CPB Netherlands Bureau for Economic Policy Analysis

CPB Discussion Paper | 244

Do rich households live farther away from their workplaces?

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15 April 2013

Abstract

One of the classic predictions of urban economic theory is that high-income and low-income households choose different residential locations and therefore, conditional on workplace location, have different commuting patterns. According to theory, the effect of household income on commuting distance may be positive or negative. Empirical tests of this effect are not standard, due to reverse causation and lack of good control variables. To address reverse causation, estimates of household income on commuting distance are derived using changes in distance through residential moves keeping workplace location constant. Our results show that the (long-run) income elasticity of distance is non-negative and around 0.14 for dual wage-earners.

Keywords: commuting; household income; wage

JEL codes: D1; JE; R2

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This research has been supported financially by the Netherlands Organisation for Scientific Research (NWO).

1. Introduction

Arguably, the centrepiece of urban economics is its elegant application of equilibrium analysis to land rent theory (Persky, 1990). Urban economic theory predicts that individuals with high incomes have different commuting patterns than those with low incomes. However, there has been a good deal of contention over the exact nature of this relationship. Empirical tests of this theory are not standard, due to reverse causation and lack of good control variables. The current paper aims to examine the long-run causal effect of income on the workers' commute.

The basic urban economic model argues, based on a monocentric city model (so, workplace is given), that those with higher incomes will have longer commuting distances (Alonso, 1964; Muth, 1969). Essential assumptions in this model are that employment is constrained in one location, monetary commuting costs depend on distance, and workers may freely choose the optimal residence location. Furthermore, it is assumed that house prices are endogenous and workers are homogeneous in all aspects except for income (Straszheim, 1987). Finally, the standard urban model is a static model which assumes away the presence of residential moving costs (one exception is Zenou, 2009), so it essentially deals with the long-run effect of income on residential location decisions.

The resulting effect of income on the commuting distance is then the result of two underlying mechanisms: (*i*) a higher income leads to an increase in demand for housing and therefore for more residential space, and (*ii*) house prices (per unit of residential space) fall with distance from the employment centre. Consequently, in equilibrium, high-income households have a longer commute (e.g. Brueckner, 2000).

Although elegant, it appears that this intuitive result does not hold when some of the more restrictive assumptions are relaxed. For example, the unambiguous positive effect of income on commuting distance does not hold when commuting costs not only depend on monetary costs but also on commuting time. In time-extended urban economic models, which assume that the workers' commuting costs include time costs that positively depend on income, the effect of income on commuting distance is ambiguous (e.g. Beckman, 1974; Hochman and Ofek, 1977; Henderson, 1977; Fujita, 1989, p. 31).[1] To be more precise, the effect of income on commuting costs depends on the difference between the income elasticity of residential space and the income elasticity of

[1] It is theoretically possible that increases in income move the household away from the employment centre, but beyond a certain point closer to the centre indicating a non-monotonous relationship between income and commuting (e.g. Fujita, 1989).

commuting costs.[2] The only empirical study of this effect that we are aware of finds that the income elasticities of residential space and (generalised) commuting costs are about equal (Wheaton, 1977). This implies that the overall effect of household income on commuting distance is close to zero and the observed spatial variation in household income should be explained by other factors than commuting, such as residential amenities (e.g. Brueckner et al., 1999).

Extending the standard model in other directions, for example allowing for more than one employment centre, generally complicates matters as the spatial distribution of wages, and therefore household income, is endogenously determined in equilibrium (e.g. Fujita and Ogawa, 1982; White, 1988; Lucas and Rossi-Hansberg, 2002). In essence, however, the general idea that workers trade-off housing prices and commuting costs, and that the level of income determines housing demand and therefore the length of the commute remains in these models.

One fundamental extension is the distinction between single wage and two-earner households. An important assumption is then whether or not wage earners belonging to the same household have the same workplace location. One possibility is to allow for two-earner households that do *not* work in the same workplace location (White, 1977). A typical example is that one wage earner of such a household commutes to the CBD, whereas the other commutes to a workplace in the suburbs (at the edge of the city). Then a change in residential location (e.g. towards the central business district) might reduce the commuting distance for one wage earner but increase it for the other. So, the effect of household income on commuting distance is then zero for two-earner households independent of the direction of the residential move.

A priori, one may think that the assumption of the same workplace is too restrictive. However, it appears that the *large* majority of wage earners belonging to the same household commute in the same direction towards their work (Van Ommeren, 2000), suggesting that the assumption that two members of the same household work at the same workplace location may be appropriate for the large majority of workers.

Allowing for two-earner households with two members who both work in the same CBD does not change the idea that households trade-off house prices and commuting costs. For example, given the extreme assumption that the demand for housing depends only on household income — and not

[2] Allowing for endogenously-chosen speed levels (through changes in travel mode) implies that higher income workers choose higher speed levels, so income-related differences in the length of the commute are likely more pronounced in terms of distance than of time.

on other characteristics which may differ between single-earner and two-earner households — and the value of travel time is proportional to wages, then an increase in wages, and therefore household income, will have similar effects on the length of the commute for single-earner households and two-earner households. However, it is usually thought that the demand for residential space strongly differs between single-earner households and two-earner households because most single-earner workers do not have a partner or children, so the single-earner households' demand for residential space is usually thought to be more inelastic with respect to income (Kohlhase, 1986).[3] This strongly suggests that the income elasticity of commuting distance is higher for two-earner households than for single-earner households. Hence, any empirical test of the standard urban economic model should distinguish between single-earner households and two-earner households.

In the standard urban economic model, the role of residential amenities is usually ignored (Brueckner et al., 1999). As is well known, the demand for residential amenities depends positively on household income. In Germany, residential amenities tend to be in historic city centres. In case that all workers commute to historic city centres, one is more likely to find a more negative effect of income on commuting distance than predicted by the standard urban economic model. We will see however that in Germany, the correlation between commuting distance and distance to the nearest city is positive but very close to zero (0.1), suggesting that the size of this (negative) effect is less relevant here. Nevertheless, this suggests that as a test of the standard urban economic model, we are more likely to have an underestimate of the overall effect.

Despite the large theoretical debate on these issues, and the extensive discussion of this topic in urban economics textbooks (e.g. O'Sullivan, 2009), there are no accurate empirical estimates of the *causal* effect of income on the commute. Previous studies do not make a distinction between two-earner and single-earner households and rely on cross-section estimates (e.g. White, 1977; Rouwendal and Rietveld, 1994), which makes a causal interpretation problematic. As far as we are aware, there are currently three studies which use panel data and estimate models with worker fixed

[3] Note that Kohlhase (1986) estimates the demand for housing, and not for residential space. This difference is important, because of differences in housing quality. To her own surprise, her data suggests that single-earner households have a higher income elasticity than two-earner households. One reason might be that she does not control for neighbourhood. It is plausible that two-earner households with children increase the demand for housing by consuming more residential space (and moving to the suburbs), whereas single workers may choose more luxurious apartments. In the current study, we use household fixed effects and observe relatively small changes in commuting distance, so we implicitly control for differences in the neighbourhood.

effects (Benito and Oswald, 1999; Van Ommeren et al., 1999; Simonsohn, 2006). Although the use of fixed effects may reduce some of the endogeneity issues, these studies still have severe limitations.[4]

First, although panel studies deal with time-invariant unobserved worker characteristics, they do not deal with reverse causation, which may play a role as labour market theories indicate that the length of the commute might affect wages. For example, as explained in detail later on, firms located at locations far from residences might compensate their workers with higher wages. Simonsohn (2006) writes "Most of the [ ] parameter estimates are hard to interpret, as [ ] higher incomes may increase opportunity costs of time (decreasing desired commute length) but may also be the result of compensatory wages for longer commutes."

Second, by including worker fixed effects and by using annual data, these studies identify *short-run* effects of household income, as most households do not move residence. When focusing on the effect of income, economic theory usually assumes away residential moving costs and therefore applies to effects in the long run. In the short run, due to the presence of residential moving costs, even when experiencing a large change in household income, few workers will immediately change commuting distance by moving residence. We are interested to estimate the long-run effect, so conditional on a residential move. We emphasise that the bulk of the current literature is not informative about the long-run causal effect of income on commuting.[5]

The current paper aims to fill this gap in the literature by examining the *long-run* effect of household income on the length of the commute. Using German panel data, our approach essentially analyses the effect of changes in household income on changes in commuting distance for workers who stay at the same workplace and who must move residence at least once during the period of observation (on average, 4.2 years). In this way, we deal with time-invariant unobserved worker characteristics, reduce reverse causation, and identify long-term effects. We emphasise that our paper must not be interpreted as a test of the monocentric model, but as a test of its prediction that high-income households have different commuting patterns than those with low incomes.

The next section shortly discusses two alternative (labour market) theories which may give rise

[4] These studies report a positive, but small, effect of income on commuting distance.

[5] In the current paper, we will deal with the above issues by estimating reduced-form models. Hence, we cannot relate our findings directly to more fundamental properties of a structural urban economic model, such as Wheaton (1977). However, our study is the first one which deals with three fundamental difficulties — unobserved heterogeneity of workers, the presence of residential moving costs and a reverse causational relationship of income and distance — which have not been properly addressed in the literature.

to reverse causation. We explicitly discuss these theories because we aim to avoid reverse causation. Section 3 introduces the identification strategy and the specification of the model, information on the data employed and presents the empirical results. Section 4 concludes.

2. Theories which explain an income-commuting relationship

As emphasised above, one of the predictions of urban economic models is a causal relationship between household income and commuting distance, because the endogenously chosen location of the residence depends on income. In essence, in addition to the explanation proposed by urban economic theory, there are *two* alternative labour market theoretical explanations for a relationship between income and commuting distance.

First, given a competitive labour market, firms located at locations far from residences compensate their workers with appropriately higher wages, which implies a *spatial wage gradient* (e.g. Fujita et al., 1997). This idea is confirmed by empirical findings (Timothy and Wheaton, 2001). These empirical findings are consistent with urban economic theory, but they do not tell us anything about the causal effect of income on the length of the commute through variation in residence location.

Second, in imperfect labour markets (e.g. a labour market characterised by incomplete information about job offers), the workers' commute is positively related to wages, because, given the residence location, job offers from employers far away are more likely to be accepted if employers offer higher wages (Manning, 2003). Also given monopsonistic labour markets, workers with a longer commute are more likely to receive compensation from the employer, because they bargain from a less advantageous position (Van Ommeren and Rietveld, 2005). Hence, there is simultaneity between the determination of wages and commute, due to search imperfections when workers change job, which also implies reverse causation.[6] However, in Germany, the data of our study, employers are unlikely to pay higher wages because workers get a substantial tax relief on their commute of

[6] Manning (2003) argues that this phenomenon is not relevant in the UK. Besides, Rouwendal and Van Ommeren (2007) find some evidence for the Netherlands that wages do not increase with longer commutes, but explicit reimbursement rules imply that workers receive some compensation. It must be emphasised that Zenou (2009, p. 23) shows that the simultaneity between the determination of wages and the commute does *not* hold in a standard urban model, because house prices compensate for the length of the commute, so wages are independent of the length of the commute.

about €0.30 per kilometre per day.[7] Hence, this tax structure in Germany implies that the workers' net-of-tax income is endogenous.

In addition to these labour market theories, we have to deal with endogeneity issues due to unobserved variables. The standard argument is that unobserved variables (e.g. skills) affect both income and the commute, causing spurious correlation between income and the commute (for commuting evidence of this phenomenon see Manning, 2003). Hence, due to the presence of wage gradients, job search imperfections, tax systems, and unobserved variables, it appears that based on a standard regression of commuting distance on household income (with controls), the effect of income on the commute is difficult to interpret as a causal effect of household income.

A common method of dealing with these endogeneity difficulties is instrumental variables (IV) estimation. One problem with this approach is finding suitable instruments for the household income variable, as emphasised by Manning (2003).[8] Given cross-section data, it turns out to be difficult to find an instrument that affects household income but not workplace location. In the current paper, we will use an alternative approach to address endogeneity of household income. In essence, we keep job location constant for a certain period and then examine to what extent changes in household income during this period induce positive or negative changes in commuting distance. As one may argue that this approach does not fully address endogeneity, we also use the income of the spouse to instrument household income. We emphasise that this instrument is only available for two-earner households.

3. Empirical Analyses

3.1. Methodology

In our analysis, we assume a standard reduced-form specification of commuting distance for a worker *i* at year *t*:

$$\log D_{it} = \beta_0 + \beta_1 \log Z_{it} + \beta_2 X_{it} + S_i + u_{it}, \tag{1}$$

[7] It should be noted that for some years in our analysis the tax relief depends on how far away the workplace is located from the individual's residence. In particular, in parts of 2007 and 2008, the commute's tax relief was partially abolished for either small or long distances.

[8] Manning (2003) re-examines the results by Benito and Oswald (1999) and finds that the IV approach used by Benito and Oswald (1999) is sensitive to the choice of the instruments. For example, the use of educational level as an instrument for household income, which is common in the demand for residential space analysis (Glaeser et al., 2008), is not appropriate, as educational level has a (strong) effect on the job arrival rate and therefore on the observed commute.

where D denotes the commuting distance, Z denotes the (net) household income. Furthermore, X_{it} denotes exogenous time-varying controls for household and work characteristics, S_i denotes time-invariant controls, and u_{it} is the overall error term. The coefficients β are unknown, and our main interest is in β_1.

Observe that (1) is formulated as a static panel data model.[9] Consistent estimation of β_1 requires that income is exogenous. In order to avoid reverse causation, we keep workplace location constant by following workers *over time until they change workplace*. Hence, in our analysis, we will only use variables which are formulated as changes over time. So, we will rewrite (1) as follows:

$$\log D_{it} - \log D_{is} = \beta_1 \left[\log Z_{it} - \log Z_{is}\right] + \beta_2 \left[X_{it} - X_{is}\right] + u_{it} - u_{is}, \qquad (2)$$

where $t > s$, and where the worker has not changed workplace between t and s.

The observed time for a worker at the same workplace location will be called the workplace spell (which is censored when a worker stopped answering the survey before changing workplace). In line with standard arguments, by using (2), one also addresses endogeneity issues due to time-invariant unobserved worker characteristics (e.g. educational level which affects income and length of the commute) as S_i is removed from the estimation procedure.

As is well known for (static) panel data models, when variables are formulated as changes over time for short periods (e.g. $t - s$ is one year), one obtains the effect of the explanatory variables on the dependent variable that is almost immediate (e.g. within one year). Hence, it is a short-run effect, whereas we are interested to estimate the long-run effect of changes in household income.[10]

By focusing on changes in household income over longer time periods, we are more likely to capture the long-run effect of changes in income in which we are interested. By construction, the maximum duration during which we can observe a household income change (while not changing workplace location) is the workplace spell. So, we will only employ changes in variables observed

[9] Formulating the model as a dynamic panel data model (so, past commuting distance is used as an explanatory variable) is not helpful here, because commuting distance changes are relatively infrequent, and one needs to observe several changes in commuting distance *per worker* to identify such a model.

[10] For example, if income increases one year and decreases the following year by the same amount, annual measures of change in income capture *transitory* changes in income. Using changes in transitory income rather than changes in income that are permanent results in downward biased long-run income estimates, in a similar way as random measurement error would downward bias the estimates.

between the beginning and end of each workplace spell. Hence, to estimate the long-run effect of income on distance, we estimate models based on the following equation:

$$\log D_{it_B} - \log D_{it_E} = \beta_1 \left[\log Z_{it_B} - \log Z_{it_E} \right] + \beta_2 \left[X_{it_B} - X_{it_E} \right] + u_{it_B} - u_{it_E}, \tag{3}$$

where subscripts t_B and t_E denote the time at the beginning and the end of a workplace spell respectively. Hence, per workplace spell we only use two observations (and calculate differences for these observations).[11]

In order to interpret the estimated coefficient β_1 as a long-run effect of household income, households must have been able to freely choose a residence location when experiencing a change a in household income (while staying at the same workplace). However, there is ample evidence that households face substantial residential moving costs (Weinberg at al., 1981; Van Ommeren and Van Leuvensteijn, 2005). As a result, households that experience a change in household income (and may prefer to change their commute) will usually not move residence, so commuting distance will not change.[12] Estimates based on (3) are therefore unlikely to generate the long-run effect of household income, because most workers will not change residence (within the period defined by a workplace spell).[13] Hence, we estimate (3) using observations of workplace spells of workers *who moved residence* during the workplace spell, i.e. workers who are free to choose the distance from their residence to the workplace.[14] See Figure 1 for examples of workplace spells that are included or excluded in the estimation approach.

[11] Using only two observations per workplace spell has also the advantage that it allows for any form of serial correlation between u_{itB} and u_{isE}. For some workers, observed workplace spells in our data are still quite short. So, we will also focus on results where we interact income with the length of the workplace spell.

[12] For example, given an increase in household income, households may be inclined to occupy a larger residence at a larger commuting distance, but due to moving costs households may decide not to move, but to wait for another increase in income and then to move residence.

[13] If one would estimate (3) using only observations for workers who do not move residence, so that the dependent variable is zero, β would be biased towards zero. Hence, one would strongly bias the long-run effect towards zero. In Germany, the annual residential moving rate is one of the lowest of Europe, and is about 7%. This suggests that by including non movers, there will be a downward bias in the order of 93%.

[14] Note that for most workers we only observe one workplace spell (workers who remained at the same workplace during the period of observation that involved a residence more) whereas for few workers, we observe several workplace spells.

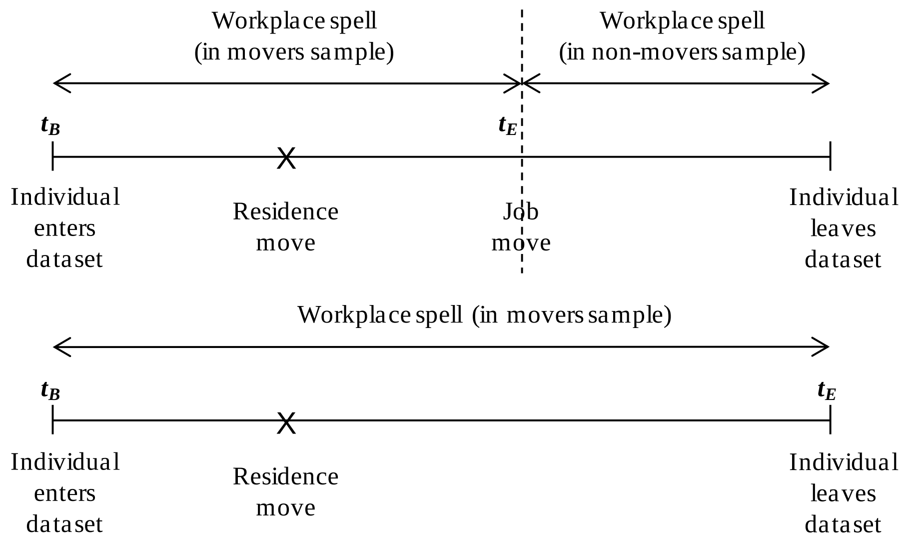

Figure 1. Example of (Censored) Workplace Spells

Note: t_B and t_E denote the beginning and the end of a workplace spell respectively.

Up to this point, for convenience, we assumed that workplace changes are observed. However, in most panel data sets the exact workplace location is unknown (for reasons of privacy). Fortunately, it is usually straightforward to determine indirectly in most surveys in which period workplace location has (not) changed. It is noted that when a worker does not change employer and does not change of type of job, it is almost always true that the worker did not change workplace location. One complication is firm relocations, because some workers may change workplace location while staying with the same employer as the firm relocated the job to another location. It is plausible that if a worker does not change employer or type of job (and does not change residence) while a change in commuting distance is observed in the data, then the worker must have changed workplace location because of a firm relocation. These changes in workplace location due to firm relocation have been shown to be quite common, see Gutiérrez-i-Puigarnau and Van Ommeren (2010). We deal with this issue by excluding workplace spells which refer to these observations.[15]

Finally, to deal with reverse causation due to the German tax system, we aim to instrument $\log Z_{it_B} - \log Z_{it_E}$ for the sample of two-earner households in equation (3). For two-earner households,

[15] This procedure also removes observations referring to measurement error in changes in commuting distance, as some workers may misreport their distance in some years (e.g. 30 km in one year and 31 km in another year). Changes in commuting distance due to an employer-induced workplace relocation may still play a role if this occurs within the same time period as a residence relocation, but this only occurs with a very small probability.

one may argue that *changes in spouse income* is a suitable instrument for changes in household income. The key assumption is that controlling for household income and therefore the wage earners' *own* income, the individual's commuting distance is exogenous with respect to spouse household income. Consequently, if we estimate (3) using only two observations per workplace spell of workers who have moved residence, and we further instrument changes in household income (with changes in spouse income) we avoid endogeneity issues due to reverse causation.

We emphasise that this instrument is arguably valid given the estimation procedure described above, but it is unlikely a valid instrument given a standard cross-section data analysis, because wages (of spouses) and commuting distances are not randomly distributed over space. For example, in large cities where commuting distances are longer and wages are higher, workers with a long commute are more likely to have a spouse with a high income. In our procedure, we control for workplace location so this is not an issue.

3.2. Data

Our study is based on information from seventeen waves of the 1990–2010 German Socio-Economic Panel (SOEP). For the years 1991, 1992, 1994 and 1996, information on commuting distance is missing, hence these years are excluded.[16] The data contains information on usual commuting distance, household and individual income (net of tax), usual monthly workhours, whether a partner is present in the household, whether the partner is employed, age, gender, and number of children. We use as indicator of children the presence of a child below 14 years. We also have information when workers change residence, when workers change of type of job (while staying with the same employer), or when workers change employer. Hourly wage rates are calculated by dividing monthly income by monthly workhours.

3.3. Selection of sample and descriptive statistics

We focus on samples of employed workers aged between 20 and 65 with a positive commute.[17] In our data selections, we follow exactly the approach explained in Section 3.1. We analyse changes

[16] Note that these years have no information on commuting distance but do have information, which we use, on whether the worker changes residence, changes of type of job or changes employer.

[17] In our analysis, we include both one-adult and two-adult households to facilitate the interpretation of how household income and commuting distances may be interrelated through single-earner and two-earner households.

over time, so we exclude observations for workers who are observed only once in the data. We also exclude observations of workers for the years that they change commuting distance but do not change residence, job type or employer. These observations refer either to firm relocations or measurement error in changes in commuting distance.[18] Further, we use information on differences that are based on the first and the last observation for the same workplace spell, see equation (3). We are then left with 26,878 workplace spells. For these spells, we impose that households must have moved residence. Hence, our main analysis will be based on a movers sample (so, households must have made at least one residential move during the workplace spell). The descriptive statistics of the movers and non-movers samples are almost identical, suggesting that sample selection is not an issue.[19] We have a sample of 6,269 workplace spells; this contains 3,747 spells of two-earner households and 2,522 spells of one-earner households. The mean workplace spell is 4.2 years.

In the movers sample, mean household income is of €3,061 (see Appendix A, Table A1). The mean one-way daily commuting distance, conditional on residence move, is 17.60 km. This is in line with a range of other studies employing German data. Hence, our sample selection is likely unrelated to commuting behaviour. Females in the sample have shorter distances and lower individual incomes than males (15km and €1,745 for females versus 20km and €2,432 for males). Workers belonging to two-earner households have slightly smaller distances than those belonging to one-earner households, but have higher household incomes. The mean distance increases by income quartile: from 15.9 km for those with the lowest incomes (first quartile), 17.9 km for those in the second quartile, 18.2 km for those in the third quartile, to 19.8 km for the highest earners (fourth quartile). This suggests a rather strong positive relationship, but a focus on changes over time shows that the strong correlation between income and distance is largely a spurious relationship. The (partial) correlation of changes in log distance and changes in log household income, controlling for year dummies, is positive and equal to 0.0334 (with a p-value of 0.0082).

We have also (subjective) information about the distance to the nearest city centre. This distance is reported by households only for the years 1999, 2004 and 2009, so it cannot be used in the analysis that will follow. Although we do not use the data in the main analysis, this data is useful for

[18] So, the observation is dropped if distance differs between t_B and t_E but residence location, job and employer do not change.

[19] We are anyway not too worried about any bias in the estimates due to this selection: a similar approach is (implicitly) taken in the hedonic pricing literature which uses data on housing transactions and therefore identifies demand for housing by households which move residence.

interpretation of the results later on. It appears that there is a positive, but small, correlation of 0.10 between log commuting distance and log distance to the nearest city centre, controlling for year dummies. This suggests that distances to workplaces are hardly correlated to distances to amenities.

3.4. Estimation results

The results of several models based on equation (3) are shown in Table 1. The estimated household income elasticity on distance is positive and around 0.0818, with a standard error of around 0.0423, see column [1].[20] This indicates that a higher household income induces households to relocate *farther away* from the workplaces of the household members in accordance with the above reported correlations. The size of the effect is small but not negligible. For example, given a daily mean commuting distance of 17.60 km, a 100% increase in income increases the commuting distance by 1.01 km ($0.7 \times 0.0818 \times 17.60$ km).[21] The positive effect of income on distance is in line with previous (non-causal) studies in the literature (Benito and Oswald, 1999; Van Ommeren et al., 1999; Simonsohn, 2006).[22]

This positive effect reported is likely due to a combination of a positive income elasticity of residential space, which tends to increase the commuting distance for high-income households, a negative income elasticity of commuting costs, which tends to decrease the commuting distance for high-income households, and a positive income elasticity of city centre amenities, which tends to decrease commuting distances for high-income households. Hence, our results imply that the income elasticity of residential space exceeds the income elasticity of commuting costs.

If individual household income shocks correlate with average income changes in the neighbourhood then our effect of household income of location choice might be the result of a general equilibrium effect. However, most likely, this correlation is extremely small, particularly because the individual income shocks are idiosyncratic and will be due to productivity changes in the

[20] The presence of a partner (controlling for single wage-earner and dual wage-earner households) has a significant positive effect indicating that when an individual starts to cohabite with a partner, the commuting distance increases. Hence, there is a tendency to move farther from the workplace for at least one of the members of the household. This is in line with standard urban economic theory, as the demand for housing increases with the presence of a partner.

[21] $0.7 \approx \log(2) - \log(1)$.

[22] We have re-estimated the model excluding observations for the region that contains the city of Frankfurt, as Frankfurt is one of the few German cities without a historic city centre and therefore likely with different, and more likely less, city centre amenities. We find that the effect of income remains the same.

workplace neighbourhood, whereas the average income changes apply to the *residence* neighbourhood.

When we exclude time-varying explanatory variables (except for year dummies), it appears that the estimates are similar to those reported above. Hence, including time-varying explanatory variables does not appear to be relevant for the results.[23] We examine further the results distinguishing between *one-earner* and *two-earner households*, see columns [2] and [3]. The effect of income on distance for two-earner households is equal to 0.1389 (s.e. 0.0434), so positive and statistically significant at the 5% level. The effect is positive but statistically insignificant for one-earner households (0.0232 with an s.e. of 0.0686).[24] So, our main result seems to be mainly driven by the effect of household income for two-earner households. However, a t-test of equality of the one-earner and two-earners income elasticities (t-test = 1.32) indicates that equality cannot be rejected. Hence, the observed differences in elasticities by type of households may be spurious.

Table 1. Estimates of Household Income on Logarithm of Commuting Distance

	[1]	[2] 1-earner	[3] 2-earners	[4] 2-earners IV
household income (log)	0.0818 (0.0423)*	0.0232 (0.0686)	0.1389 (0.0434)**	0.1725 (0.0705)**
one-earner household	0.0640 (0.0512)			
presence of partner	0.2788 (0.0749)**	0.2034 (0.1164)		
female partner	–0.2610 (0.0762)**	–0.2609 (0.1423)		
number of children	–0.0190 (0.0261)	–0.0037 (0.0566)	–0.0020 (0.0291)	0.0390 (0.0409)
female with child	–0.1159 (0.0726)	–0.0381 (0.1273)	–0.2797 (0.0876)**	–0.2906 (0.1002)**
year dummies	yes	yes	yes	yes
number of observations	6,269	2,522	3,747	2,560
adjusted R^2	0.0378	0.0239	0.0467	
F (instrument)				1633.04

Notes: Observations are measured in differences; differences are based on the first and last observation for the same workplace spell. Household income is instrumented with log spouse income in [4]. F-test=Kleibergen-Paap weak identification test.

**, * indicate that the estimates are significantly different than zero at the 0.05 and 0.1 level, respectively. Robust standard errors are in parentheses.

[23] We also estimated standard cross-section models (OLS) with the above controls as well time-invariant controls (e.g. gender). This results in an upward bias in the income elasticity estimate (0.3049 with an s.e. of 0.0103).

[24] The latter elasticity is lower than the elasticity estimated for all households (column [1]). This is in line with the literature arguing that single-wage earners benefit more than dual-wage earners from living in dense areas, where they are closer to amenities and potential spouse partners, than in rural areas, and are therefore willing to pay higher house prices (Gautier et al., 2010).

We have argued above that we estimate long-run effects. If this is true, then it should be the case that the estimated effect is rather independent of the length of the period of observation. To test for this, we have included interactions of household income with a dummy variable indicating a workplace spell of more than seven years, see Table A2 in the Appendix. The results do not reject the null hypothesis that the interaction does not have an effect, see columns [1]–[3] (these results also hold when we use other thresholds, e.g. a five years threshold).

Furthermore, we experimented with other specifications for income, but results are very similar to the results using the logarithm of income. For example, given a linear specification of income, the estimate of household income for two-earner households is 0.0028 (s.e. 0.0011). This estimate corresponds to an elasticity of 0.1063 (for a household with a mean household income of €3,796).

To see the importance of analysing a sample of residential movers, we also estimated models including observations of workers without a residential move, so the movers and non-movers sample. The sizes of the household income effects are now about 60 percent smaller than the ones discussed above. Hence, to obtain long-run effects it is important to select households that move residence.

The above estimates of household income may be argued to be an overestimate because of reverse causation, as explained above, due to the German tax system that tends to reduce tax payment for longer commutes. As explained in Section 3.1, for two-earner households, changes in spouse income is likely a suitable instrument for changes in household income. We therefore instrument household income for two-earner households in column [4].[25] The instrument is strong, with an F-value far above 10.

The estimate of household income with instrumentation is 0.1725, so very similar to that of column [3].[26] As the standard errors of the IV approach are larger, the IV result are more difficult to interpret than the previous results. A standard Hausman test (p-value of 0.96) indicates that

[25] Individual income is not available for some years that household income is available, so the number of observations decreases when we instrument household income with spouse income.

[26] The results also hold when age and its square are used as instruments in addition to changes in spouse income (a standard overidentifying test does not reject the validity of the instruments). Age and its square are frequently used in the labour supply literature to instrument wages (e.g. MaCurdy, 1981; Lee, 2001). The instrument will be valid, when age has no direct effect on time changes in commuting distance. The latter instrument is strong (and has the expected sign in the first stage). Given this instrument, both the point estimate as well as the standard error are much larger (0.47 with the standard error of 0.17). Although the point estimate is statistically significant, it is difficult to interpret this result as a 95% confidence interval is from 0.13 to 0.81. Nevertheless, this result supports our conclusion that income positively affects distance.

exogeneity of income cannot be rejected. Hence, the household income estimate effects in columns [1]–[3] are likely casual.

Our main finding is that household income has a positive effect on commuting distance, at least for two-earner households. To interpret this finding, one may wonder whether the different components of household income has the same effect on commuting distance. For this reason we distinguish between the effects of individual income and spouse household income. Table 2 presents estimates of the effect of household income on commuting distance through the separate effects of individual income and spouse household income. Because the effects of individual and of spouse income are likely additive, we use a linear-in-income specification.

Table 2. Estimates of Individual Income on Logarithm of Commuting Distance for Dual-Earner Households

	[1]	[2] constraint	[3]
individual income/100	0.0044 (0.0027)	0.0029 (0.0013)**	
hourly wage			0.0085 (0.0040)**
monthly workhours			0.0001 (0.0008)
spouse household income/100	0.0026 (0.0014)*	0.0029 (0.0013)**	0.0027 (0.0015)*
number of children	0.0655 (0.0440)	0.0713 (0.0429)*	0.0844 (0.0446)*
female with child	–0.1816 (0.1252)	–0.1916 (0.1241)	–0.2359 (0.1291)
year dummies	yes	yes	yes
number of observations	2,560	2,560	1,746
adjusted R^2	0.0403		0.0437

Notes: Observations are measured in differences; differences are based on the first and last observation for the same workplace spell in the movers sample. Equality constraint of estimates of individual income and spouse household income in [2].
**, * indicate that the estimates are significantly different than zero at the 0.05 and 0.1 level, respectively. Robust standard errors are in parentheses.

We now find that both income variables have a positive effect. Given this specification, the results are comparable to the aforementioned results: the implied household income elasticity is now 0.1594 (evaluated at mean individual income of €2,279 and mean spouse income of €2,275). The own income effect on distance appears to be somewhat larger than the income of the spouse, but the difference in estimates is likely spurious, as equality tests of the estimates of own income and spouse income indicate that equality cannot be rejected (t-tests<1). Equality of coefficients supports our previous conclusion that changes in household income can be interpreted as exogenous, because only the own income effect is thought to be endogenous. Imposing the constraint that both coefficients are the same, the corresponding estimate of household income remains the same: positive and around 0.13, see column [2].

To interpret these results, it is also insightful to note that the effect of individual income may be due to changes in number of hours worked or are due to changes in wage, see column [3].[27] When we allow for separate effects of hours worked and wage, it appears that the effect of changes in income is mainly through changes in hourly wages.[28] The implied elasticity of household income is robust to the previous results: 0.1180 (evaluated at the mean hourly wage of €13,161, 160.7 mean monthly hours, and mean spouse income of €2,275).

Finally, we re-estimated the models for two-earner households separately for females and males, because there may be differences in commuting patterns by gender as a response to changes in income, see Table A3. The sample size is too small to expect any precise results with IV. Nevertheless, these results do not invalidate our previous conclusion that the effect of household income on distance is non-negative, so there is *not* a negative effect.

4. Conclusions

The equilibrium relationship between household income and the length of the commute is one of the most well-known applications of urban economic theory. Empirically, we know very little about this. This paper seeks to determine the (long-run) effect of income on commuting distance. The paper addresses possible reverse causality bias by keeping workplace location constant. For Germany, we find a non-negative long-run income elasticity of distance. For one-earner households, the elasticity is either zero or slightly positive. For two-earner households, the elasticity is positive in all specifications and about 0.14. Our results imply that rich households tend to move farther away from the workplace, in line with suggestions for US cities (Brueckner et al., 1999).

[27] Hourly wage is not available for some years that household income is available, so the number of observations decreases when we control for wage.

[28] Workhours may be endogenously chosen, as workers are likely heterogeneous in their preference for leisure time and therefore workhours. In Germany this may not be so relevant, because of constraints on total labour supply set by employers: collective bargaining agreements, as well as European Union labour laws. In addition, first differencing is a way of dealing with worker fixed effects which capture time-invariant unobserved workhours preferences of workers. Gutiérrez-i-Puigarnau and Van Ommeren (2010) find a very small effect of commuting distance on overall labour supply. This implies that, when examining the effect of labour income on distance, endogenous changes in workhours are likely not problematic.

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Appendix A: Tables

Table A1. Descriptive Statistics of Movers Sample

	movers and non-movers sample		movers sample					
	all households		all households		2-earners		1-earner	
	mean	sd	mean	sd	mean	sd	mean	sd
commuting distance (in km)	16.57	18.53	17.60	18.85	17.50	18.95	17.73	18.70
monthly household income (in €)	3,227	1,874	3,061	1,793	3,796	1,819	1,989	1,046
monthly individual income (in €)	2,267	1,148	2,156	1,119	2,279	1,154	1,989	1,046
spouse income (in €)	1,239	1,671	1,199	1,538	2,275	1,428	0.00.0	0.000
hourly wage (in €)	13.02	6.506	12.28	6.261	13.09	6.473	11.20	5.793
monthly workhours	164.7	50.85	165.3	48.151	160.7	50.472	172.2	43.60
one-earner household	0.429	0.495	0.402	0.490	0.000	0.000	1.000	0.000
presence of a partner	0.607	0.488	0.631	0.483	1.000	0.000	0.083	0.276
female partner	0.274	0.446	0.295	0.456	0.472	0.499	0.009	0.094
number of children	0.725	0.987	0.764	1.010	1.047	1.080	0.344	0.712
female with a child	0.190	0.393	0.187	0.390	0.231	0.421	0.121	0.326
number of observations	26,878		6,269		3,747		2,522	

Table A2. Estimates of Household Income on Logarithm of Commuting Distance with Interactions

	[1]	[2] 2-earners	[3] 1-earner
household income (log)	0.0917 (0.0448)**	0.1389 (0.0484)**	0.0556 (0.0710)
household income (log) x 7 plus years spell	–0.0407 (0.0668)	0.00003 (0.0859)	–0.1571 (0.1097)
one-earner household	0.0633 (0.0510)		
presence of partner	0.2802 (0.0747)		0.2121 (0.1156)*
female partner	–0.2602 (0.0761)**		–0.2518 (0.1417)*
number of children	–0.0176 (0.0263)	–0.0022 (0.0292)	–0.0001 (0.0528)
female with child	–0.1147 (0.0726)	–0.2797 (0.0876)**	–0.0362 (0.1264)
year dummies	yes	yes	yes
number of observations	6,269	3,747	2,522
adjusted R^2	0.0377	0.0465	0.0244

Notes: Observations are measured in differences; differences are based on the first and last observation for the same workplace spell in the movers sample.

**, * indicate that the estimates are significantly different than zero at the 0.05 and 0.1 level, respectively. Robust standard errors are in parentheses.

Table A3. Estimates of Household Income on Log of Commuting Distance by Gender for Dual-Earner Households

	[1] females	[2] females IV	[3] males	[4] males IV
household income (log)	0.1168 (0.0561)**	0.1039 (0.0865)	0.1572 (0.0686)**	0.2073 (0.1269)
number of children	–0.1118 (0.0429)**	–0.0985 (0.0434)**	–0.0168 (0.0322)	0.0520 (0.0491)
year dummies	yes	yes	yes	yes
num. observations	1,700	1,009	2,047	1,551
adjusted R^2	0.0447		0.0597	
instrument		change spouse income (log)		change spouse income (log)
F (instrument)		1102.59		564.87

Notes: See Table 1.

Publisher:

CPB Netherlands Bureau for Economic Policy Analysis
P.O. Box 80510 | 2508 GM The Hague
T (070) 3383 380

May 2013 | ISBN 978-90-5833-600-2