and B5 present summary statistics for the three experiments (there are four tables, since Experiment 1 had two versions: Experiment 1 and Experiment 1a). Table B3 presents the regression results for Experiment 1a.

Table B1: Experiment 1: Summary Statistics (Sec. III.B.)

	Mean	SD	Min.	Max.
Homophily Score	9.005	1.645	4	12
Female	0.5			
Age	50.467	15.22	21	84
Marital Status				
Divorced	0.1033			
Living Together	0.0870			
Married	0.5707			
Other Marital Status	0.0217			
Separated	0.0109			
Single	0.2065			
Condition				
One Partner	0.3587			
Two Partners	0.2826			
Ten Partners	0.3587			
N=184				

Table B2: Experiment 1a: Summary Statistics (Sec. III.B.)

	Mean	SD	Min.	Max.
'Transferring money is risky'	60.89	33.24	0	100
Homophily Score	9.54	1.86	4	12
Female	0.60			
Age	37.83	17.08	18	80
Marital Status				
Divorced	0.12			
Living Together	0.13			
Married	0.29			
Other Marital Status	0.02			
Separated	0.02			
Single	0.42			
Condition				
Ten rounds per partner (one partner)	0.33			
Five rounds per partner (two partner)	0.35			
One round per partner (ten partner)	0.32			
N=173				

Table B3: Experiment 1a: Regression Results (Sec. III.B.)

	Full sample	"this game is risky" (top 75%)	"this game is risky" (top 75%)
5 rounds per partner*	-0.455	-0.547	-0.723*
	(0.348)	(0.420)	(0.410)
1 round per partner*	-0.679**	-0.755*	-0.663*
	(0.344)	(0.397)	(0.401)
Female			0.139
			(0.334)
Age			-0.017
			(0.012)
			0
Living Together			0.918
			(0.776)
Married			0.396
			(0.601)
Other Marital Status			-3.508***
			(1.206)
Separated			-0.861
			(1.070)
Single			0.314
			(0.659)
Constant	9.929***	10.047***	10.377***
N	173	129	129

Note: The omitted category is 10 rounds per partner

* p<0.1, **p<0.05, ***p<0.01

Table B4: Experiment 2: Summary Statistics (Sec. III.C.)

	Mean	SD	Min.	Max.
'Transferring money is risky'	58.10	29.20	0	100
Number of Rounds Selected	6.33	3.35	0	10
Female	0.37			
Age	51.45	21.40	18	88
Marital Status				
Divorced	0.07			
Living Together	0.04			
Married	0.60			
Other Marital Status	0.02			
Separated	0.01			
Single	0.26			
Condition				
Partner assigned: similar	0.47			
Partner assigned: different	0.53			
	N=143			

Table B5: Experiment 3: Summary Statistics (Sec. III.D.)

	Mean	SD	Min.	Max.
Homophily Score	8.21	2.07	4	12
Female	0.41			
Age	55.84	22.52	18	89
Marital Status				
Divorced	0.18			
Living Together	0.05			
Married	0.41			
Other Marital Status	0.08			
Separated	0.01			
Single	0.27			
Condition				
Partner assigned: similar	0.47			
50% back	0.33			
150% back	0.39			
100% back	0.28			
	N=210			