

Long-Run Peer Effects: Some Danish Evidence

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Abstract

This paper examines long run peer effects in Denmark. Due to data limitations, the huge peer effect literature has so far concentrated on short run outcomes such as end-of-year test-scores, leaving open the question of whether peer effects stick or fade out. I use Danish administrative data to allow for a long run analysis, using taxable income as the outcome of interest. The administrative registers contain several cohorts of students for each school, while at the same time providing a wealth of background information about the parents and hence the class-room composition. In addition, the registers allow tying students to their tax-returns later in life. This data structure allows identifying the peer-effect parameters through within-school variation. I provide evidence that there are detectable effects twenty years after completing 9th grade - class-room composition with respect to observables matter in the long run. Finally, an analysis of variance suggests that there is an additional "class effect" that cannot be attributed to class-room observables.

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1 Introduction

This paper asks a very simple question: Do peer effects stick or fade out? That is, is it possible to establish a connection between observable peer characteristics and long run labor market outcomes?

However narrow this question may seem, it is an important one: if peer effects fade, why spend time and energy on the more subtle questions of disentangling how they might work and why they are present. Since after all, test scores and grades are only a noisy signal of what schooling is really about: the underlying human capital, cognitive and non-cognitive skills. This is the first paper in the literature to successfully establish such a connection between observable peer characteristics and long-run outcomes.

Due to data limitations, only a few papers of the extensive peer effects literature consider anything but short term outcomes such as end-of-year test scores¹. Data from administrative registers in Denmark allow looking at students graduating 9th grade in the early 1980's and their corresponding income 20 years later. Chetty et al. (2011a) use the randomized data from Project STAR combined with tax records to construct a similar data set, but the analysis with respect to observable peer characteristics is not powerful enough to establish any link between peer observables and income. They do however establish a link between unobservables and income - a one standard deviation increase in classroom quality increases earnings by roughly 10% at age 27 in their sample. In a somewhat related paper, Chetty et al. (2011b) look at the long term impact of teachers and find substantial impact of good teachers on adult outcomes. Another contribution looking at intermediate outcomes is Dynarski et al. (2011) who, also using the Project STAR data, look at college enrollment and degree-completion and find a positive effect of the treatment (small class size in kindergarten-3rd grade) on both outcomes.

Ever since Manski (1993) a substantial amount of time and effort has been put into the question of how to separately identify the different ways in which peers might affect each other. The identification issues are tricky (for an introduction to the issues involved and an overview of recent literature, see Cooley (2010)), and the only reason the question asked in this paper is simple, is that the question here is only whether or not these effects are present when looking at the long run, not how and why. That is, by taking a reduced form approach most of the challenges are avoided, and the only remaining real identification problem is then student se-

¹An overview of the literature is given in Sacerdote (2011) and Gibbons (2008)

lection into peer groups. As my data is not randomized, I rely on the idiosyncratic variation in class-level peer composition across cohorts within a school along the lines of Hoxby (2000), Hanushek et al. (2009) and Lavy and Schlosser (2011).

The paper is organized as follows: Section 2 discusses the estimation model and the identification strategy, section 3 gives a background on the Danish administrative data and section 4 discusses the results. Section 5 conducts an analysis of variance along the lines of Chetty et al. (2011a), and section 6 concludes.

2 Empirical strategy

I follow the literature on peer effects in taking a standard linear-in-means achievement production function as the starting point for the analysis. Let Y_{icst} denote student i 's achievement in class c , at school s , at time t . X_{icst} is observed individual characteristics that includes gender, age and immigration status as well as parental information on education, unemployment and income. μ_{cst} captures unobserved input at the classroom level. In addition, θ_s is a school fixed effect and D_t is a year fixed effect. The achievement production is then

$$Y_{icst} = X_{icst} \gamma_x + \bar{X}_{-icst} \gamma_{\bar{x}} + \bar{Y}_{-icst} \gamma_{\bar{y}} + \mu_{cst} + \theta_s + D_t + \varepsilon_{icst} \quad (1)$$

where spillovers work through endogenous effects (\bar{Y}_{-icst}) and exogenous effects (\bar{X}_{-icst}), using the Manski (1993)-terminology. As is well-known in the literature, solving the simultaneity problem and separately identifying $\gamma_{\bar{x}}$ and $\gamma_{\bar{y}}$ is very difficult because it requires exclusion restrictions that affect students within the same peer group differently (Moffitt and Comments, 2001). While Fruehwirth (2012) actually uses a student accountability policy in North Carolina as such an exclusion restriction, most applications instead turn to the simpler problem of estimating a reduced form model

$$Y_{icst} = X_{icst} \Pi_x + \bar{X}_{-icst} \Pi_{\bar{x}} + \mu_{gst} + \theta_s + D_t + \varepsilon_{icst} \quad (2)$$

where Π_x and $\Pi_{\bar{x}}$ are functions of the peer group size and the structural parameters of equation (1). The parameter of interest is then the social effect parameter $\Pi_{\bar{x}}$, and $\Pi_{\bar{x}} \neq 0$ will only hold if either $\gamma_{\bar{x}} \neq 0$ or $\gamma_{\bar{y}} \neq 0$ (or both). In other words, identifying and estimating $\Pi_{\bar{x}}$ will be informative on whether peer effects exist or not, but will provide no information about the

channel through which they operate. One implication of this is that the presence of social multipliers cannot be determined.

The key challenge to identification of $\Pi_{\bar{x}}$ is non-random assignment into a peer group. That is, if it holds that

$$E[\varepsilon_{icst} | X_{icst}, \bar{X}_{-icst}, \theta_s, D_t] = 0 \quad (3)$$

then $\Pi_{\bar{x}}$ can be estimated consistently through standard linear regression methods. There are two main concerns: One concern is that high-ability students select into classrooms with other high-ability students. Since (2) contains a school fixed effect, within-school variation is what drives the estimate of $\Pi_{\bar{x}}$, and so the identifying assumption holds as long as students cannot predict or influence the characteristics of peers conditional on attending school s - in other words the within school variation of \bar{X}_{-icst} needs to be idiosyncratic. This assumption seems plausible and estimation along these lines has been performed in several applications, including Lavy and Schlosser (2011), Hanushek et al. (2009) and Hoxby (2000).

A second concern is that students sort with respect to unobserved classroom inputs μ_{cst} within the same school in the same year. Hoxby (2000) argues that this is likely to happen and describes how this can either happen through parents pressuring the school to put students in particular classrooms with perceived good teachers, or through the school assigning teachers and other resources based on classroom characteristics. Hoxby (2000) notes that such non-random assignment within school-by-year is not a problem, as long as estimation is performed using class-level data instead of classroom-level data². As class-level data is what I have available, the analysis is robust to such within-class sorting into classrooms, even if such sorting seems highly unlikely in an extremely egalitarian school system like the Danish one. The parameter estimates can still be interpreted at the classroom level.

Since this paper looks at long run outcomes and uses the within-school variation to identify parameters, it is expected that power is an issue in the estimations. Particularly, since there is a lot of individual background information, the dimensionality of X_{icst} is simply high, potentially rendering the peer effects estimated with very low precision. Consider instead an identifying assumption of

$$E[\varepsilon_{icst} | \bar{X}_{-icst}, \theta_s, D_t] = 0 \quad (4)$$

²Clarifying the terminology: I use the word class in the same way as (Hoxby, 2000) uses cohort: the group of students at a particular school attending the same grade. Hence there may be a number of classrooms within the same class.

Comparing this to (3), equation (4) requires that assignment to peers is not only random based on unobservables but also on observables. In this application where there is a school fixed effect in the model, the same arguments that apply for the random assignment on unobservables mentioned earlier will hold for the observables. In fact, it seems hard to argue that the assumption in (3) would hold but the assumption in (4) would not: how would parents be able to select into peer groups based on observables (such as gender), but not based on an unobserved characteristic (such as ability).

Ultimately, I'm estimating the following specifications:

$$Y_{icst} = X_{icst}\Pi_x + \bar{X}_{-icst}\Pi_{\bar{x}} + \theta_s + D_t + \varepsilon_{icst} \quad (5)$$

$$Y_{icst} = \bar{X}_{-icst}\Pi_{\bar{x}} + \theta_s + D_t + \varepsilon_{icst} \quad (6)$$

with the outcome of interest being log-income twenty years after graduating 9th grade.

Both are estimated with standard fixed effect estimation, allowing the error terms to be clustered within class. Both specifications give consistent estimates of $\Pi_{\bar{x}}$ under the assumption in (4), while only the first specification gives consistent estimates if the assumption in (3) holds but the assumption in (4) fails.

An important thing to keep in mind is the reduced form nature of the model. In particular, unobserved ability of peers may be an important channel through which a social effect operates. As it is very likely that such unobserved characteristics of peers are correlated with the observed characteristics of the peers, any significant effect along any peer characteristics may simply be driven by those characteristics being the ones with the highest correlation with the unobserved peer characteristics. This is a valid point for almost the whole peer effects literature³, but it is not necessarily a problem in itself. The reduced form peer effects parameter $\Pi_{\bar{x}}$ is of interest in and of itself, especially in establishing whether or not peer effects stick in the long run. However, any causal interpretation should be avoided, and the parameter is not very informative on policy.

³An exception is (Arcidiacono et al., 2012) who develop a method to recover spillovers from unobservables

3 Data

I use data from two sources in this analysis. First, data from administrative records collected by the ministry of education ("elevregisteret") covers the full population of students finishing 9th grade back to 1980 and contains identifiers of students, the school and year of graduation. It does not contain any information on grades or any classroom identifier⁴. The second data source is the Data Base for Labor Market Research ("IDA"), which contains administrative records on all Danish residents back to 1980. The data set contains information about labor market variables such as wages and other income and unemployment in addition to a wealth of background information about education and family characteristics⁵. Notably, the database contains a link between the person identifier and the person identifiers of the parents.

These two data sources are combined into a data set that contains information about a student's parents with respect to income, labor market status and education levels; information about the student with respect to gender, immigrant status and age; and linking this information at the time of graduating 9th grade to the individual's income twenty years later. In addition, since the school and year of graduation is known, the information on the individual level can be aggregated to the class-level, creating peer observable characteristics. These peer characteristics are created as leave-one-out means, such that for student i in class c at schools at time t , the peer characteristics are means for the rest of the students ($-i$) belonging to the same cst class.

As the outcome of interest for students I use *total taxable income* which includes all major sources of income including wages, income as self employed, capital gains, retirement benefits and social security benefits, and other public benefits such as paid maternity leave and unemployment benefits. The variable is measured twenty years after graduating 9th grade.

As individual characteristics I use *age*, *gender* and *immigrant status*. The immigrant status classifies an individual as either native, immigrant or descendant. An immigrant is defined as an individual born outside Denmark with neither parent being a Danish citizen born in Denmark, while a descendant is an individual born in Denmark with neither parent being a Danish citizen born in Denmark. Anybody not in those two groups are classified as a native Dane. This means that students with one parent who is a Danish citizen born in Denmark will be classified as native independently of place of birth. For the parental income information, I again use total

⁴Detailed data of grades from 9th grade final exams are only available from 2002, making an analysis combining the short-run and long-run outcomes unfeasible at this point.

⁵The data base is only well-documented in Danish; some further information is given in Abowd and Kramarz (1999).

taxable income but group the household income into quartiles, leaving the second quartile as the reference group. For the unemployment information, I combine into 5 groups: Employed with no unemployment in the year (reference group), low unemployment (unemployment of less than a total of 3 months in the calendar year), medium unemployment (3-6 months), high unemployment (more than 6 months) and not in labor force. Finally, the education information is combined into a fairly standard grouping of education in the Danish data, containing 5 categories: 9th grade or less (the mandatory minimum amount of schooling) used as reference group, vocational and short further education, medium further education, long further education, and finally an indicator for unknown education level⁶.

The peer characteristics are calculated from the individual background characteristics. As the characteristics are all indicators, the peer variables are of the form "fraction of other students in the same class with characteristic...", eg. "the fraction of other students' fathers with vocational training". I calculate these peer variables both treating mothers and fathers separately, and pooling the information.

3.1 Sample selection

It is worth noting, that while students who I cannot find in the administrative registers twenty years after graduating are left out of the regression, they still matter in calculating the class peer characteristics. The gender, immigrant and age characteristics of the class is accurate for all classes irrespectively of students not entering the estimation sample. The parental background information is less precise since not all parents can be identified in the registers, but again these calculations are not affected by peers not being in the registers twenty years after graduating.

I make two important sample selection choices. The first is the sample period. I include all students who graduated 9th grade in 1980-1985 and look at income data twenty year later, 2000-2005. To avoid dealing with any age effects of income, I want to look at income a fixed number of years after graduating. At the same time, the time passed between graduating 9th grade and observing income needs to be long enough that individuals have completed their education and that those with long further education have had some time to catch up. As it is very common not to finish master-level programs until the late twenties, a fairly long window is

⁶It is not straightforward to translate these education categories into "college" or not. The reason for this is that the Danish education system until recently did not really have academic bachelor degrees available: after high school completion students must choose between either 5-6 year academic master programs or the shorter 3-4 year professional degrees, the latter consisting of a mix of schooling and practical training. As such, even though these programs are on the bachelor level they were non-academic in nature with little or no option of further education in the field after completion. Examples of such programs are school teachers, nurses, police officers, lab technicians etc.

needed. Looking at income twenty years after graduation accomplishes this while at the same time allowing a reasonable panel in the school dimension (up to 6 classes pr school).

The second selection rule is school size. There are a number of very small schools that are excluded from the sample. These are mostly special institutions for kids with disabilities and other non-standard institutions. As these schools will have a very high amount of within school variation from class to class because of the size while it at the same time is very likely that spillovers work differently at these schools it is preferable to leave them out of the estimation sample. The analysis also excludes schools that by Danish standards are very big (≥ 100 students pr. class). Finally, a few international schools where extremely few students are in the register with income observations are left out.

Summary statistics for the estimation sample is shown in table 6 in the appendix.

4 Results

The main results of the paper are presented in table 1. The table displays four columns, each representing a linear regression with school and year fixed effects and where the parent characteristics of the peers are based on pooled information. Column 1 contains estimates for the full set of peer variables without individual demographic controls, and column 2 shows the same regression with individual demographic controls. Column 3 and 4 takes a subset of the peer variables in an attempt to gain a little more power, again with the latter column containing the demographic controls.⁷

One thing to note is that the amount of variation of log-income that is explained is low, even with the individual demographic controls. This is not too surprising as there is a lot of wage dispersion in any income cross section, and in this application this dispersion is only explained by variables twenty years earlier (even before knowing education level or quality). Yet, it does seem like the class has an impact on income: the peer effect parameters on parents long further education, parents not in labor force, immigrant classmates, low-income classmates and to some extent the gender composition are all significant. Although they are not estimated too precisely, the size of these estimates are non-negligible. As a classroom typically has the size of roughly 20 students, replacing 2 students of a reference group (eg. parents with only 9 years

⁷I show estimation result from running log-income on the demographic controls without spillovers in table 7 in the appendix. As expected, these variables are highly significant, but the amount of explained variation is low. It is worth noting that the parameter estimates are very similar to the estimates obtained in the peer effects specifications with demographic controls (parameter for the demographic controls not shown) and very stable across peer effect specifications.

Table 1: Main results

	Log-income			
	(1)	(2)	(3)	(4)
Fraction classmates parents vocational training	-0.0111 (0.0347)	-0.0041 (0.0342)		
Fraction classmates parents medium further education	-0.0046 (0.0690)	0.0136 (0.0678)		
Fraction classmates parents long further education	0.2139 (0.1308)	0.2267+ (0.1304)	0.2326+ (0.1265)	0.2519* (0.1250)
Fraction classmates parents unknown education	-0.1544 (0.1216)	-0.1426 (0.1186)		
Fraction classmates parents low unemployment	0.0707 (0.0636)	0.0463 (0.0633)		
Fraction classmates parents medium unemployment	0.0872 (0.1143)	0.0627 (0.1164)		
Fraction classmates parents high unemployment	-0.0226 (0.1317)	-0.0207 (0.1293)		
Fraction classmates parents not in labor force	0.1164* (0.0524)	0.0945+ (0.0487)	0.1032* (0.0488)	0.0817+ (0.0464)
Fraction female classmates	0.0759** (0.0245)	0.0179 (0.0240)	0.0758** (0.0245)	0.0178 (0.0239)
Average age of classmates	0.0138 (0.0343)	0.0140 (0.0348)		
Fraction immigrant classmates	-0.3651** (0.1399)	-0.2284+ (0.1373)	-0.3939** (0.1376)	-0.2570+ (0.1334)
Fraction descendant classmates	-0.1757 (0.2888)	-0.1435 (0.3084)	-0.1759 (0.2874)	-0.1438 (0.3062)
Fraction income quartile 1 classmates	-0.0781* (0.0348)	-0.0685* (0.0343)	-0.0737* (0.0299)	-0.0681* (0.0290)
Fraction income quartile 3 classmates	-0.0174 (0.0342)	-0.0196 (0.0342)		
Fraction income quartile 4 classmates	0.0128 (0.0380)	0.0212 (0.0373)		
Demographic controls	NO	YES	NO	YES
Year dummies	YES	YES	YES	YES
School fixed effects	YES	YES	YES	YES
R^2	0.0095	0.0336	0.0095	0.0336
Observations	372825	372825	372825	372825

** p<0.01, * p<0.05, + p<0.10

Robust standard errors in parentheses

of schooling) with 2 students with particular characteristics (eg. parents with long further education) will change the fraction of peers with respect to that group by 10%-points. And since the left hand side is log-income, parameter estimates can be interpreted (roughly) as percent income changes. So, a parameter estimate 0.25 of fraction of classmates parents with long further education (column 4) means that replacing 2 students from the reference group with two students with highly educated parents will change the income of the classmates by 2.5%. The impact of having immigrants in the class is negative but similarly sizeable: replacing 2 native students with two immigrants will decrease yearly income for classmates by 2.3%-3.9%.

The point estimates for the fraction of low-income classmates (classmates from households in the first income quartile) are smaller, but the effect is still important. This is particularly true since there is much more variation in the income quartile compared to the education and immigrant composition (only 2.7% of parents have long further education, while less than 1% of students are immigrants - by design a quarter of the student body belongs to a household in the first income quartile). Comparing a classroom from a mixed neighborhood with a quarter of the students from low-income household to a classroom with only middle-class families will result in $0.068 \cdot 0.25 = 1.7\%$ difference in yearly income from peer effects. Finally, the fraction of parents not in the labor force seems to matter too.

Again, it is important to emphasize that these parameters cannot be interpreted causally and contain very little information on policy. They are uninformative on the channels of the effects (endogenous effects versus exogenous effects) and are very likely to reflect correlation between the peer observables and the peer unobservables. Particularly, if income for parents is correlated with some unobserved ability and there is correlation between students and their parents unobserved ability, then it is very likely that what the household income peer effect is picking up is really spillovers from unobserved ability.

Finally, the estimations for gender composition are a little puzzling at a first glance. In the estimations without demographic controls, the estimate is highly significant and of some magnitude (replacing two boys in the classroom with girls changes income by 0.75%), this is consistent with Lavy and Schlosser (2011). However, as the only peer effect variable both the point estimate and the significance level changes considerably when including demographic controls. It is worth to note that the classroom gender composition is a variable with low-within school variation. Under the estimation assumption (4), the class gender composition and the individual demographic controls are orthogonal conditional on the fixed effects. Instead of interpreting the

change of significance as a sign of violation of the identifying assumption, I think the gender composition is an example of the power issues discussed earlier that arises with a fairly high dimensionality of controls.

Next, I reproduce the peer effect results for one set of variables at a time. I also look at separate effects for mothers and fathers. Table 2 shows results for the education spillovers. The effects when looking at the pooled parental information are similar to results in the main estimation, while there is not enough power to get significance for the separate spillover variables.

Table 3 looks at the unemployment variables in a similar way. When looking at the variables of pooled parental information, it is worth noting that the positive effect of parents not in labor force is not significant when not controlling for the other peer effects. Also, there seems to be a difference between mothers not in labor force (negative point estimate but not significant) and fathers not in labor force (positive significant effect). Finally, table 4 shows the rest of the peer effect variables estimated one by one. These variables behave in the same way as in the main estimations. The estimations of one set of peer effects at a time are performed in the hope that this will produce more power. As shown, estimates stay essentially the same. Peer variables seem to be somewhat uncorrelated with each other in the within-school dimension, pointing to the potential benefit of having multiple dimensions of peer input when assessing the magnitude of the reduced form peer spillovers.

5 Analysis of variance

As an additional check for long-run peer effects, I perform an analysis of variance along the lines of Chetty et al. (2011a). Consider

$$Y_{icst} = X_{icst}\Pi_x + \bar{X}_{-icst}\Pi_{\bar{x}} + \theta_s + \beta_{cs,t \neq T(s)} + D_t + \varepsilon_{icst} \quad (7)$$

where $\beta_{cs,t \neq T(s)}$ is a class effect for class c at school s at time t , leaving out a class effect in the last year the school is observed. This is of course the same model as a model with no school fixed effect and a full set of class fixed effects. However, testing $H_0 : \beta_{cs,t \neq T(s)} = 0$ for all c, s in this model and testing $H_0 : \beta_{cst} = 0$ for all c, s in the model with no school fixed effect is not the same. The latter tests if there is detectable co-variation in outcomes of students in the same class generally. This could be driven entirely by school effects or entirely by class effects, or a mix. In contrast, when estimating (7) the school fixed effect will be identified by students in

Table 2: Parental education spillovers

		Log-income		
	(1)	(2)	(3)	(4)
Fraction classmates parents vocational training	0.0051 (0.0328)	0.0116 (0.0324)		
Fraction classmates parents medium further education	0.0142 (0.0627)	0.0374 (0.0626)		
Fraction classmates parents long further education	0.2378+ (0.1273)	0.2615* (0.1268)		
Fraction classmates parents unknown education	-0.2261+ (0.1161)	-0.1788 (0.1136)		
Fraction classmates father vocational training			0.0268 (0.0276)	0.0218 (0.0275)
Fraction classmates father medium further education			0.0485 (0.0591)	0.0451 (0.0604)
Fraction classmates father long further education			0.1029 (0.0793)	0.1103 (0.0784)
Fraction classmates father unknown education			-0.0753 (0.0766)	-0.0868 (0.0765)
Fraction classmates mother vocational training			-0.0046 (0.0289)	0.0088 (0.0285)
Fraction classmates mother medium further education			-0.0312 (0.0465)	-0.0072 (0.0461)
Fraction classmates mother long further education			0.2178 (0.1484)	0.2358 (0.1467)
Fraction classmates mother unknown education			-0.2050+ (0.1181)	-0.1222 (0.1160)
Demographic controls	NO	YES	NO	YES
School fixed effects	YES	YES	YES	YES
Year dummies	YES	YES	YES	YES
R^2	0.0094	0.0336	0.0094	0.0336
Observations	372825	372825	372825	372825

** p<0.01, * p<0.05, + p<0.10

Robust standard errors in parentheses

Table 3: Parental employment spillovers

	Log-income			
	(1)	(2)	(3)	(4)
Fraction classmates parents low unemployment	0.0649 (0.0618)	0.0344 (0.0616)		
Fraction classmates parents medium unemployment	0.0625 (0.1118)	0.0403 (0.1135)		
Fraction classmates parents high unemployment	-0.0702 (0.1296)	-0.0640 (0.1275)		
Fraction classmates parents not in labor force	0.0294 (0.0440)	0.0181 (0.0422)		
Fraction classmates father low unemployment			0.0227 (0.0454)	0.0038 (0.0444)
Fraction classmates father medium unemployment			0.0142 (0.0942)	0.0164 (0.0984)
Fraction classmates father high unemployment			-0.1124 (0.1095)	-0.0934 (0.1092)
Fraction classmates father not in labor force			0.0836* (0.0397)	0.0770+ (0.0396)
Fraction classmates mother low unemployment			0.0481 (0.0599)	0.0370 (0.0606)
Fraction classmates mother medium unemployment			0.0586 (0.0804)	0.0332 (0.0799)
Fraction classmates mother high unemployment			0.0199 (0.0852)	0.0112 (0.0836)
Fraction classmates mother not in labor force			-0.0229 (0.0310)	-0.0280 (0.0297)
Demographic controls	NO	YES	NO	YES
School fixed effects	YES	YES	YES	YES
Year dummies	YES	YES	YES	YES
R^2	0.0094	0.0336	0.0094	0.0336
Observations	372825	372825	372825	372825

Robust standard errors in parentheses

** p<0.01, * p<0.05, + p<0.10

Table 4: Parental spillovers from other variables

	Log-income							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fraction female classmates	0.0764**	0.0177						
	(0.0245)	(0.0239)						
Average age of classmates			-0.0084	-0.0018				
			(0.0337)	(0.0338)				
Fraction immigrant classmates					-0.3865**	-0.2501+		
					(0.1352)	(0.1315)		
Fraction descendant classmates					-0.1790	-0.1480		
					(0.2910)	(0.3096)		
Fraction income quartile 1 classmates							-0.0661*	-0.0600+
							(0.0329)	(0.0324)
Fraction income quartile 3 classmates							-0.0228	-0.0237
							(0.0334)	(0.0335)
Fraction income quartile 4 classmates							0.0171	0.0308
							(0.0350)	(0.0344)
Demographic controls	NO	YES	NO	YES	NO	YES	NO	YES
School fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Year dummies	YES	YES	YES	YES	YES	YES	YES	YES
R ²	0.0094	0.0336	0.0094	0.0336	0.0094	0.0336	0.0094	0.0336
Observations	372825	372825	372825	372825	372825	372825	372825	372825

Robust standard errors in parentheses

** p<0.01, * p<0.05, + p<0.10

Table 5: Analysis of variance

Unobserved Class Effects: Analysis of Variance			
	Log-Income		
	(1)	(2)	(3)
p-value of F-test on school-by-year fixed effects	0.000	0.000	0.000
SD of class effects (RE-ML)	0.0588	0.0396	0.0275
Year dummies	YES	YES	YES
School fixed effects	YES	YES	YES
Demographic controls	NO	YES	YES
Observable classroom characteristics	NO	NO	YES
Observations	419057	373844	373844

the class of the last year the school is in the panel ($t = T(s)$). The class fixed effects are then measured relative to this, and so testing $H_0 : \beta_{cs,t \neq T(s)} = 0$ is testing if there is more clustering of outcomes on the class-level than the amount imposed by the school-effect. I perform F-tests to jointly test if all class-effects are zero.

Finally, I estimate (7) under the assumption that the class effects are random and normally distributed. This approach then provides an estimate of the standard deviation of the class effect. I perform both the fixed effects F-test and the random effects estimation without controls, with individual demographic controls, and with both individual demographic controls and observable peer characteristics ⁸

The results in table 5 suggest that class-level fixed effects are significant, and that there are substantial clustering at the class level. A one standard deviation in class-quality is associated with yearly income increases of 2.8% – 5.9%. Although smaller in magnitude, these results are consistent with Chetty et al. (2011a), who find significant classroom effects on wages. They find that a one standard deviation in classroom quality increase wage earnings at age 27 by approximately 1500 USD, which corresponds to roughly 10% of the mean earnings in sample.⁹

⁸The last estimation is possible because there is within-class variation in the peer effects as they are constructed as leave-one-out means. It is probably not a good way to identify class fixed (or random) effects and peer effects separately, but the point of this analysis of variance is merely to quantify how much clustering at the class-level is left unexplained by the observable peer characteristics.

⁹The difference in the method used might explain this as (Chetty et al., 2011a) uses wages as the outcome of interest, including a lot of zero-observation in their estimation data set.

6 Conclusion

This paper asks a very simple question: Do peer effects stick or fade out? That is, is there any evidence that observable peer characteristics influence long run labor market outcomes? The question is answered in the most reduced form sense possible, since the estimated social effects cannot distinguish between endogenous effects or simultaneous effects on the one hand and exogenous effects on the other hand. On top of that, there is no way of determining if estimated effects of observable peer characteristics are simply driven by unobserved peer characteristics such as ability. Nevertheless, this paper is the first to establish a significant link between peer characteristics and long run income. In particular, low-income peers and immigrants seem to have a negative effect on students income later in life, while peers from highly educated backgrounds and the number of females in the class seem to positively affect the classroom. In addition, an analysis of variance reveals that there is significant clustering at the class level from unobserved characteristics on top of the effects from the observable peer characteristics.

As the question of long run peer effects is answered only in a reduced form sense, the results in this paper provide no insight into the channels through which peer effects might work; and they provide no guidance for policy. They do however suggest that peer effects are sticking and they are of magnitudes that matter.

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A Appendix

A.1 Summary statistics

Table 6: Summary statistics

Variable	Mean	Std. Dev.	Min.	Max.
Log income	12.381	1.125	0	16.804
Student age	35.982	0.38	33	38
Student female	0.5	0.5	0	1
Students father vocational training	0.447	0.497	0	1
Students father medium further education	0.09	0.285	0	1
Students father long further education	0.047	0.211	0	1
Students father unknown education	0.03	0.17	0	1
Students mother vocational training	0.33	0.47	0	1
Students mother medium further education	0.093	0.291	0	1
Students mother long further education	0.01	0.097	0	1
Students mother unknown education	0.012	0.107	0	1
Student immigrant	0.006	0.078	0	1
Student descendant	0.002	0.04	0	1
Students household in income quartile 1	0.18	0.384	0	1
Students household in income quartile 3	0.278	0.448	0	1
Students household in income quartile 4	0.272	0.445	0	1
Students father low unemployment	0.086	0.281	0	1
Students father medium unemployment	0.025	0.155	0	1
Students father high unemployment	0.012	0.11	0	1
Students father not in labor force	0.093	0.29	0	1
Students mother low unemployment	0.062	0.242	0	1
Students mother medium unemployment	0.022	0.146	0	1
Students mother high unemployment	0.016	0.126	0	1
Students mother not in labor force	0.214	0.41	0	1
Fraction classmates parents vocational training	0.358	0.103	0	0.737
Fraction classmates parents medium further education	0.086	0.052	0	0.556
Fraction classmates parents long further education	0.027	0.039	0	0.438
Fraction classmates parents unknown education	0.02	0.019	0	0.353
Fraction classmates parents low unemployment	0.072	0.037	0	0.343
Fraction classmates parents medium unemployment	0.023	0.019	0	0.294
Fraction classmates parents high unemployment	0.014	0.014	0	0.176
Fraction classmates parents not in labor force	0.154	0.06	0	0.727
Fraction female classmates	0.474	0.077	0	1
Average age of classmates	35.988	0.084	35.143	37.222
Fraction immigrant classmates	0.008	0.025	0	0.609
Fraction descendant classmates	0.003	0.009	0	0.308
Fraction income quartile 1 classmates	0.244	0.106	0	1
Fraction income quartile 3 classmates	0.251	0.075	0	0.727
Fraction income quartile 4 classmates	0.25	0.142	0	0.935
N students		372825		
N schools		1536		
N years		6		
N classes		8484		

A.2 Regression of demographic controls

Table 7: Demographic controls

	Log-income		
	coef	se	tstat
Student age	-0.0827**	(0.0049)	(-16.86)
Student female	-0.3038**	(0.0072)	(-42.14)
Students father vocational training	0.0138**	(0.0038)	(3.652)
Students father medium further education	0.0348**	(0.0073)	(4.755)
Students father long further education	0.0578**	(0.0138)	(4.199)
Students father unknown education	0.0071	(0.0140)	(0.506)
Students mother vocational training	0.0412**	(0.0041)	(10.05)
Students mother medium further education	0.0422**	(0.0100)	(4.223)
Students mother long further education	0.0153	(0.0310)	(0.495)
Students mother unknown education	-0.0538*	(0.0260)	(-2.067)
Student immigrant	-0.2663**	(0.0479)	(-5.558)
Student descendant	-0.2491**	(0.0850)	(-2.932)
Students household in income quartile 1	-0.0451**	(0.0074)	(-6.123)
Students household in income quartile 3	0.0143**	(0.0051)	(2.806)
Students household in income quartile 4	0.0571**	(0.0053)	(10.74)
Students father low unemployment	-0.0416**	(0.0061)	(-6.832)
Students father medium unemployment	-0.0633**	(0.0109)	(-5.825)
Students father high unemployment	-0.0875**	(0.0149)	(-5.885)
Students father not in labor force	-0.0821**	(0.0094)	(-8.770)
Students mother low unemployment	-0.0361**	(0.0071)	(-5.098)
Students mother medium unemployment	-0.0585**	(0.0117)	(-4.999)
Students mother high unemployment	-0.0686**	(0.0166)	(-4.131)
Students mother not in labor force	-0.0647**	(0.0048)	(-13.60)
School fixed effects	YES		
Year dummies	YES		
R^2	0.0336		
Observations	372825		

Robust standard errors in parentheses

** $p < 0.01$, * $p < 0.05$, + $p < 0.10$