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# Explaining residential clustering of fertility

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**Abstract:**
Numerous studies have shown that fertility behavior is spatially clustered. In addition to pure context effects, two causal mechanisms could drive this pattern. First, neighbors may influence each other's fertility behavior, and second, household fertility intentions and behavior may influence residential decisions. This study provides an empirical examination of these two potential causal mechanisms using the sex composition of the two firstborn children and twin births as instrumental variables (IVs) for having a third child. We measure effects of the third child on three separate outcomes: mothers' propensity to move, characteristics of their final neighborhood, and the fertility of their neighbors. Residential and childbearing histories for the years 2000-2018 are drawn from Norwegian administrative registers (N ~ 167,000 women). Individual neighborhoods are defined using time-varying geo-coordinates on place of residence. We identify selective moves as one plausible causal driver of the residential clustering of fertility. The effects are relatively small, though statistically significant. This suggests that the residential clustering of fertility is also driven by factors that we effectively control for in our design – most importantly self-selection based on preferences for a family-oriented life style. Because of the difficulty to measure social interaction effects among neighbors we are reluctant to say that they do not exist, even though we do not identify them. As such, we contribute to the understanding of fertility and relocation, but also to the literature on social interaction effects in fertility by testing the relevance of yet another network, i.e. that of neighbors.

**Keywords:** IV estimation; spatial fertility; k-nearest neighbors; family size; third births

**JEL classification:** J11, J13, R20, R21, R23

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## Sammendrag

Mange studier viser at antallet barn i familier varierer etter hvor man bor. I tillegg til at kjennetegn ved bostedet i seg selv kan påvirke barnetall (rene konteksteffekter), kan to årsaker ligge bak geografiske forskjeller i familiestørrelser. For det første kan naboer påvirke hverandres barnetallsønsker (gjennom smitteeffekter), og for det andre kan (ønsket) familiestørrelse påvirke flyttemønstre (selektiv flytting). Denne studien undersøker disse to siste potensielle årsaksmekanismene empirisk, ved å bruke tvillingfødsler og kjønnssammensetningen til de to førstefødte barna som instrumentvariabler (IV) for å få et tredje barn. Vi måler effekten av det tredje barnet på tre separate utfall: foreldrenes tilbøyelighet til å flytte, egenskaper ved nabolaget der disse foreldrene bor seks år etter andre fødsel og fruktbarhetsadferden til naboene. For analysene bruker vi bostedsadresser og fødsler for årene 2000-2018 fra Folkeregisteret og Matrikkelen (N ~ 167,000 kvinner). Individuelle nabolag defineres ved hjelp av tidsvarierende geokoordinater på bostedet.

Vi identifiserer selektiv flytting som en plausibel årsaksforklaring for geografiske forskjeller i familiestørrelser. Effektene er relativt små, men statistisk signifikante. Dette antyder at den geografiske korrelasjonen i barnetall også skyldes faktorer som vi effektivt kontrollerer for i vårt studiedesign – viktigst av alt seleksjon basert på barnetallsønsker og en familieorientert livsstil. Siden det er vanskelig å måle effekter av sosial interaksjon blant naboer, er vi ikke villig til å si at de ikke eksisterer, selv om vi ikke identifiserer dem i denne studien. Oppsummert bidrar denne studien til forståelsen av fruktbarhet og flytting, men også til litteraturen om effekter av sosial interaksjon for fruktbarhet ved å teste relevansen av ytterligere et nettverk; det som finnes blant naboer.

# 1 Introduction

The spatial clustering of fertility is a well-established demographic finding. A large literature documents that fertility is higher in rural than in urban contexts (Kulu, 2013). Furthermore, within urban regions, fertility is consistently found to be higher in suburbs than in city centers (Kulu, Boyle, & Andersson, 2009; Kulu & Washbrook, 2014). Evidence of the importance of local fertility contexts for fertility behavior is also found for smaller geographic units such as city districts (Meggiolaro, 2011), statistical neighborhoods (Fiori, Graham, & Feng, 2014) and couples' nearest neighbors (Bergsvik, 2020).

Previous research suggests three important drivers of the residential clustering of fertility: First, neighbors share living conditions that are found to affect fertility – for instance kindergarten supply (Rindfuss, Guilkey, Morgan, & Kravdal, 2010) and housing prices (Clark, 2012). Such shared conditions may give rise to *contextual effects*. Next, individuals may *self-select* into neighborhoods that fit well with their lifestyle and preferences, including their intended family size (Kulu & Washbrook, 2014). Couples who intend to have (many) children may prefer neighborhoods perceived as 'family friendly' – e.g. with good schools, available green areas, and spacious single family houses (Mulder, 2013). Last, neighbors may influence each other's fertility by exchanging information, norms and ideals. Such *social interaction effects* have been found among friends (Balbo & Barban, 2014), siblings (Lyngstad & Prskawetz, 2010; but for a counterexample see Cools & Hart, 2017), colleagues (Pink, Leopold, & Engelhardt, 2014) and network members in general (Lois & Becker, 2014). Because neighbors and neighborhoods are an important part of families' networks (Kalmijn, 2012), family size increases may influence neighbors' fertility.

Beyond this, there is a large literature demonstrating a strong link between childbirth and residential relocation. In line with common sense, the literature also indicates that increases in family size results in different housing needs. However, because residential moves often are made in anticipation of a birth (Ermisch & Steele, 2016; Öst, 2012), social interaction effects among neighbors are notoriously hard to distinguish from selective residential moves. Properly identifying both mechanisms requires a research design that nets out confounding factors (Manski, 1995).

This study adds to the literature of fertility effects on residential moves and social interaction effects among neighbors by exploring an alternative way of handling selection. We use random variation in fertility (i.e. having a third child) caused by two much-used 'instrumental variables' (Angrist & Pischke, 2009): twin births and children's sex composition. A twin birth involves an unintended family increase, demonstrably random conditional on the mother's age (Black, Devereux, & Salvanes, 2005; Rosenzweig & Wolpin, 1980).[1] Children's sex composition is also random, but having two first born children of the same sex increases parents' probability of having a third child (e.g. Andersson, Hank, Rønsen, & Vikat, 2006; Angrist & Evans, 1998; Mills and Begall, 2010). Using these instruments, we test how family expansions impact both residential choices and neighbor's fertility.

We also contribute to the network and neighborhood effects literature by operating with a careful view on networks and neighborhoods. Using detailed geo-referenced data from Norwegian administrative registers covering familial and residential histories of all residents of Norway for the years 2000 to 2018 (N ~ 167,000), we move beyond an understanding of families and their relocations as isolated actors and choices, instead recognizing them as being inherently linked to wider social contexts (Coulter, Ham, & Findlay, 2016). Moreover, rather than understanding neighborhoods solely as fixed categories (such as 'urban/rural'), our neighborhoods are constituted by their inhabitants. Further, we assess all steps in this dynamic neighborhood process, where families may choose to move away from, stay in, influence and/or relocate to a different neighborhood network.

# 2 Mechanisms of spatial clustering

## 2.1 The effect of family size on residential decisions

The actual or anticipated number of children may influence where couples want to live for several reasons. Most importantly, a larger family – all else equal – requires more space (Mulder, 2013). Furthermore, couples with more children may benefit more from living in a neighborhood with a family-friendly infrastructure than couples with fewer children: The value (in terms of saved time) of access to schools, recreational spaces and activities will increase with the number of children. In line with this, Kulu and Boyle (2009) find evidence of selective moves from city centers to surrounding suburbs.

As increases in family size may change housing needs, it is not surprising that propensities to undertake residential moves peak around childbirth (Ermisch & Steele, 2016; Mulder, 2013). Ermisch and Steele (2016) have also demonstrated how fertility *intentions* work to predict moves in Britain, indicating that couples move in anticipation of family expansions. In support of this, several studies find indications that moves precede (first) births (see Öst, 2012 for Sweden; Feijten & Mulder, 2002 for the Netherlands; Kulu & Steele, 2013 for Finland). Also, in Norway fertility intentions and migration intentions are positively related (Dommermuth & Klüsener, 2019). Besides this, however,

[1] However, the use of various fertility treatments, such as IVF, has given rise to a correlation between twin births and other features of the mothers than just age. We return to this discussion in relation to the balancing tests in Section 5.

transitions into parenthood and growing family size are found to be associated with a lower propensity to make (long-distance) moves (Clark & Withers, 2007; Dommermuth & Klüsener, 2019; Kulu & Milewski, 2007). Ermisch and Steele (2016) discuss a 'taste for stability', where individuals with more children are less likely to relocate due to high costs of moving with a large family and families' place attachment, e.g. the importance of local networks for parents and children, as well as potential established ties to local schools and kindergartens (Clark, Duque-Calvache, & Palomares-Linares, 2017). Hence, it is not obvious how an unplanned family addition (at higher parities) affects moving behavior.

As decisions (and plans) of housing and childbearing often are made together, they can be jointly influenced by values and ideals ('tastes'). Kulu and Steele (2013) model residential moves and childbearing jointly, and find that the two processes are positively correlated, i.e. that individuals prone to relocate also are more likely to have children. This simultaneity complicates assessing whether childbearing has a causal effect on residential moves. Because (long-term) fertility intentions can influence residential decisions (Ermisch & Steele, 2016), a correct temporal ordering of events is not sufficient to ensure causality.

## 2.2 Social interaction effects between neighbors

For families with children, neighbors are quite present in everyday life, whether it is at the local kindergarten, school or playground. Couples' networks have been shown to shift to more local ties after becoming parents, and respondents of a Swiss panel study state that they feel closer to more neighbors and report more neighborly contact and support after having a child than before the childbirth (Kalmijn, 2012; Rözer, Poortman, & Mollenhorst, 2017). Parents have many opportunities to interact with neighbors in a similar family situation and such interaction might be particularly relevant. Neighbors may exchange knowledge and perceptions of norms, and through everyday interactions reveal the joys and stresses of life in different family sizes. Through such social learning neighbors have the potential to shape what is seen as a normal or desirable number of children, and in turn influence each other's fertility behavior (Bernardi & Klarner, 2014).

Social influence on the transition to parenthood has been documented for other peer groups and might be present for higher parity transitions as well. Individuals whose friends, acquaintances and siblings have young children are more likely to become parents, net of initial childbearing intentions (Lois & Becker, 2014). An individual's probability of becoming a parent has also been found to increase after siblings' (Lyngstad & Prskawetz, 2010) and high school friends' childbearing (Balbo & Barban, 2014), and the year after a colleague gives birth to a child (Pink et al., 2014). Pink et al. (2014)

emphasize perceived similarity as an important amplifier for social learning effects, arguing that this should imply a parity specific social influence.

Social influence among neighbors is examined for a range of individual outcomes such as mothers' labor marked participation (Maurin & Moschion, 2009) and problem behavior among adolescents (Sampson, Morenoff, & Gannon-Rowley, 2002), but fertility contagion among neighbors has been studied mostly in high-fertility contexts, as for example rural Nepal (Axinn & Yabiku, 2001; Jennings & Barber, 2013) and Kairo (Weeks, Getis, Hill, Gadalla, & Rashed, 2004) where individual fertility behavior was found to vary with neighbors' family size preferences and local community context. Still, there is evidence that contextual factors such as settlement size or opportunity structures for families in a municipality also matter for fertility behavior in countries that already have gone through major demographic transitions (e .g. Kravdal, 2002; Kulu, Vikat, & Andersson, 2007; Rindfuss et al., 2010). However, no study has yet tested causal interaction effects of neighbors' family behavior in a context such as Northern Europe where fertility is usually seen as a highly individualized couple-based choice (Lesthaeghe, 2010).

# 3 Self-selection and confounding factors: The scope for using Instrumental Variables

To empirically identify the separate mechanisms of moving behavior and social interaction effects, we need to distinguish them both from each other and from confounding factors and other forms of self-selection. Using a source of exogenous (random) variation in fertility could potentially solve these problems and allows for testing of the following three hypotheses:

A) Having a third child causes mothers to relocate

B) Having a third child causes mothers to live in family-friendly neighborhoods

C) Having a third child causes one's neighbors to have more children themselves

Regarding social interaction effects (Hypothesis C), there are two main factors that complicate the task of causal identification: In addition to being influenced by each other, neighbors may display similar behavior because they are similar at the outset (which, in turn is the result of selective residential sorting) and because they are influenced by the same environment (contextual effects). An exogenous source of variation in fertility would be independent of both the self-selection of neighbors and their shared environment. Hence, evidence of social interaction effects exists if an exogenous increase in the family size of one neighbor tends to be followed by a change in another neighbor's fertility.

Also, regarding the estimation of the effects of larger family sizes on residential decisions (Hypotheses A and B), self-selection may be a confounder, albeit in a slightly different way. Consider two couples, one residing in a large suburban house with four children, another in a compact central urban apartment with one child. Surely, the number of children need not be the only difference between the couples relevant to their residential decisions: Differences in tastes and lifestyle preferences, in combination with economic resources, are likely to influence *both* residential decisions and fertility decisions. Again, we will isolate the effect of family size on residential decisions only by using a source of exogenous variation in fertility.[2]

One approach to handle the simultaneity of housing and fertility decisions has been to jointly model both processes within a multilevel multiprocess statistical framework. Kulu and Steele (2013), for instance, find that results change little when housing and fertility decisions are simultaneously estimated, with their residual effects allowed to correlate. However, this modeling strategy will not handle omitted variables that vary over time. Estimates are therefore prone to suffer from omitted variable bias (Wooldridge, 2010). To further improve the understanding of the drivers of the residential clustering of fertility this paper tests another approach using instrumental variables.

We apply two much-used instrumental variables (IVs) in order to obtain exogenous variation in having a third child: the event of a twin birth at second parity (Rosenzweig & Wolpin, 1980) and the sex composition of the two firstborn children (Angrist & Evans, 1998). Twin births represent an unplanned immediate increase in family size and a permanent increase in family size for couples who would otherwise not have had more children. To the extent that having twins is conditionally random (i.e. if parents of twins are no different from parents of singletons after observable characteristics are netted out), it is potentially valid as an IV for family size. The sex-composition instrument relies on the fact that many couples have a preference for having one child of each sex, so that they will have a third child if and only if the two first born are of the same sex (Andersson et al., 2006). As child sex is random, so are increases in family size induced by child sex composition. Tests for (conditional) randomness on observable characteristics are presented in Section 5.

The two IVs employed in this paper represent quite different fertility experiences. The twin instrument captures the effect of a third child among couples who would otherwise prefer only two children, whereas the sex-composition instrument captures the effect of a third birth among parents who would

[2] To ensure that the effect of fertility on moves is not influenced by wider residential socialization, the correct temporal order is sufficient: Given that the second conception takes place before the relocation, it seems highly unlikely that future neighbors influence fertility plans. When it comes to the effect of fertility on relocation, correlated effects are not a source of bias – rather, they play a role in the mechanism – as a 'selective' move by definition is influenced by characteristics of the destination neighborhood.

stop at two children if – and only if – they were of mixed sex (see also Hart & Cools, 2019). For many reasons, having another child because of the desire for children of both sexes could be less straining than having twins. Most importantly, there is no spacing between twins which might make the family increase more stressful. These particularities of the IVs open an important discussion about their validity. To be valid our instruments must affect our outcomes through the instrumented variable (family size) only. This assumption cannot be tested directly but must rather be approached through reasoning and indirect tests (see e.g. Huber, 2015). Regarding the twin instrument it is required that the short spacing itself has no direct effects on neighbor's fertility and the family's residential decisions. When it comes to sex composition, earlier research has argued that children of the same sex generate lower expenses because they for example could share clothes and a room, which could lead to differing effects on moving behavior as compared to that of otherwise similar large families. However, Huber (2015) notes that differences in the economies of scales by children's sex composition have not been confirmed for high-income countries. While our main aim is to test the above-mentioned hypotheses (A, B and C), we also explore the different nature of these 'random' family increases to see how they matter for interaction effects and residential decisions.

# 4 Data and study sample

This study is based on combined individual-level records from several Norwegian administrative registers covering residential and childbearing histories for the whole population of Norway in the years 2000 to 2018. Women are linked to their neighbors and children using yearly updated geocoded addresses and personal identification numbers (PINs) respectively.[3]

## 4.1 Study sample and timeline

Our study sample consists of women who gave birth to a second child between 2002 and 2012, represented by measurement point 't' in the timeline in Figure 1. Inclusion in the sample was conditional on being between ages 25 and 35 at second birth and being registered with a Norwegian address two years before the birth, i.e. in t-2 (~167,000 women). To analyze how an increase in one of these women's fertility affects her neighbors' fertility we split the sample into two parts: A 33% random subsample of these women (~ 55,000) constitute the 'index women' (IW) whose fertility will potentially influence the fertility of their neighbors. The remaining 67% enter the pool of neighbors

[3] In the analyses we only employ data on women, although also men and the whole family is important. We do not include partners' characteristics and do not control for partnership status and potential changes in such statuses. As there is scarce evidence that partner characteristics or dissolution propensities of partnerships differ between couples who have twins or children of the same sex as opposed to other couples with young children in the household (Jena, Goldman & Joyce, 2011), this is unlikely to influence our results in a substantial manner.

whose fertility is potentially influenced by that of the index women. In this way no pair of women can simultaneously influence each other – every woman is *either* a potential influencer *or* potentially influenced, thereby avoiding reflection bias (Manski, 1993).

Index women's individual neighborhoods are captured two years before the childbirth (2000-2010, t-2 in Figure 1) and defined by way of geographical coordinates on place of residence at the end of that year. Neighborhoods consist of each woman's 50 nearest female neighbors ages 20-44, defined by straight-line distances. Within the neighborhood, women of ages 20 to 36 are defined as 'potentially influenced'. This gives on average 29 neighboring women, within which 15 are mothers, and of these, six are two-child mothers. The average distance between the index woman and their neighbor is approximately 400 meters (median approximately 136 meters) (see Appendix Table 1).

**Figure 1: Timeline for measurement points in study**

| 2000-2010 | | 2002-2012 | | | | | | 2008-2018 |
|---|---|---|---|---|---|---|---|---|
| t-2 | t-1 | t | t+1 | t+2 | t+3 | t+4 | t+5 | t+6 |
| **Start**<br><br>**Exogenous measures**:<br>Initial neighbors;<br>IW's background characteristics;<br>'Historical' fertility of final neighborhood | | **IW's 2nd birth: Fertility shock?**<br>Treatment assigned for sex mix IV, treatment received for twin IV<br><br>Used for sample construction:<br>IW 25-35 years old | | | **3rd birth** realized among average sex mix 'complier' | | | **End**<br>**2nd child school age**<br><br>**Outcomes:**<br>A) Did IW relocate?<br>B) IW's final neighborhood<br>C) Fertility of IW's initial neighbors |

## 4.2 Outcome variables:

In several separate models, we analyze how a family increase (i.e. having a third child) impacts three groups of outcomes:

A) mothers' propensity to move,

B) characteristics of their final neighborhood, and

C) the future fertility of neighbors from the initial neighborhood.

As can be seen in Figure 1, we measure these outcomes six years after the index woman's second childbirth (at measurement point t+6), i.e. when twins are six years old and the third child of the mean (median) 'complier' is 2.2 (2.7) years old.

***Outcome Group A: Propensity to move***

To measure the propensity to move, we construct an indicator variable taking the value 1 if the mother has moved at least once between the year before the second birth (t-1) and the year the second child reaches age six (t+6), otherwise zero. For the same time span, we measure the number of relocations and if the mother has had a move of at least three kilometers.[4] Since we also are interested in the timing of the decision to move, we estimate the cumulative moving propensity for each year between t-1 and t+6 as well.

***Outcome Group B: Characteristics of the final neighborhood***

Outcome Group B captures aspects of the neighborhood where the index woman lives when her second child is six years old (measurement point t+6 in Figure 1) – independently of whether she has moved or not. We proxy the 'family-friendliness' of the final neighborhood by the average number of children per woman aged 25-44 in that neighborhood – as measured eight years earlier.[5] We measure these characteristics eight years earlier (in t-2, two years before second birth) in order to construct a measure that is free from potential interaction effects running from the index woman to her neighbors. To explore effects at different margins, we also construct variables that capture the proportion of women in the neighborhood eight years earlier that had at least one, at least two, and at least three children.

***Outcome Group C: Fertility of neighbors in the initial neighborhood***

For estimating social interaction effects among neighbors without confounders due to relocations, we need to hold the initial neighborhood constant at measurement point t-2 (see timeline in Figure 1). Our outcome is therefore the average number of children among female neighbors aged 20-36 from the initial neighborhood (defined as the index woman's neighborhood 2 years *prior* to her 2nd birth), measured six years *after* the index woman's second birth (i.e., in t+6). The outcome is aggregated for different groups of neighbors, who were all residing next to the index woman in year t-2. Besides measuring the aggregated number of children, we distinguish between those who, in t-2, were either i) childless women, ii) mothers with one child, and iii) two-child mothers.

[4] In our sample the median distance moved is 3.8 kilometers and declines with the age of the second born child. This fits well with findings from other contexts, i.e. the study of Ermisch and Steele (2016), where the median distance moved was three kilometers.

[5] The neighborhood is defined using basic statistical units, which on average include 131 women.

**Table 1. Descriptive statistics for main outcomes**

| **Outcome Group A: Propensity to move**[a] | Mean | SD | N |
|---|---|---|---|
| Has moved within 2nd child is six years old | 0.661 | 0.473 | 166,710 |
| Has moved at least 3 km | 0.423 | 0.494 | 166,927 |
| Number of moves (any distance) | 1.048 | 1.086 | 166,927 |
| **Outcome Group B: Characteristics of the final neighborhood**[b] | | | |
| Average number of children per female neighbor 25-44 | 1.394 | 0.303 | 166,666 |
| Percentage with at least one child | 64.93 | 11.05 | 166,657 |
| Percentage with at least two children | 48.18 | 11.82 | 166,657 |
| Percentage with at least three children | 18.06 | 8.41 | 166,657 |
| **Outcome Group C: Fertility of initial neighbors**[c] | | | |
| Young female neighbors' number of children in t+6 | 1.613 | 0.351 | 54,787 |
| Childless neighbors' number of children in t+6 | 0.847 | 0.301 | 54,755 |
| One-child neighbors' number of children in t+6 | 1.809 | 0.385 | 54,475 |
| Two-child neighbors' number of children in t+6 | 2.344 | 0.312 | 53,461 |

[a] Cumulative sum measured from the year before IW's second birth (t-1) until six years after (t+6).
[b] Neighborhood where family lives when 2nd child is six years old (t+6). Neighbors' characteristics measured at start (t-2). Here, neighborhoods refer to basic statistical units with on average 131 women of age 25-44.
[c] Young female neighbors who were 20-36 years old and living next to the index woman in t-2.

## 4.3 Background characteristics

To increase precision and to meet the assumption of (conditional) random assignment (for the twin instrument), several observable characteristics of index women and wider geographical attributes of neighborhoods are included as covariates in all regression models. Further, calendar year dummies are included in all models.

*Individual characteristics* include age at second childbirth in years, the time between the first and second birth in years (min. 0.75 years = 9 months) and an indicator for being foreign born (ref. Norwegian born). Further, a mother's employment status was defined as active (ref.) if her annual income from wages and salaries exceeded the social security base income (~ 50,000 NOK in 2000). Additionally, her income (inflation-adjusted to 2000-NOK) is included using her position in the sample's income quartile (Q1: 135,000 NOK; Q2: 215,000 NOK (ref.); Q3: 275,000 NOK). A set of dummies for educational attainment distinguishes between the following categories: (i) Primary education (≤ 10 years); (ii) Secondary education (11–13 years) (ref.); (iii) Short university education (14–17 years); and (iv) Long university education (≥ 18 years). We also include a covariate for the number of years a mother has lived in her current dwelling, including a squared term to capture possible nonlinearities. All characteristics are measured two years before the birth of the second child (in t-2).

*Place of residence* is captured by a set of dummies for the seven main regions in Norway which were: The Capital region (previously Oslo and Akershus, ref.), South Eastern Norway, Hedmark and Oppland (now: Innlandet), Agder and Rogaland, Western Norway, Trøndelag, and Northern Norway. Further, a measure of municipal centrality is included. Centrality describes a municipality's geographical position in relation to urban settlements and these settlements' population size (see Statistics Norway Standard Classification of Centrality at http://stabas.ssb.no/, 2014 classifications). This study used the following five categories: (i) Municipality with a regional center; (ii) Municipality within 35 minutes commuting time to a regional center (ref.); (iii) Municipality within 36 to 75 minutes commuting time to a regional center; (iv) Somewhat central municipalities; and (v) Less and least central municipalities.

## 4.4 Descriptive statistics and balancing tests

In order to see whether the instrumental variables we use are randomly assigned, we test differences in background variables among mothers according to whether they had either twins at second birth or two same-sex children, or not. The results of these tests are shown in Tables 2 and 3. For the instrument to be randomly assigned, there should not be systematic differences by instrument status on outcomes measured before the instrument is assigned.

For the sex-composition instrument (column 4-6), there are no significant differences by instrument status. Mothers with two first children of the same sex are statistically similar to mothers whose two first children are of opposite sex, both with respect to age, years since first birth and being born in Norway. For the twin instrument (column 1-3) we find multiple statistically significant differences by instrument status, some of them of sizeable magnitude. This finding is in line with previous applications, which show this instrument to be only conditionally random (Hart & Cools 2019).

**Table 2. Background characteristics by instrument status (full sample)**

| | | | | Singleton birth | | |
|---|---|---|---|---|---|---|
| | *Twin* | *Singleton* | **t-test** | *Same sex* | *Diff. sex* | **t-test** |
| **Covariates** | **Mean** | **Mean** | **Diff.** | **Mean** | **Mean** | **Diff.** |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Birth year 2nd child | 2004.91 | 2005.02 | .108 | 2005.01 | 2005.03 | .021 |
| Age at 2nd birth | 30.929 | 30.331 | -.597*** | 30.338 | 30.324 | -.014 |
| Years since 1st birth | 4.049 | 3.686 | -.363*** | 3.681 | 3.692 | .009 |
| Norwegian born | .894 | .861 | -.033*** | .860 | .861 | .001 |
| N | 2,771 | 164,156 | 166,927 | 81,948 | 82,208 | 164,156 |

Note: * p<0.05; ** p<0.01; *** p<0.001

**Table 3. Balancing tests: Unconditional and conditional dependence on IVs**

| **reg X IV** | **IV= twin** | | **IV= samesex** | |
|---|---|---|---|---|
| **X=** | (1) | (2) | (3) | (4) |
| Active in labor force | .013* | -.003 | -.000 | -.000 |
| Income in 1000s | 10.121*** | 2.461 | 1.012 | .893 |
| Has higher education | .020* | .004 | .002 | .001 |
| Time since last move | .242*** | .131* | .005 | .005 |
| Table 2 covariates included | No | Yes | No | Yes |
| N | 166,927 | 166,927 | 164,156 | 164,156 |

Note: Covariates include: birth year 2nd child, age at 2nd birth, years since 1st birth, and Norwegian born
* $p<0.05$; ** $p<0.01$; *** $p<0.001$

To test whether twin births are *conditionally* random in this sample, in Table 3 we estimated how the IVs predict several other background characteristics, first without conditioning on the background variables in Table 2 (columns 1 and 3), then conditioning on them (columns 2 and 4). Under conditional independence, significant associations should disappear when controlling for background characteristics in Table 2 (see also Hart & Cools, 2019). For the sex-composition instrument, there are no significant associations. The twin instrument is however significantly associated with the outcomes in Table 3, but the association disappears for all characteristics except for time since last move when covariates are included in column 2. We do not have a clear explanation for why there remains a significant difference between the groups in terms of this one variable. We control for this difference in our analyses.

# 5 Results

The IV estimation is done in two steps, using 2SLS regression. First, we give the first-stage estimates, which estimate the effect of having twins or two firstborn children of the same sex on family size—captured by the probability of having a third child within the time it takes for the second child to reach age six. Then IV estimates are obtained by regressing the various outcomes on the part of the variation in the index mother's family size tied to twinning or sex composition. The IV estimates capture the average treatment effects among those moved by the instruments ('compliers')—that is, those mothers who will have a third child if and only if their second childbirth is a twin birth or the two first children are of the same sex (Angrist & Evans, 1998). Reduced-form estimates of the effect of children's sex composition or having twins on the outcomes are also presented. Reduced-form estimates give the impact of a twin birth or children's sex composition on the outcome in question, without assuming that the effect is channeled through family size. Last, we describe the correlation between the outcomes and family size using OLS regression. All specifications include dummies for age at second birth and calendar year. In all tables, the even-numbered columns also include a set of exogenous control variables (see section Background characteristics).

## 5.1 Outcome Group A: How family size affects residential moves

The main results for Outcome Group A are presented in Table 4. The upper and lower panels give estimation results for the twin (Panel A) and the sex-composition (Panel B) instruments, respectively.

Having twins raises our sample mothers' probability of having three children by 67 percentage points, on average, meaning that 33 percent of mothers would have had a third child within six years regardless of the twin birth (first-stage estimates, columns 1 and 2). Having two firstborn children of the same sex increases the likelihood of having a third child within six years after the second birth by about four percentage points. Although the first-stage estimates differ in strength, statistics for both satisfy the criteria for instrument relevance. In columns 3 and 4 we see that having a twin birth increases the probability of moving for mothers in our sample by 0.015, on average ($p$ <.1). On the other hand, having two firstborn children of the same sex is negatively, but not statistically significant, associated with the moving probability for mothers in our sample.

Instrumented with a twin birth, having a third child increases mothers' probability to move within six years after the second birth by 0.022 ($p$ <.1), on average (column 6). Conversely, the estimates derived from the sex-composition instrument are negative and not statistically significant. OLS estimates show, consistent with previous research, that having a third child is positively correlated with a mother's propensity to move (columns 7 and 8).[6] Compared with the 2SLS estimates in column 6, the OLS estimates are substantially more positive.

***Distance, number and timing of moves***

Our main results showed how having a third child affects mothers' propensity to move at least once between the year before second birth until six years after the birth, regardless of the type of relocation. The positive effect found with the twin IV seems to be driven by a higher propensity to relocate once and in the immediate neighborhood (Appendix Table 2). No effects emerge for relocations with a distance of at least three kilometers and on the number of relocations in the period between the year before until six years after the second birth.[7]

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[6] The estimates differ marginally between Panel A and B because twin mothers (N=2,771) are taken out of the sample of Panel B.

[7] Estimates are also close to zero for moves of at least five and ten kilometers. The larger the moving distance, the more similar the moving behavior of mothers who recently had a second child - with or without a fertility shock (results upon request).

The impression of immediate adjustment moves due to space needs being one driver among those with a (twin) fertility shock is strengthened by results for the probability of having moved at least once up until different points in time (between year t-1 and t+6). Effects emerge the two years following the birth and then again six years after birth (see Appendix Figure 1).

**Table 4. Effects of a twin birth, children's sex composition and having a third child on the propensity to move**

| | First Stage | | Reduced form | | IV Estimate | | OLS Estimate | |
|---|---|---|---|---|---|---|---|---|
| **OUTCOME:** | **> 2 children (t+6)** | | **Move (t-1 to t+6)** | | **Move (t-1 to t+6)** | | **Move (t-1 to t+6)** | |
| | OLS | OLS | OLS | OLS | 2SLS | 2SLS | OLS | OLS |
| **Panel A** | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Twin birth** | .668*** | .671*** | .004 | .015* | | | | |
| | (.009) | (.009) | (.009) | (.009) | | | | |
| > 2 children at t+6 | | | | | .007 | .022* | .071*** | .059*** |
| | | | | | (.013) | (.013) | (.002) | (.002) |
| Cons | .375*** | .383*** | .630*** | .661*** | .628*** | .665*** | .603*** | .650*** |
| | (.005) | (.009) | (.005) | (.009) | (.007) | (.010) | (.005) | (.009) |
| Adjusted $R^2$ | .054 | .088 | .013 | .079 | .014 | .081 | .019 | .082 |
| N | 166,710 | 166,012 | 166,710 | 166,012 | 166,710 | 166,012 | 166,710 | 166,012 |
| **Panel B** | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Same Sex** | .043*** | .043*** | -.003 | -.004 | | | | |
| | (.002) | (.002) | (.002) | (.002) | | | | |
| > 2 children at t+6 | | | | | -.081 | -.084 | .074*** | .060*** |
| | | | | | (.055) | (.053) | (.002) | (.002) |
| Cons | .355*** | .362*** | .631*** | .674*** | .660*** | .704*** | .602*** | .649*** |
| | (.005) | (.009) | (.005) | (.009) | (.021) | (.022) | (.005) | (.009) |
| Adjusted $R^2$ | .027 | .063 | .013 | .078 | -.005 | .062 | .019 | .082 |
| N | 163,944 | 163,258 | 163,944 | 163,258 | 163,944 | 163,258 | 163,944 | 163,258 |
| Other covariates | No | Yes | No | Yes | No | Yes | No | Yes |

Note: All specifications include dummies for mother's age and calendar year at second birth. Other covariates: years since 1st birth, Norwegian born, time since last move, employment, income, education, country region, and centrality. Standard errors in parentheses. Women with twin births excluded in panel B.
* p<.1; ** p<.05; *** p<.01

### *Heterogeneous effects by size and type of dwelling and centrality*

To the extent that the effect of the (twin) fertility shock on moving is driven by the need for more housing room, we expect the effects to be stronger among those that started out with relatively smaller dwellings.[8] We show results by subsamples defined by index women's number of rooms, type of

[8] Note, that among those in the smallest dwellings effects of a third birth could be less pronounced if the dwelling already is not suitable for the second child.

dwelling and centrality in Appendix Figure 2. Housing data come from the official registry of ground properties and addresses and are linked to individuals through detailed address codes.[9] For housing type we differentiate between apartments and (terraced) houses. For the number of rooms in the current dwelling we separate between those with (i) up to four and (ii) at least five rooms (excluding kitchen and bathrooms).

The twin IV estimates confirm that immediate effects are concentrated among mothers in relatively smaller dwellings (with up to four rooms) and mothers in apartments. Mothers in apartments are more likely to move all measured distances after a (twin) fertility shock and the effect lasts for moves further than three kilometers (results upon request).[10] For mothers who start out in houses, immediate effects are smaller, but they persist.

To sum up, our analyses shows that a family increase due to the desire for having one child of each sex does not significantly affect the moving behavior of mothers after conceiving a second child with the same sex as the first. On the other hand, a family increase due to a twin birth increases a mother's propensity to relocate, with single immediate space adjustment moves of short distances being one driver. In itself, these patterns do not reveal the kind of neighborhood families end up in. This will be in focus in the next section.

## 5.2 Outcome Group B: How family size affects choice of neighborhood

In this part of the analysis we focus on how family size affects aspects of the neighborhoods where mothers live six years on from the second birth. Importantly, we do not consider whether mothers move or not, meaning that family size can affect neighborhood characteristics both by inducing and preventing moves.

The main results for Outcome Group B are presented in Table 5, measuring the average number of children in the neighborhood where the mother resides when the second child is six years old. To obtain an unbiased measure of neighborhood characteristics we measure them with an eight-year time lag (two years before second birth) using basic statistical units (see Data section). The sample and control variables, as well as the first stage estimates (see columns 1 and 2), are identical to the ones in Section 5.1. The IV estimates also show similar effects: The sex-composition instrument gives a non-significant negative IV estimate (-0.03), while the twin-IV estimate is positive but small (0.03 $p$ <.01).

[9] Unfortunately, the housing data suffer from 20 percent missing values, where presumably especially dwellings that haven't lately been sold on the housing marked lack detailed information.

[10] A similar pattern emerges among mothers in central areas, potentially supporting the idea of both space and neighborhood adjustments (alternatively, it is - in face of a fertility shock - in central areas more difficult to find something affordable and bigger close by).

In line with previous research, our OLS estimates (columns 7 and 8) show that index women's high fertility is correlated with high historical fertility in their final neighborhood – and the estimate lies very close to that for the twin IV.

Parity specific results reveal that both the OLS and the twin IV estimates are largely driven by mothers of three children being more likely to live in neighborhoods with a large proportion of mothers with larger families (see Appendix Table 3). Hence, even though our previous analyses showed that increases in family size associated with twin births encouraged particularly high propensities to undertake short-distance consumption-related residential relocations, these same families also appear to end up in relatively family oriented, high fertility neighborhoods.

**Table 5. Effects of a twin birth, children's sex composition and having a third child on average number of children in the final neighborhood**

| OUTCOME: | First Stage **IP > 2 children (t+6)** | | Reduced form **Average no. of children** | | IV Estimate **Average no. of children** | | OLS Estimate **Average no. of children** | |
|---|---|---|---|---|---|---|---|---|
| | OLS | OLS | OLS | OLS | 2SLS | 2SLS | OLS | OLS |
| **Panel A** | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Twin birth** | .668*** | .671*** | .035*** | .021*** | | | | |
| | (.009) | (.009)) | (.006) | (.005) | | | | |
| IP > 2 children | | | | | .052*** | .031*** | .022*** | .020*** |
| at t+6 | | | | | (.009) | (.008) | (.002) | (.001) |
| Cons | .375*** | .383*** | 1.427*** | 1.303*** | 1.408*** | 1.291*** | 1.419*** | 1.296*** |
| | (.005) | (.009) | (.003) | (.006) | (.005) | (.006) | (.003) | (.006) |
| Adjusted $R^2$ | .054 | .088 | .020 | .156 | .019 | .157 | .021 | .157 |
| N | 166,666 | 165,805 | 166,666 | 165,805 | 166,666 | 165,805 | 166,666 | 165,805 |
| **Panel B** | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Same Sex** | .043*** | .043*** | -.001 | -.001 | | | | |
| | (.002) | (.00)) | (.001) | (.001) | | | | |
| IP > 2 children | | | | | -.026 | -.026 | .021*** | .019*** |
| at t+6 | | | | | (.035) | (.032) | (.002) | (.002) |
| Cons | .355*** | .362*** | 1.428*** | 1.303*** | 1.437*** | 1.313*** | 1.420*** | 1.295*** |
| | (.005) | (.009) | (.003) | (.006) | (.013) | (.013) | (.003) | (.006) |
| Adjusted $R^2$ | .027 | .063 | .020 | .156 | .016 | .152 | .021 | .157 |
| N | 163,898 | 163,053 | 163,898 | 163,053 | 163,898 | 163,053 | 163,898 | 163,053 |
| Other covariates | No | Yes | No | Yes | No | Yes | No | Yes |

Note: All specifications include dummies for mother's age and calendar year at second birth. Other covariates: years since 1st birth, Norwegian born, time since last move, employment, income, education, country region, and centrality. Standard errors in parentheses. Women with twin births excluded in Panel B.
* p<.1; ** p<.05; *** p<.01

## 5.3 Outcome Group C: Social interaction effects among neighbors

In the third set of outcomes considered, we turn away from how family size affects mothers' residential choices and instead consider how fertility transmits among neighbors. This task is complicated by several features already pointed out in the previous sections: Neighborhoods are not fixed entities, and fertility among neighbors may be correlated due to selective co-location and common environmental factors. Many studies on interaction effects among neighbors do not fully address selective moving behavior (at least not for the other neighbors and the neighborhood composition), which may lead to biased measures (Hedman, 2011; Hedman & van Ham, 2012). In a previous design, we assigned neighborhoods in one year, and then 'backtracked' instrument assignment for all neighbors with a second birth in the last five years. With such an aggregated, retrospective measure, our IVs showed social interaction effects of these neighbors' fertility on an index woman's fertility (results upon request). In such a design, however, there is a theoretical potential that some of the neighboring influencers may have made a selective (non)move because of the (intention to have a) third child, compromising the exogeneity of neighborhood family size composition. Importantly, this also raises concerns about other applications of the aggregated instrument (Maurin & Moschion, 2009). The sample and method we used for this analysis is described in detail in Section 4.

The main results for Outcome Group C are presented in Table 6. To measure the general effect of index women's fertility on her neighbors' fertility we use as an outcome the average number of children among neighbors six years after the index woman's second child is born. The control variables are identical to the ones used before. The first stage estimates turn out to be identical also in this smaller subsample.

The results indicate no significant effect of index women's fertility shock on neighbors' fertility when instrumented with twin births or the children's sex composition. Interestingly, an index woman's third birth is not even correlated with initial neighbors' future number of children. Also when dividing initial neighbors into subgroups by their number of children at start, no social interaction effect of index women's fertility shock can be found with our IV regressions (see Appendix table 4). However, dividing neighbors into subgroups by their initial number of children leads to positive correlations between an index woman's family increase and her neighbors' number of children six years later (see Appendix Table 4, columns 5 and 6). This suggests that index women who have a third child initially lived in neighborhoods with somewhat fewer children (not accounted for in the previous table) and slightly more neighboring women with childbearing plans (confirmed by increasing yearly estimates, results upon request).

**Table 6. Effects of an index women's twin birth, children's sex composition and third child on initial young female neighbors' average number of children (in t+6)**

| OUTCOME: | First Stage **IP > 2 children** | | Reduced form **Neighbors fertility** | | IV Estimate **Neighbors fertility** | | OLS Estimate **Neighbors fertility** | |
|---|---|---|---|---|---|---|---|---|
| | OLS | OLS | OLS | OLS | 2SLS | 2SLS | OLS | OLS |
| **Panel A** | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Twin birth** | .668*** | .674*** | .023** | .003 | | | | |
| **(IP)** | (.015) | (.015) | (.011) | (.010) | | | | |
| IP > 2 children | | | | | .035** | .004 | .005 | -.002 |
| at t+6 | | | | | (.017) | (.015) | (.003) | (.003) |
| Cons | .363*** | .358*** | 1.725*** | 1.646*** | 1.713*** | 1.645*** | 1.724*** | 1.647*** |
| | (.009) | (.015) | (.007) | (.011) | (.009) | (.012) | (.007) | (.011) |
| Adjusted $R^2$ | .054 | .090 | .051 | .215 | .049 | .215 | .051 | .215 |
| N | 54,787 | 54,517 | 54,787 | 54,517 | 54,787 | 54,517 | 54,787 | 54,517 |
| **Panel B** | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Same Sex** | .045*** | .046*** | .003 | .004 | | | | |
| **(IP)** | (.004) | (.004) | (.003) | (.003) | | | | |
| IP > 2 children | | | | | .067 | .084 | .004 | -.002 |
| at t+6 | | | | | (.066) | (.058) | (.003) | (.003) |
| Cons | .341*** | .334*** | 1.722*** | 1.641*** | 1.699*** | 1.614*** | 1.722*** | 1.644*** |
| | (.009) | (.016) | (.007) | (.011) | (.025) | (.023) | (.007) | (.011) |
| Adjusted $R^2$ | .026 | .064 | .051 | .215 | .044 | .202 | .051 | .215 |
| N | 53,844 | 53,581 | 53,844 | 53,581 | 53,844 | 53,581 | 53,844 | 53,581 |
| Other covariates | No | Yes | No | Yes | No | Yes | No | Yes |

Note: All specifications include dummies for mother's age and calendar year at second birth. Other covariates: years since 1st birth, Norwegian born, time since last move, employment, income, education, country region, and centrality. Standard errors in parentheses. Women with twin births excluded in Panel B.
* p<.1; ** p<.05; *** p<.01

# 6 Concluding discussion

Fertility behavior is known to be correlated within neighborhoods, yet the relative importance of the mechanisms driving this correlation remains unclear. We used random variation in fertility to test the explanatory power of two different mechanisms, namely selective moving behavior and social interaction effects among neighbors. To handle self-selection and confounding factors we used the sex composition of the two eldest children and having twins at second birth as instrumental variables (IVs) for family size increases.

When it comes to moving decisions, the OLS estimates show that third births are positively correlated with a family's propensity to move. This is in line with previous studies that have consistently shown that births and residential relocations are closely related life course transitions (Ermisch & Steele, 2016; Feijten & Mulder, 2002; Kulu & Steele, 2013; Mulder, 2013; Öst, 2012).

Having twins at second birth raises a mother's probability to relocate. Mothers of twins relocate once, and within short distances (less than three kilometers). The effect is present both in the short run (two years after the twin birth) and long run (after six years, the final year of observation). The immediate effects are concentrated among mothers who live in apartments and relatively smaller dwellings, suggesting adjustment moves due to increases in housing consumption. Interestingly, mothers of twins end up in neighborhoods which historically have a slightly higher average number of children – mainly a higher proportion of mothers with two, three or more children. This could indicate very local differences in housing characteristics causing larger families to cluster together in specific locations with suitable housing stock for larger families (see also Wessel & Lunke, 2019). For these outcomes, twin IV estimates are similar to the OLS estimates. This is solid evidence that selective moves contribute to the residential clustering of fertility. Moreover, given the potential that twin births bring forth increased demands on finances, and a greater need for more housing space, it is noteworthy to find that families with twins remain able to access family-orientated neighborhoods.

Using the sex-composition IV, no main effects for moving were identified, neither on the propensity to move nor on the characteristics of the destination neighborhood. We suggest two explanations for the diverging effects of the two instruments. First, the sex-composition IV captures effects of third births due to a preference for having at least one child of each sex. Parents may, at some level, anticipate this preference and that they are open to having a large family. This could lead them to locate to a spacious dwelling in a family-friendly area from the outset, so that they do not need to relocate when a third child is born. Second, effects of the third child might be canceled out by direct effects of the sex composition, for example in the form of more room sharing among siblings of the same sex, as discussed in Section 3.

Turning to social interaction effects between neighbors, none of the instruments show significant effects. More specifically, an index woman's fertility shock did not impact her original neighbors' number of children six years later. We note that the correlation between the index woman having a third child and her neighbors' future number of children, as estimated by OLS, was also weak. Many women move after having children, and as neighbor relations thrive on proximity and everyday encounters, it is not clear whether initial neighbors keep contact after a move, and the extent of such contact. Hence, as long as families relocate, finding a study design that both excludes self-selection to neighbor networks and ensures the networks' relevance seems especially difficult. The need to lock neighbor networks to the 'start year' (t-2) to ensure 'network exogeneity' means that we cannot exploit the full flexibility of time-varying and individual-centered neighborhoods that our data allow. As any measurement error, failure to appropriately measure networks will bias estimates of network effects towards null. An alternative setup that did not lock neighbor networks two years before the birth showed social interaction effects (see also Section 5.3). However, these neighbor networks were potentially endogenous, i.e. potentially driven by selective moves due to family size preference, a mechanism that the first part of our analysis convincingly shows is one causal component.

Taken together, our study demonstrates convincingly that selective moves are an important driver of the residential clustering of fertility. Such moves can follow an unanticipated fertility shock, such as having twins, but family size preferences can also influence housing choices before children are born. To the best of our knowledge, this has not been demonstrated previously with a design that handles selection bias as convincingly as we do here. We find little evidence of social interaction effects between neighbors, but we note that the measures we take to ensure causal identification potentially bias our measure of the effect toward null.

Finally, it is important to note some caveats. First, some of our findings might question the general validity of the instruments. It is well known that twin births are only conditionally random, but in this application, our instrument fares somewhat worse on tests for conditional randomness than what is ideal. Next, the lack of similarity between the IV estimates indicates that at least one of the fertility shocks poorly informs us about the average third child in this setting. Regarding the twin instrument, we suspect that zero spacing between the second and third child means that the nature of the causal effect may be quite different from the average causal effect of a third child in the population. For the sex-composition instrument, we cannot rule out that direct effects of children's sex composition influence the results when the outcome is housing choices.

To conclude, the results presented in this paper identified selective moves as one plausible causal driver of the residential clustering of fertility. The effects we identify are relatively small, though statistically significant. This suggests that the residential clustering of fertility is also driven by factors that we effectively control for in our design – most importantly self-selection based on preferences for family size and a family-oriented life style. Because of the difficulty to measure social interaction effects among neighbors, we are reluctant to say that they do not exist, even though we did not identify them in our study. To conclude, we contribute to the understanding of fertility and relocation, but also to the literature on social interaction effects in fertility by testing the relevance of yet another network, namely that of neighbors.

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# 8 Appendix

**Appendix Table 1. Descriptive statistics for all subsamples of neighbors (women of age 20-36 at start).**

| | All | Childless | One-child | Two-child |
|---|---|---|---|---|
| Average number of neighbors | 29.4 | 14.5 | 5.9 | 6.4 |
| Mean average distance (meters) | 399 | 399 | 398 | 407 |
| Median distance (meters) | 136 | 136 | 133 | 138 |
| N (index women) | 54,787 | 54,755 | 54,475 | 53,461 |

**Appendix Table 2. Effects of a having a third child on the propensiy to relocate with a distance of at least 3 kilometers and on the number of relocations, irrespective of distance (Twin IV and OLS estimates)**

| | Twin IV | | OLS estimate | |
|---|---|---|---|---|
| | 2SLS | 2SLS | OLS | OLS |
| **Move > 3km** | (1) | (2) | (3) | (4) |
| > 2 children (t+6) | -0.010 | 0.009 | 0.078*** | 0.065*** |
| | (0.014) | (0.014) | (0.003) | (0.003) |
| Cons | 0.407*** | 0.451*** | 0.373*** | 0.429*** |
| | (0.008) | (0.011) | (0.005) | (0.009) |
| Adjusted $R^2$ | 0.008 | 0.072 | 0.015 | 0.075 |
| **No. of moves (any distance)** | (1) | (2) | (3) | (4) |
| > 2 children (t+6) | -0.031 | -0.000 | 0.121*** | 0.103*** |
| | (0.031) | (0.029) | (0.006) | (0.006) |
| Cons | 0.98 *** | 1.16 *** | 0.92 *** | 1.12 *** |
| | (0.016) | (0.023) | (0.012) | (0.021) |
| Adjusted $R^2$ | 0.030 | 0.102 | 0.034 | 0.104 |
| N | 166,927 | 166,063 | 166,927 | 166,063 |
| Other covariates | No | Yes | No | Yes |

Note: All specifications include dummies for mother's age and calendar year at second birth. Other covariates: years since 1st birth, Norwegian born, time since last move, employment, income, education, country region, and centrality. Standard errors in parentheses. * p<.1; ** p<.05; *** p<.01

**Appendix Table 3. Effects of having a third child on family sizes in the final neighborhood (Twin IV and OLS estimates)**

| | Twin IV | | OLS estimate | |
|---|---|---|---|---|
| **Neighbors (25-44)** | 2SLS | 2SLS | OLS | OLS |
| **with ≥ 1 child** | (1) | (2) | (3) | (4) |
| IP > 2 children (t+6) | 1.295*** | .817*** | -.385*** | -.067 |
| | (.316) | (.305) | (.057) | (.056) |
| Cons | 65.772*** | 61.672*** | 66.420*** | 62.015*** |
| | (.169) | (.236) | (.116) | (.212) |
| Adjusted $R^2$ | .004 | .075 | .009 | .076 |
| **with ≥ 2 children** | (1) | (2) | (3) | (4) |
| IP > 2 children (t+6) | 1.704*** | 1.009*** | .464*** | .491*** |
| | (.338) | (.319) | (.061) | (.059) |
| Cons | 48.557*** | 43.591*** | 49.035*** | 43.792*** |
| | (.181) | (.248) | (.124) | (.221) |
| Adjusted $R^2$ | .009 | .110 | .011 | .111 |
| **with ≥ 3 children** | (1) | (2) | (3) | (4) |
| IP > 2 children (t+6) | 1.517*** | .864*** | 1.432*** | .969*** |
| | (.238) | (.210) | (.044) | (.040) |
| Cons | 18.176*** | 15.467*** | 18.208*** | 15.426*** |
| | (.127) | (.163) | (.091) | (.145) |
| Adjusted $R^2$ | 0.030 | 0.237 | 0.030 | 0.237 |
| N | 166,657 | 165,796 | 166,657 | 165,796 |
| Other covariates | No | Yes | No | Yes |

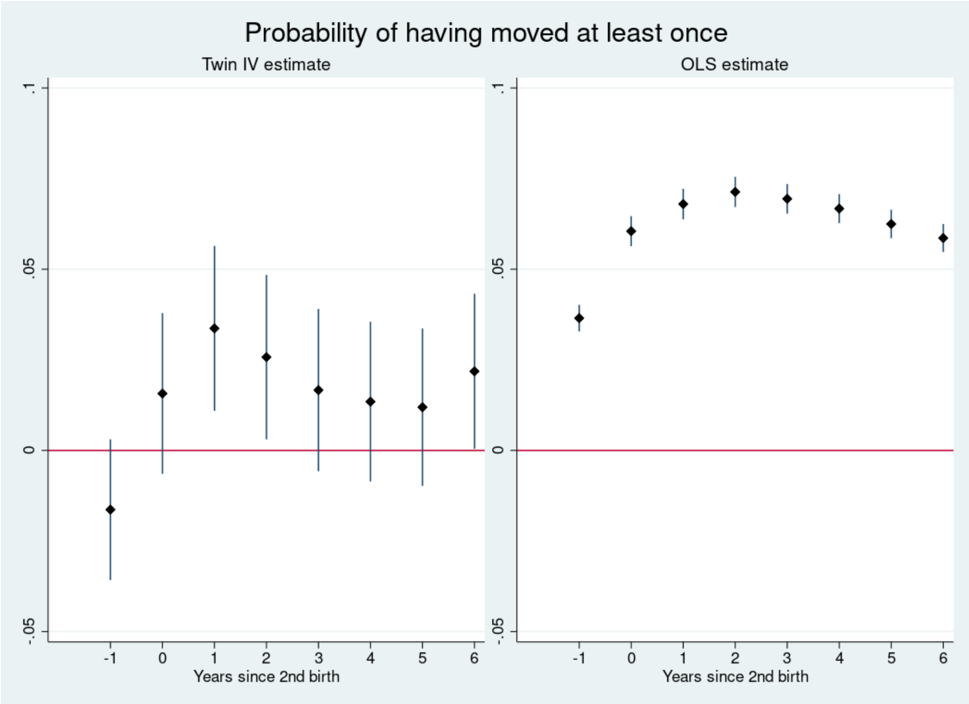
Note: All specifications include dummies for mother's age and calendar year at second birth. Other covariates: years since 1st birth, Norwegian born, time since last move, employment, income, education, country region, and centrality. Standard errors in parentheses. * p<.1; ** p<.05; *** p<.01

**Appendix Table 4. Effects of an index women having a third child on initial young female neighbors' average number of children in t+6, by neighbor subgroups (Twin IV, sex mix IV and OLS estimates)**

| | Twin IV | | Same sex IV | | OLS estimate | |
|---|---|---|---|---|---|---|
| | 2SLS | 2SLS | 2SLS | 2SLS | OLS | OLS |
| **Childless female neighbors** | (1) | (2) | (3) | (4) | (5) | (6) |
| IP > 2 children (t+6) | -.005 | -.009 | .046 | .062 | .021*** | .008*** |
| | (.015) | (.014) | (.058) | (.055) | (.003) | (.003) |
| Cons | .824*** | .780*** | .804*** | .754*** | .814*** | .774*** |
| | (.008) | (.011) | (.022) | (.022) | (.006) | (.010) |
| Adjusted $R^2$ | .003 | .041 | .003 | .035 | .005 | .041 |
| N | 54 755 | 54 485 | 53 813 | 53 550 | 54 755 | 54 485 |
| **Female neighbors with 1 child** | (1) | (2) | (3) | (4) | (5) | (6) |
| IP > 2 children (t+6) | .019 | .017 | .000 | .029 | .033*** | .011*** |
| | (.019) | (.018) | (.074) | (.070) | (.004) | (.003) |
| Cons | 1.811*** | 1.801*** | 1.819*** | 1.799*** | 1.805*** | 1.803*** |
| | (.010) | (.014) | (.028) | (.028) | (.007) | (.013) |
| Adjusted $R^2$ | .003 | .047 | .001 | .047 | .003 | .047 |
| N | 54 475 | 54 208 | 53 538 | 53 278 | 54 475 | 54 208 |
| **Female neighbors with 2 children** | (1) | (2) | (3) | (4) | (5) | (6) |
| IP > 2 children (t+6) | -.011 | -.013 | -.004 | .009 | .029*** | .013*** |
| | (.015) | (.015) | (.061) | (.058) | (.003) | (.003) |
| Cons | 2.348*** | 2.379*** | 2.345*** | 2.372*** | 2.333*** | 2.369*** |
| | (.008) | (.012) | (.023) | (.023) | (.005) | (.010) |
| Adjusted $R^2$ | -.001 | .038 | .000 | .040 | .003 | .040 |
| N | 53 461 | 53 192 | 52 533 | 52 271 | 53 461 | 53 192 |
| Other covariates | No | Yes | No | Yes | No | Yes |

Note: All specifications include dummies for index mother's age and calendar year at second birth. Other covariates: years since 1st birth, Norwegian born, time since last move, employment, income, education, country region, and centrality. Standard errors in parentheses. Women with twin births excluded in columns 3 and 4.
* p<.1; ** p<.05; *** p<.01

*Appendix Figure 1: Probability of having moved at least once before specified time points (Twin IV and OLS estimates with 95% CIs).*



*Appendix Figure 2: Heterogeneous effects by size and type of dwelling and centrality (Twin IV estimates with 95% CIs).*