The Causal Effect of Studying on Academic Performance

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Abstract
While substantial recent attention has been paid to understanding the determinants of educational outcomes, little is known about the causal impact of the most fundamental input in the education production function - students' study effort. In this paper, we examine the causal effect of studying on grade performance by taking advantage of unique new data that have been collected specifically for this purpose. Important for understanding the potential impact of a wide array of education policies, the results suggest that human capital accumulation is far from predetermined at the time of college entrance.

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Abstract
While substantial recent attention has been paid to understanding the determinants of educational outcomes, little is known about the causal impact of the most fundamental input in the education production function - students' study effort. In this paper, we examine the causal effect of studying on grade performance by taking advantage of unique new data that have been collected specifically for this purpose. Important for understanding the potential impact of a wide array of education policies, the results suggest that human capital accumulation is far from predetermined at the time of college entrance.

Section 1. Introduction

While substantial recent attention has been paid to understanding the determinants of educational outcomes, little is known about the causal impact of the most fundamental input in the education production function - students' study effort. One primary reason for the current lack of information is that standard data sources have not traditionally collected information about how much time students spend studying. However, there is another important reason for the current lack of information; because the amount of time a person studies is endogenously determined, a sample correlation between study quantity and grade performance may not represent a useful estimator of the causal effect of studying. The difficulty of providing information about the causal effect of studying is highlighted by an ambitious ten year study by Schuman et al. (1985) at the University of Michigan. The authors took four different approaches for measuring study effort in an explicit attempt to "produce a positive relation between amount of study and GPA," but could not uncover a (conditional) correlation which indicated evidence of the "hypothesized substantial association."[1]

In this paper we provide new evidence about the production of first-semester college grades by taking advantage of both a useful institutional detail at Berea College and unique new data from the Berea Panel Study (BPS) that we collected specifically for the purposes of this paper. With respect to the institutional detail, our identification strategy relies heavily on the fact that Berea assigns first-year roommates using a mechanism that is unconditionally random, so that the academic ability, motivation, and all other characteristics of a student at the time of assignment are uncorrelated with the characteristics of his/her roommate. This detail allows us to partition students into groups that are identical in all respects at the time of college entrance except that the students in the different groups are assigned roommates of different observable type. Then, differences in average college grade performance between the groups can only arise if some grade inputs which are determined after the time of college entrance are influenced by the observable type of a student's roommate. Thus, if one could observe the grade inputs of students that are determined after

[1]Similar replication results at different schools by Hill (1991) and Rau and Durand (2000) produced generally similar results. Within the recent economics literature, Stinebrickner and Stinebrickner (2004) estimated the descriptive relationship between a student's first semester grade performance and his/her average daily study hours using the same data as in this paper. For theoretical work related to study effort, see, for example, Becker (1982).

the time of college entrance and are influenced by the observable type of a student's roommate, then one could estimate the causal effect of these inputs on academic performance.

With respect to the uniqueness of the data, the complete flexibility we had in designing the BPS survey instruments allows us to deal with the two potential difficulties that would typically render the identification strategy described in the previous paragraph infeasible. The first potential difficulty is that it is not trivial to find observable characteristics of roommates that can be used to generate student groups which differ in terms of average college grades; indeed, past research examining peer effects in higher education has found little evidence that a student's grade performance is related to observable academic characteristics of his/her roommate (Sacerdote, 2001, Zimmerman, 2003). Of relevance for understanding this difficulty, Stinebrickner and Stinebrickner (2006) suggested that, in the short-run, a student's ability is to a large extent fixed, but that the student's decisions about time-use may be influenced by peers. This motivated us to use the BPS to collect a variety of unique information about a student's roommate that could possibly influence the student's time-use. For example, of central importance for this paper, the BPS reveals whether a student's first-year roommate brought a video game with him/her to school at the beginning of the academic year. The second potential difficulty associated with the identification strategy described in the previous paragraph is that standard data sources typically do not provide information about the amount that a student studies or the other grade inputs that are potentially determined after the time of college entrance and, therefore, are potentially influenced by roommates. This reality motivated us to include in the BPS multiple 24-hour time diaries and additional survey questions that allow us to characterize study quantity and a long list of other college choices and behaviors (e.g., class attendance, sleeping, partying, study location, paid employment, interactions with peers, etc.) that we could imagine influencing grade performance.

We take a sequential approach to learn as much as possible about the grade production function. Our key finding in Section 3 is that whether a student's roommate brings a videogame to school has a strong causal effect on the student's grade performance. Even without any additional information, this result provides strong evidence that important inputs in the grade production function can be influenced after college entrance. That is, important for a variety of policy conclusions, grade performance is far from predetermined at the time of college entrance.

Section 4 involves examining what inputs are responsible for the difference in average grade performance between students whose roommates bring videogames and students whose roommates do not bring videogames. Noting that the presence of a roommate videogame could affect a student's grades either because the physical game itself influences the student's inputs or because roommates who bring games are systematically different in ways that influence the student's inputs, we group the set of inputs that could be influenced by the presence of the roommate videogame into three broad categories: class attendance, study quantity, and study efficiency. Not surprisingly given our knowledge of institutional details, we find that class attendance is not influenced by the presence of a roommate videogame. Thus, even without any additional information, we can conclude that studying - defined generally to include both quantity and efficiency - plays a very important causal role in determining grade performance.

In an attempt to refine our conclusion further, we provide evidence about whether it is study quantity or study efficiency (or both) that causes the grade difference between students whose roommates bring video games and students whose roommates do not bring videogames. We find evidence of substantial differences in study quantity between the groups, but no evidence of any differences in study efficiency. Given our desire to be appropriately cautious, we note that it would never be possible to establish with certainty that study efficiency is identical between two groups of students. For example, it would never be possible to measure whether even small amounts of videogame playing could harm a student's short-term thinking skills to some extent. Nonetheless, given that our unique data allow us to rule out a seemingly close-to-exhaustive set of reasons that study efficiency may be different between the two groups and given that the evidence of differences in study quantity between the two groups is strong, it seems reasonable to conclude that study quantity plays a central role in determining grade performance.

Further, if we are willing to believe from the findings described in the previous paragraph that the difference in study efficiency is trivial between students whose roommate bring videogames and students whose roommates do not bring videogames, we can formally quantify the causal effect of study quantity on academic performance with an Instrumental Variable (IV) strategy in which the presence of a roommate videogame serves as an instrument for a student's study quantity. As described in Section 5, this IV estimator is intuitively straightforward; it bears a very close relationship to the Wald estimator that attributes the difference in average grades between the group of students whose roommates bring videogames and the group of students whose roommates do not bring games to the difference in average study quantity between these groups. Thus, given that both average grades and average study quantity are lower for the group whose roommates bring video games, it is not surprising that the IV estimate indicates that study quantity has a substantial effect on grade performance. We obtain additional support for this conclusion when we disaggregate our videogame variable into different types of games, when we create a somewhat different form of the instrument by interacting whether a student's roommate brought a video game with whether the student himself brought a videogame, and when we take advantage of other potential instruments that are available in our data.

As discussed in Section 6, the IV estimates are much larger than the Ordinary Least Squares (OLS) estimate. The unobservable in our grade equation captures both permanent attributes of individuals and semester specific influences on grade performance. As such, endogeneity problems could arise either because students who spend more time studying have different permanent, unobserved attributes such as ability or because of a "dynamic selection" effect in which students who receive bad grade shocks or have difficult classes during a particular semester react by changing their effort during that semester. With respect to the former possibility, we find no evidence that study quantity varies with our observable permanent measure of ability - a college entrance exam score. However, using a test which takes advantage of two semesters of data, we find evidence that the difference between the IV and OLS estimates can be explained by the dynamic selection effect. Not surprisingly given this finding, we find that a Fixed Effects estimator performs substantially worse than even OLS - with the estimate suggesting that, if anything, studying has a negative effect on grade performance. Thus, not only does this test provide some compelling evidence about the reasons for the difference we find between the IV and OLS estimates, but it also provides a general cautionary alarm about the use of certain types of estimators that one might be tempted to employ in the absence of the type of experiment utilized in this paper.

In Section 7 we discuss some additional reasons that this work is important. Among these, the work provides some of the strongest available evidence about the importance of peer effects in education, and, perhaps more importantly, provides some of the first evidence about a particular channel through which peer effects operate.

Section 2. A general overview of the Berea Panel Study and the sample used in this paper

Located in central Kentucky where the "bluegrass meets the foothills of the Appalachian mountains," Berea College is a liberal arts college which operates under a mission of providing educational opportunities to students of "great promise but limited economic resources." The survey data used in this paper are part of the Berea Panel Study (BPS) that Todd Stinebrickner and Ralph Stinebrickner initiated with the explicit objective of collecting the type of detailed information that is necessary to provide a comprehensive view of the decision-making processes of students from low income families. The BPS design involved attempting to survey all of the students in two Berea College entering classes approximately twelve times each year while they were in school and linking this survey data to administrative data from the school. Baseline surveys were administered to the students in the first BPS cohort prior to their freshman year in the fall of 2000 and were administered to the students in the second BPS cohort prior to their freshman year in the fall of 2001. Approximately 85% of all entering students in the two cohorts accepted our inviation to participate in the baseline surveys of the BPS.

In this paper we focus on the BPS cohort that entered Berea as freshmen in the fall of 2001 because some important information is not available for the other cohort. As mentioned earlier, our identification strategy takes advantage of the fact that students at Berea who do not request roommates are unconditionally randomly assigned roommates.[2] Slightly more than one-third of students at Berea either live off campus or

[2] Unlike students at most schools, freshmen at Berea are not asked to complete a housing preference questionnaire. To assign roommates, approximately two weeks before the start of school (and after all members of the freshman class are determined) pairs of roommates were drawn in a purely random fashion (for this cohort using a random number generator on the campus administrative computing system) from the pool of all freshmen who need roommates. Stinebrickner and Stinebrickner (2006) provide a set of empirical checks which find no evidence of a relationship between a student's observable characteristics and those of his/her roommate.

request a roommate. The sample used in this paper contains the 210 participants of the 2001 cohort who live on campus, did not request a roommate, and are not missing important administrative data needed for this paper.

Descriptive statistics for the entire sample are shown in the first column of Table 1. The first panel of Table 1 shows the outcome of interest - the grade point average in the first semester at Berea (GPA_i). On average, students have a GPA_i of 3.0 on a 4.0 scale. The second panel of Table 1 shows a variety of characteristics of students at the time of college entrance, as measured by either our baseline survey or by administrative data. The college entrance exam is the American College Test (ACT).[3] Forty-five percent of students in the sample are male and 17% of students in the sample are black. On the baseline survey, 37% percent of students report being in excellent health and slightly less than 7% of students report being in bad health. $MAJOR_1$,...,$MAJOR_7$ are college major variables where $MAJOR_i$ is equal to one if the student believes at the time of entrance that he/she is more likely to end up with $MAJOR_i$ than any of the other majors.

As discussed in the introduction, one reason that our identification strategy is feasible is that our data contain unique information about a student's roommate that could possibly influence the student's time-use. Information of this type is shown in the third panel of Table 1. Of central importance is the information from the first row of Question A of Appendix A which elicits whether, at the beginning of the year, a student's roommate brought a "video game" and whether a student's roommate brought a "computer game."[4] Our intention when we wrote the question was for the former to identify a stand-alone video console (e.g., Nintendo, Sony Playstation) and for the latter to identify the variety of videogames that were played on personal computers. However, it seems that somewhat different wording would have allowed us to more

[3]The ACT exam is taken by most students. In cases where students took the SAT, we converted SAT scores to ACT scores using equivalence tables.

[4]All first year dorm rooms are double rooms. Note that we focus primarily on a student's assessment of whether his roommate brought a videogame because this is available for all individuals in our sample. By contrast, a roommate's assessment of whether he brought a videogame is only available if the roommate also chose to participate in the BPS. Later we show that our primarily results are very similar when we use the roommate's assessment.

clearly distinguish between the prevailing categories of games in 2001.[5] Perhaps more importantly, as of the year 2001 it is not clear that differentiating between these categories of games is even useful/warranted; while the games played on personal computers in 2001 might have included low-level games like solitaire that would not be played on video consoles, the games played on personal computers also included very popular games such as Halo and Doom that were also available on a wide range of video consoles.[6] Therefore, we focus primarily on a variable that is created by simply combining the categories in Question A of Appendix A; defining the indicator variable $RGAME_i$ to be equal to one if student i's roommate brought either a "video game" or a "computer game" (or both), we find that $RGAME_i$ is equal to one for 36.7% of all students, 52.6% of male students, and 23.5% of females. However, throughout the paper we also show results when the $RGAME_i$ variable is disaggregated into its two categories which, for expositional convenience, we refer to as $RCONSOLE_i$ and $RCOMPUTER_i$. The third panel of Table 1 shows descriptive information about these two variables as well as information about how much a student's roommate studied in high school ($RSTUDYHS_i$) and how much, at the time of entrance, a student's roommate expected to study in college ($REXSTUDY_i$). We discuss the usefulness of this additional information in detail later.

As also discussed in the introduction, a second reason our identification strategy is feasible is that the BPS contains multiple 24-hour time diaries and additional survey questions that allow us to characterize the amount that students study and a long list of other college choices and behaviors that we could imagine influencing grade performance. The fourth panel of Table 1 shows information of this type. During the first semester, the time diaries, which are shown at the end of Appendix A, were collected on four different

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[5]Seemingly the most informative wording would have been to split games into three categories: video consoles, hand-held video games (e.g., Nintendo Game Boy), and games played on the computer.

[6]The presence of games played on personal computers might signal something different about whether a personal computer is available for academic use than the presence of games played on video consoles. However, it is not clear whether the availability would be higher or lower if the roommate brings a computer game than if the roommate does not since the presence of a computer game makes it more likely that a computer will be present but also more likely that an available computer will be tied up with non-academic uses. Regardless, our knowledge of the institutional details at Berea suggests that this issue is not likely to be very important since, in 2001, high quality computer labs were available in all of the dorms at Berea. Nonetheless, in all of the specifications in the paper we include the variable COMPUTER-IN-ROOM from the second panel of Table 1 which indicates whether either a student or his roommate brings a computer to school, and, as discussed in the next paragraph, we also have access to a variable which elicits how much time a person spends per week using a computer for academic purposes.

weekdays. Response rates were relatively high on these surveys; the median person in our sample described below answered all four surveys and the average number of responses was 3.11. Our measure of how much each person studies, $STUDY_i$, is created by averaging the number of study hours a student reported over all of the time-use diaries that he completed. The first row of the fourth panel of Table 1 shows that, on average, students in our sample report studying 3.427 hours per day. The time diaries can also be used to compute information about how many hours students sleep, the number of hours students spend in class, the time at which students go to sleep, and the number of hours students spend partying. Other questions in the BPS are used to provide independent information about the proportion of classes attended (question B Appendix A), the hours per week students spend using a computer for academic purposes, the percentage of study time that takes place in the dorm room (Question C Appendix A), and the percentage of study time that takes place in the dorm room with the TV on (question C Appendix A). The information from these questions is discussed in detail later.

Section 3. Is there evidence that grades are not entirely predetermined? Differences in grade performance by RGAME and other roommate variables (instruments)

In the population, the random assignment of roommates implies that students with $RGAME_i$ equal to one have the same characteristics, with the exception of sex, at the time of entrance as students with $RGAME_i$ equal to zero. Thus, conditional on sex, any differences in grade performance between the two RGAME groups in the population can be attributed to differences in inputs that are influenced after the time of college entrance.

While it would perhaps be desirable to perform all analyses in the paper separately by sex, as a concession to our small sample size we typically pool male and female observations and condition on a sex in the specifications throughout the paper. In addition, our small sample size suggests that it may be desirable in our specifications to condition on the other observable characteristics in X_i (panel 2 of Table 1) because, although the random assignment of roommates implies that no differences in X_i (other than $MALE_i$) will exist between RGAME groups in the population, differences may exist in X_i between RGAME groups in the sample because of sampling variation. We discuss this issue in detail in Section 5 in the context of our IV estimator.

In the first column of Table 2 we pool male and female observations and regress GPA_i on both $RGAME_i$ and X_i. The estimated effect of $RGAME_i$ indicates that having a roommate who brings a videogame reduces first semester grade point average by .241 of a grade point, and the null hypothesis that $RGAME_i$ has no effect on GPA_i is rejected at significance levels greater than .008. Thus, this primary result indicates that inputs that can be influenced after the time of college entrance play an important role in the grade production function. That is, important for a variety of policy discussions, grade performance is far from predetermined at the time of college entrance.

It is worth examining whether somewhat different specifications of the video game information from Question A (appendix A) produce similar results. In columns 2-4 of Table 2 we disaggregate the $RGAME_i$ variable. In the second and third columns of Table 2 we reestimate the specification in column 1 of Table 2 after replacing $RGAME_i$ with the variables $RCONSOLE_i$ and $RCOMPUTER_i$, respectively. In the fourth column we replace $RGAME_i$ with both $RCONSOLE_i$ and $RCOMPUTER_i$. The results in columns 2-4 are similar in spirit to those in column 1. For example, in the second column we find that the effect of $RCONSOLE_i$ is statistically significant at levels greater than .005 and that the estimated effect of .300 represents nearly half of a standard deviation in GPA_i . In the third column we find a somewhat smaller estimated effect for $RCOMPUTER_i$, but that it is still statistically significant at .10.

It also seems worthwhile to examine whether the effect of having a roommate who brings a videogame depends on whether or not the student himself brought a game. However, from a theoretical standpoint it is not clear whether we should expect a larger effect of having $RGAME_i$ equal to one for students who bring videogames themselves (which we denote $OGAME_i$=1) or for students who do not bring videogames themselves (which we denote $OGAME_i$=0). Suggesting that the former group might see a larger effect would be the notion that game playing might be highest when both students in a room are interested or experienced in playing games. This notion is consistent with studies examining peer effects and alcohol use which find that the effect of being assigned a roommate who was a drinker in high school depends critically on whether the student himself was a drinker in high school (Duncan et al. 2005; Kremer and Levy, 2003). Suggesting that the latter group might see a larger effect would be the notion that having a roommate who brings a videogame would only lead to a large change in the availability of games in the room if the student does not own one himself.

The third column of Table 3 provides the descriptive statistics that are relevant for examining whether important interaction effects might be present in our context, although for reasons of sample size we have not stratified the sample by sex. Comparing the fourth entry of column 3 to the second entry of column 3 reveals that, for a student in the sample with $OGAME_i$ =1, average grades are .285 lower (2.754 versus 3.039) if $RGAME_i$ is equal to one than if $RGAME_i$ is equal to zero. Comparing the third entry of column 3 to the first entry of column 3 reveals that, for a student in the sample with $OGAME_i$ equal to one, average grades are .196 lower (2.932 versus 3.128) if $RGAME_i$ is equal to one than if $RGAME_i$ is equal to zero. These findings are formalized further in column 5 of Table 2 where we reestimate the specification in column 1 of Table 2 after adding the variable $OGAME_i$ and the interaction term $OGAME_i$ x $RGAME_i$. The impact of having a roommate who brings a videogame is -.200-.080=-.280 if the student himself brings a videogame, and this effect is significant at levels greater than .03. The impact of having a roommate who brings a game is .08 smaller (-.20 versus -.28) if the student does not bring a game himself, but this difference is not statistically significant at traditional levels.

Thus, the interaction effect between whether a roommate brings a videogame and whether a student himself brings a videogame does not appear to be as important as was found in the alcohol studies, and the $RGAME_i$ variable does a reasonable job of summarizing the information in Question A (Appendix A) that is useful for identifying the direct effect of video games on GPA_i. As a result, for ease of exposition, in Section 4 where we examine what inputs are responsible for differences in grade performance, we focus the discussion exclusively on differences in grade performance generated by the binary roommate variable $RGAME_i$. We return in Section 5 to a discussion of results related to specifications involving $RCONSOLE_i$, $RCOMPUTER_i$, and the interaction $OGAME_i$ x $RGAME_i$. At that point we also explore the potential value of the roommate variables $RSTUDYHS_i$ and $REXSTUDY_i$ from the third panel of Table 1.

Section 4. What inputs are responsible for differences in grades between RGAME groups?

The difference in GPA_i between RGAME groups implies that the presence of the roommate videogame affects grade inputs that are determined, at least partially, after the time of college entrance. These types of inputs can be grouped into three broad categories: study quantity, class attendance, and study efficiency. The first two categories are self-explanatory. Study efficiency includes everything about how productive a unit of study time (e.g., an hour) is including, for example, whether a student is rested/alert when studying, the quality of the physical location where a student studies, and the quality of the academic input that the student receives from faculty or peers. While not explicit in the category title, we also think of this category as capturing how productive a unit of time is in class.[7]

In the next three subsections we examine whether our data provide evidence of differences in these three categories by RGAME group. We note that characterizing differences in study quantity and class attendance is a rather straightforward accounting exercise given the time diaries and other questions that we included in the BPS. It is perhaps more difficult to be certain that we are able to fully characterize possible differences in study efficiency, even with the complete flexibility we had in designing surveys specifically for this purpose.

In thinking about what we might find in the next three subsections, it is worth noting that the presence of a roommate videogame could affect a student's grades either because the physical game itself influences the student's inputs or because roommates who bring games are systematically different in ways that influence the student's inputs. There is some independent evidence that the former possibility is important. At the end of the first semester, we asked each student how much time he spent playing video games in an average week during the semester. On average, students who have $RGAME_i$ equal to one reported playing 4.06 hours a

[7]Course difficulty may come to mind as a possible factor that does not fit into these three categories. However, it seems somewhat unlikely that course difficulty would be different between RGAME groups. Because individuals preregister for courses before college entrance, the random roommate assignment ensures that course difficulty is the same between groups at the time of entrance. In practice, many first year courses are mandatory and it seems that, by the time the video game has had time to have a substantial effect, it would be too late to drop courses and add other courses. Dropping courses without adding other courses could be an option, but below we find no evidence that there exist differences in the number of courses between students in the RGAME groups. Perhaps most importantly, if the reasoning above is incorrect and individuals with $RGAME_i$=1 are somehow taking easier courses, taking this into account in Section 5 would strengthen our results further.

week and students who have $RGAME_i$ equal to zero reported playing only .79 hours per week. A test of the null hypothesis that there is no difference in game playing between students who have $RGAME_i$ equal to one and students who have $RGAME_i$ equal to zero yields a t−statistic of 3.54, and, as a result, the null is rejected at all traditional significance levels. With respect to the latter possibility, at least in terms of observable characteristics, we find no evidence that students who bring video games are much different than those who do not bring videogames. For example, in our sample, we find that the average ACT score differs by only .06. and this effect is not close to being rejected at traditional significance levels.[8]

Section 4.A. Class Attendance

With respect to class attendance, our knowledge of institutional details at Berea suggests that there would be little effect of $RGAME_i$ at Berea. Unlike many other schools, class attendance is to a large degree mandatory at Berea with many faculty members imposing strict attendance policies and faculty typically either formally or informally keeping track of the attendance of individual students. Thus, we expected a priori that attendance would be very high for all students, and we find strong empirical support for this belief. At four times during the first semester, we used Question B in Appendix A to elicit information about the number of times in the previous seven days that a student's classes were scheduled to meet and the number of these classes that the student attended. For each student we compute the proportion of classes that he attended across all time-use surveys that he completed. In column 1 of Table 4a we regress this proportion, PATTEND, on $RGAME_i$ and $MALE_i$.[9] The estimated effect (std. error) of $RGAME_i$ is -.014 (.009). Thus,

[8] One would expect that ownership of consumption goods like videogames would typically vary with a student's family income across the entire population of students that attend college. However, the students at Berea come from quite homogenous backgrounds because admission is subject to an income maximum; 90% of the students in our sample have family income less than $45,000 and the average family income is approximately $27,000. This likely explains why we find that whether a roommate brings a videogame is unrelated to the family income of that roommate; the average family income differs by only approximately $1000 between students who bring videogames and students who do not bring videogames, and this effect is not close to being statistically significant at traditional levels. Given this finding and the fact that family income is missing in our administrative data for a few of the students in our sample, we do not use family income in the remainder of the paper.

[9] Note that all substantive conclusions in Table 4 remain the same if we also condition on other observable characteristics in X_i.

the estimated effect is not significant at .10 and is quantitatively very small; students in the sample with $RGAME_i$ equal to one have attendance rates that are lower by only 1.4 percentage points or just slightly more than 1.4 percent lower given an overall average attendance rate of approximately .96.[10] We can also provide information about whether the presence of a roommate videogame affects class attendance by using information from our time diaries. For each student we construct a $CLASSHOURS_i$ variable in a manner that is analogous to how the $STUDY_i$ variable is calculated - by averaging the number of daily hours a student reported being in class over all of the time-use diaries that he completed. The results of the regression of $CLASSHOURS_i$ on $RGAME_i$ and $MALE_i$ in column 2 of Table 4a indicates that students spend approximately three and one-half hours per day in class and that the estimated effect of $RGAME_i$ on class attendance is quantitatively small and statistically insignificant.[11]

Thus, we find no evidence that class attendance varies between RGAME groups. Then, even without any additional information we can conclude that studying - defined generally to include both study quantity and study efficiency - plays a very important causal role in determining grade performance. In the next two subsections we attempt to refine this conclusion further by examining whether it is study quantity or study efficiency (or both) that causes the grade differences between the RGAME groups.

Section 4.B. Study quantity

The descriptive statistics in the first row of the fourth panel of Table 1 show that, for both males and females in the sample, study quantity differs in a quantitatively important manner between students whose

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[10]We also find no difference in class attendance between students who bring videogames themselves and those who do not bring videogames themselves; when we reestimate column 1 of Table 4a after replacing $RGAME_i$ with $OGAME_i$ (whether a person brought a videogame himself/herself), we find an estimate (standard error) of -.012 (.009).

[11]For reasons discussed in an earlier footnote, it seems reasonable to assume that students with $RGAME_i = 1$ have similar numbers of classes as students with $RGAME_i = 0$ and this assumption is supported by evidence from the first part of Question B in Appendix A. On average, students who have $RGAME_i = 1$ report that their classes were scheduled to meet 14.40 hours in the previous seven days. On average, students who have $RGAME_i = 0$ report that their classes were scheduled to meet 14.10 hours in the previous seven days. A test that the number of scheduled classes is the same in the population for students with $RGAME_i = 1$ and students with $RGAME_i = 0$ cannot be rejected at significance levels less than .44.

roommates brought videogames and students whose roommates did not bring videogames. Specifically, the sample average of $STUDY_i$ is .667 lower (2.924 vs. 3.591) for males who have $RGAME_i$ equal to one than it is for males who have $RGAME_i$ equal to zero and the sample average of $STUDY_i$ is .467 lower (3.226 vs. 3.693) for females who have $RGAME_i$ equal to one than it is for females who have $RGAME_i$ equal to zero. The null hypothesis that the effect of $RGAME_i$ is the same for males as it is for females cannot be rejected at traditional levels.

Pooling the male and female observations and, for the same reasons discussed in Section 3, again conditioning on X_i, we estimate a regression of the form

(1) $STUDY_i = \beta_0 RGAME_i + \beta_1 X_i + \nu_i$

and show the results in the first column of Table 5a. With respect to the effect of X_i, our results are consistent with Stinebrickner and Stinebrickner (2004) who found that, while some students study very different amounts than other students, the majority of these differences cannot be explained by traditionally observable characteristics.[12] With respect to our effect of primary interest, we find an estimate and std. error of −.668 and .252, respectively, for β_0 so that a test of the null hypothesis that $RGAME_i$ has no effect on study-effort is rejected at all levels of significance greater than .01. Given that students in the sample study 3.43 hours per day on average, the estimated reduction of approximately two-thirds of an hour per day is quantitatively very substantial.

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[12] From a theoretical standpoint, the insignificant effect of ACT scores could arise because of offsetting forces. On one hand, to the extent that higher ACT scores arise because of higher study effort before college and motivation to study is a somewhat permanent trait, we might expect ACT_i to be positively correlated with $STUDY_i$. On the other hand, to the extent that higher ACT scores reflect more ability, we might expect students with higher ACT scores to study less since, for these students, additional studying would likely have less of an effect on the probability of failing out of school and might also have less of an effect on expected grade outcomes due to the grade ceiling of A in each class.

All of the included majors have similar effects on study effort, a finding that is not particularly surprising given that students of all expected majors tend to take a very similar set of core General Studies classes in the first year. The estimates for the included majors are quantitatively large. There are reasons that the included majors may have different study effort than the omitted major category (physical education). However, given the very small number of observations in the omitted category, these differences could be caused by the substantial amount of sampling variation that is present in the estimators, and only two of the seven included majors are significant at 5%. Regardless, removing the major variables has little effect on any results in the paper.

Section 4.C. Study efficiency

Differences in rest/alertness across RGAME groups

The efficiency of a student's study effort depends, in part, on whether the student is rested/alert when studying (or in class). Perhaps the most obvious contributor to whether a student is rested is the amount of time that he sleeps. We did not have a strong prior about whether this would vary by RGAME group. Using our time diaries we construct a variable $SLEEP_i$, which measures the number of hours that a students sleeps per night, in a way that is directly analogous to the way that the variable $STUDY_i$ is constructed. The third column of Table 4a shows the results from a regression of $SLEEP_i$ on $RGAME_i$ and $MALE_i$. The estimated effect (std. error) of $RGAME_i$ is .275 (.208). Thus, the effect is not statistically significant and the estimate indicates that students in the sample whose roomates brought videogames slept approximately fifteen minutes more per night than students in the sample whose roommates did not bring videogames. We also use our time-diaries to construct a variable $BEDTIME_i$ that indicates the time at which a student goes to bed. This variable is constructed such that positive values indicate the number of hours after midnight and negative values indicate the number of hours before midnight. Column 4 of Table 4a shows a regression of $BEDTIME_i$ on $RGAME_i$ and $MALE_i$. We find that, on average, students go to bed between 12:45 and 1:00 and that there is no evidence that the presence of the roommate videogame influences a student's bedtime.

Alcohol use may represent another reason that students are not rested/alert when studying or in class. It is well-known that the prevalence of drinking is very low at Berea relative to other schools. Contributing to this reality is the fact that Berea is a Christian (non-denominational) school and many students come from religious backgrounds in which drinking is not accepted. In addition, the immediate area around Berea is a "dry" area in which alcohol sales are prohibited. Nonetheless, it is worth directly examining this issue. This is possible because our time diaries contain a category "partying." Column 4 of Table 4b shows a regression of the number of hours spent partying on $MALE_i$ and $RGAME_i$. On average, students spend only about ten minutes a day partying, and we find no evidence of a relationship between the number of hours spent partying and $RGAME_i$. Approximately 85% of all students do not report any partying on any of the time-use surveys and this percentage also does not vary in a meaningful way with whether a person's roommate brought a video game. While we were certainly not surprised by the low prevalence of weekday drinking, it is at least possible that some students are wary of reporting this information on their time diaries. Nonetheless, our intuition is that, if substantial differences in drinking behavior exist between students with $RGAME_i$ equal to one and students with $RGAME_i$ equal to zero, these differences would reveal themselves in, for example, the variable BEDTIME. In addition, there is no strong reason to believe, a priori, that students who bring video games to school are more likely to drink and there is no evidence in the time diaries that this is the case.[13]

Another possibility is that paid employment may cause students to not be rested/alert for their studies. However, the institutional details of the school imply that there cannot be substantial differences in paid employment by RGAME group. This is the case because the school has a mandatory work-study program in which students are not allowed to hold off-campus jobs, and, during the period covered by our data, all students worked very close to ten hours per week in their first-year on-campus jobs.

Differences in the quality of the physical study location across RGAME groups

The efficiency of a student's study effort may also depend on the quality of the physical location where the student studies and we included questions related to this possibility in the BPS. One concern could be that the presence of a video game in the room implies that the student may not be able to study in the room when he wants to because, for example, the room has become a place where others congregate. We examine this possibility using question C in Appendix A. We find no difference in study locations for those who have $RGAME_i$ equal to one and those who have $RGAME_i$ equal to zero; in column 1 of Table 4b we regress the percentage of study time that takes place in the dorm room on $RGAME_i$ and $MALE_i$ and find that the estimated effect of $RGAME_i$ is not statistically significant.

A related way that studying might be less efficient for students who have $RGAME_i$ equal to one

[13]The proportion of students who report drinking on at least one time-use survey is virtually identical for students who bring videogames themselves, .146, and students who do not bring videogames themselves, .149.

It is also likely that drinking/partying is more prevalent on weekends than on the weekdays when our time diaries were collected. However, at schools where drinking is prominent, a non-trivial amount of drinking tends to also be observed during the week (Wood et. al, 2007). In a related point, it seems much less clear that sleep differences between groups would be expected to be bigger on weekends than during the week, but, of course, we cannot rule out this possibility.

would be if the videogame or the television that may accompany the video game serves as a distraction while the student is studying - perhaps because the roommate is watching television or playing the game. We start our examination of this issue using question C in Appendix A which elicits information about how much time is spent studying with the television on. In Column 2 of Table 4b we find no effect of $RGAME_i$ when we regress the percentage of time spent studying with the television on $RGAME_i$ and $MALE_i$.

It is hard to know for sure whether a person would answer that he was studying with the television on if his/her roommate was playing a videogame on the television. Nonetheless, there is a very natural bound on how much of a student's study time could occur while a videogame is being played by his/her roommate. Using the question described in the second paragraph of Section 4 which asked each student how much time he spent playing video games in an average week during the fall semester, we find that roommates who bring video games spend 36 minutes per day, on average, playing the video game. Thus, even if we make the extreme assumption that a student whose roommate brings a videogame is studying in the room at all of the times that his/her roommate is playing the video game, only approximately 20% of a student's study time would, on average, take place with the video game on.[14] Further, this is likely to be an extremely conservative bound. For example, if the times during the day at which a student studies in the room (1.8 hours per day, on average) are chosen randomly from the available non-sleep hours of the student and the times during the day at which a student's roommate plays the video game (approximately 36 minutes per day, on average) are chosen randomly from the available non-sleep hours of the roommate, then only approximately 2% of a student's overall study time would take place while his/her roommate is playing a video game. This percentage would be understated to some extent if there are some hours during the day when, for example, neither roommate can be in a room because they both have classes. However, it would be overstated to some extent if students tend to be somewhat hesitant to play a distracting video game if their roommate is studying and/or if students are wise enough look for other places to study if a roommate is behaving in a way that would substantially undermine study efficiency. It would also be

[14]Students with $RGAME_i$ equal to one study approximately three hours per day.

overstated if, as suggested by some of the evidence in Section 5, roommates tend to play videogames at the same time and, hence, are more likely to be quiet at the same time. Overall, it seems rather unlikely that students whose roommate bring videogames are suffering substantially because their studying is taking place while their roommates are playing video games or watching television.

Further, the presence of the roommate video game could potentially improve the physical environment of a room in some cases. For example, in theory, since some video games are played on computers, treated students may be more likely to have a computer in their room and this could represent an academic advantage for treated students. However, in column 3 of Table 4b we regress the number of hours per week that a student uses a computer for academic reasons on $RGAME_i$ and $MALE_i$ and find that students in the sample whose roommates bring video games report that they use the computer for academic reasons about one extra hour per week than non-treated students in the sample, but the estimated effect of $RGAME_i$ is not statistically significant. This finding is consistent with our earlier discussion in footnote 6 about the generous computing labs that were available in the dorms for all students.

Differences in inputs from peers across RGAME groups

Finally, the possibility that students whose roommates bring video games are studying less efficiently could also be of relevance if these students have roommates who are less able or less willing to help them directly with their coursework. However, Stinebrickner and Stinebrickner (2006) discuss in depth the avenues through which roommates could transmit peer effects and using unique data on the amount and nature of interactions between roommates conclude that, in the short-run, peer effects are much more likely to be transmitted by good role models influencing the time-use decisions of their roommates than by high ability students helping their roommates understand their coursework.[15] Further, in our data we find no relationship between the RGAME variable and the amount of time a student spends interacting with his roommate on

[15]There are many reasons for this conclusion. One issue is that it may be quite costly for students to help each other given that they may not be taking the same classes with the same faculty members (and are often not close friends). We find empirical evidence that, while roommates often spend considerable amounts of time together, they spend little of this time "studying or discussing course material."

academic matters and, as discussed earlier, we find no evidence that students with $RGAME_i$ equal to one have roommates who would be less able to provide effective academic assistance (i.e., of lower ability) than students with $RGAME_i$ equal to zero.[16] In short, it seems highly unlikely that grade differences between the RGAME groups are being driven in a non-trivial manner by differences in help with coursework from peers.

Section 4.D. Summary

Using real-world data, it would never be possible to rule out with certainty that there are differences in study efficiency between $RGAME_i$ groups. For example, it would be hard to provide direct evidence that even small amounts of videogame playing would not harm a student's short-term thinking skills to some extent. Nonetheless, given that our unique data allow us to rule out a seemingly close-to-exhaustive set of reasons that study efficiency may be different between the two groups and given that we find substantial differences in study quantity between the two groups, our findings seem to suggest rather firmly that study effort, as measured by the quantity that a student studies, plays a central role in determining grade performance. This suggests that simply increasing effort, even without refining study techniques, could make a substantial difference in academic outcomes. In the next section we attempt to quantify how much of a grade payoff there is to an extra hour of studying.

Section 5. Quantifying the causal effect of studying: OLS and IV estimators

Our equation of interest for quantifying the grade effect of an extra hour of studying is

(2) $GPA_i = \alpha_0 STUDY^*_i + \alpha_1 X_i + u_i$,

where GPA_i and X_i are as defined in Table 1, $STUDY^*_i$ is the average number of hours that a person studies per day over all of the days in the first semester, and u_i captures all unobserved determinants of grade performance.

[16]When we estimate a linear regression of a person's ACT score on whether the person brought a video game to school $OGAME_i$ and $MALE_i$, the estimated effect (std. error) on ACT_i is .526 (.534). Thus, holding sex constant, students in the sample who bring video games have average ACT scores that are one-half of a point higher than students who do not bring video games.

Section 5.A. Ordinary Least Squares Estimator

A practical problem that arises in equation (2) is that, because it was not feasible to collect time diaries on every day of the first semester, $STUDY^*_i$ cannot be fully observed. What is observed is $STUDY_i$, the average number of hours that a person studied on the days that he completed time diaries. Replacing $STUDY^*_i$ with $STUDY_i$ in equation (2) and estimating by OLS, we obtain the results in the first column of Table 6. The estimated effect of studying is small with an extra one hour per day increasing first semester GPA_i by only .038. A test of the null hypothesis that studying has no effect on GPA_i cannot be rejected at significance levels less than .13. Thus, our OLS results are similar in spirit to the previous literature that was discussed in the introduction.

Section 5.B. Instrumental Variables Estimators

There are three potential biases present in the OLS estimator. First, $STUDY^*_i$ could be correlated with u_i because students who spend more time studying may be different in permanent, unobserved ways than students who spend less time studying. For example, it might be the case that students who study a lot do not have the same unobserved ability as students who study less. Second, $STUDY^*_i$ could be correlated with u_i because students may react to the semester-specific factors in u_i by changing how much they study. For example, students may change their study effort when they receive bad grade shocks or have more difficult classes during a particular semester. Finally, while our data are unique in the detail they contain about how much students study, an errors-in-variables problem is created when $STUDY^*_i$ is replaced by its observable but noisy proxy $STUDY_i$.

While Instrumental Variable (IV) estimation represents a theoretically appealing way to deal with the two potential endogeneity problems and the errors-in-variables problem, in practice finding a credible instrument in this context is typically a difficult task. What is needed is a variable that has a direct effect on the amount that a person studies (i.e., satisfies a relevance condition), but is uncorrelated with the unobservable determinants of grade performance as captured by u_i (i.e., satisfies an exogeneity condition). We take advantage of unique information in the BPS to construct potential instruments.

RGAME as an instrument

We begin by considering the use of $RGAME_i$ as an instrument. The random assignment of roommates guarantees that $RGAME_i$ is unrelated to all elements of u_i, such as unobserved ability, that are predetermined at the time of college entrance. As discussed in Section 4, this implies that the avenues through which $RGAME_i$ could potentially influence grade performance are study quantity, class attendance, and study efficiency. Then, in this context, the two conditions for IV to be valid require, respectively, that $RGAME_i$ influences study quantity (relevance) but does not affect either class attendance or study efficiency (exogeneity). Section 4.B. establishes the former condition, with equation (1) representing the first stage of a two stage least squares estimation approach. Sections 4.A. and 4.C. suggest that the latter condition is, at the very least, a plausible characterization.

With a binary instrument, we can employ a straightforward Wald estimator which attributes the difference in average GPA_i between the two RGAME groups to the difference in average study quantity between the two groups. As seen in the first row of Table 1, males in the sample who have $RGAME_i$ equal to one have an average GPA_i that is .239 lower than males who have $RGAME_i$ equal to zero. As seen in Table 1 and discussed earlier, males in the sample who have $RGAME_i$ equal to one have an average value of $STUDY_i$ that is .667 hours less per day than lower than males who have $RGAME_i$ equal to zero. Then, the Wald estimate of the effect of studying on GPA_i for males would be .239/.667=.358. Similarly, the Wald estimate of the effect of studying on GPA_i for females would be .128/.467=.274.

Formal IV estimates of equation (2) are shown in column 1 of Table 7a. As noted earlier, our small sample makes it difficult to estimate the model separately for males and females. However, the earlier evidence that it is not possible to reject the null hypothesis that $RGAME_i$ has the same effect on the study quantity of males and females along with the finding in the previous paragraph that the Wald estimates are similar for males and females, suggests that pooling males and females is generally reasonable. In addition, as in the first stage equation (1), we also condition on the other elements of X_i. Given that the X_i are uncorrelated with the instrument, the IV estimator is consistent regardless of whether or not the X_i are included. Then, the motivation for including X_i comes from the possibility that doing so may lead to benefits related to the efficiency or finite sample bias of the estimator.[17]

The IV estimate indicates that an additional hour of studying per day causes first semester grade point average to increase by .360. Thus, the IV estimate is much larger than the OLS estimate in column 1 of Table 6. Although, as expected, the effect is estimated with much less precision under IV than under OLS, a test of the null hypothesis that studying has no effect on grade performance produces a t-statistic of 1.963, and, as a result, the test is rejected at significance levels greater than .051.[18]

To provide additional support that these results are not being driven by differences (between the treated and untreated groups) in behaviors other than study-effort, we also estimated a specification which added as regressors all of the dependent variables in Table 4a and Table 4b. In the interest of space considerations, full results are not shown, but the estimated effect (std. error) in this specification was .377 (.198). We also found that the results changed very little when we added the explanatory variable $OGAME_i$ which indicates whether

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[17]With respect to precision, roughly speaking the theoretical tradeoff from adding X_i is somewhat standard. On one hand, holding the variance of the unobservable u_i in equation (2) constant, adding additional parameters to be identified increases the variance of the estimator of α_0 (Goldberger, 1991), although this effect is largely mitigated here because the X_i are uncorrelated with the instrumented version of the $STUDY_i$ variable. On the other hand, controlling for X_i decreases the variance of u_i (equation 2) which represents an important source of sampling variation in the estimator of α_0.

With respect to finite sample bias, the IV estimator can be written as $\alpha_0 + \sum_i(RGAME_i \cdot u_i)/\sum_i(RGAME_i \cdot STUDY_i)$. Although $RGAME_i$ and u_i are uncorrelated in the population by assumption, the numerator in the ratio will not be zero in any particular sample due to sampling variation. For example, students in a particular RGAME group may, by chance, have higher average unobserved ability than students in the other RGAME group. Intuitively, a bias arises (i.e., the ratio above does not have an expectation of zero) because, if u_i is positively (negatively) correlated with $STUDY_i$, the sample group with higher average unobserved ability will also tend to have systematically higher (lower) average $STUDY_i$. As a result, the direction of the finite sample bias from IV is the same as the direction of the bias from OLS. Controlling for X_i may decrease the finite sample bias by reducing the sampling variation in u_i, and, therefore, in $\sum_i(RGAME_i \cdot u_i)$. However, conditioning on X_i may also have an effect on the covariance between u_i and $STUDY_i$, and, in theory, this covariance could either increase or decrease when we condition on a subset of the population. More generally, the discussion in Wooldridge (2002, page 101) suggests that understanding the effects of changes in model specification on finite sample properties of the IV estimator is extremely difficult, with the IV estimator not even having an expected value in some cases (Kinal, 1980).

[18]For a subset of 173 observations we observe a roommate's own report of whether he brought a video game. Constructing the instrument using the roommate's own report, our estimate for this subset is slightly higher, .402, although, in part because of the smaller sample size, the estimator is less precise and the t-statistic is somewhat lower, 1.8.

the student himself brought a video game.[19] While random assignment implies that both specifications with and without the "own" analogs to the instruments are valid on theoretical grounds, we choose to present full results from the specifications without the own values simply because the effect of interest is more precisely estimated in these specifications (although the point estimates are larger when own values are included).

RCONSOLE and RCOMPUTER as instruments

Here we examine whether the results are robust to a disaggregation of the $RGAME_i$ variable. With respect to the first stage, Columns 2-4 of Table 5A show the results from the first stage (equation 1) when $RGAME_i$ is replaced with $RCONSOLE_i$, RCOMPUTER, and both $RCONSOLE_i$ and $RCOMPUTER_i$, respectively. Consistent with our expectations discussed in Section 2, the estimated effects of $RCONSOLE_i$ and $RCOMPUTER_i$ are quantitatively large in these specifications and generally similar in size both to each other and to the effect for $RGAME_i$ in column 1 of Table 5A. With respect to the exogeneity condition, analyses that parallel those for the $RGAME_i$ variable in Section 4.A. and 4.C. lead to the same conclusion - there is no evidence of differences in class attendance or study efficiency by $RCONSOLE_i$ or $RCOMPUTER_i$ status. We choose not to show all of these results simply because of their repetitive nature.

The IV results associated with these three disaggregated specifications are shown in columns 2-4 of Table 7a. The results are quite consistent with those of the $RGAME_i$ specification in column 1of Table 7a. For example, in column 4, which uses both $RCONSOLE_i$ and $RCOMPUTER_i$ as instruments, the estimated effect (std. error) of an additional hour of studying is .415 (.209). In both columns 2 and 3, in which $RCONSOLE_i$ and $RCOMPUTER_i$ are used separately as instruments, we find estimates of greater than .30 although, as expected, the estimators are less precise than when either $RGAME_i$ is used by itself (column 1) or when both $RCONSOLE_i$ and $RCOMPUTER_i$ are used (column 4).

[19] In this case, the estimate (std. error) is .363 (.195). In the first stage analog to column 1of Table 5a, we find that students who bring video games study .418 less hours per day than students who do not and that this effect is significant at .10. We note that it is not clear on theoretical grounds whether the own effect should be larger or smaller than the effect of the roommate bringing a video game. Students who bring video games may be students who have found they are most able to handle the temptation the games may represent. Perhaps more importantly, video games may be not be dissimilar from other toys in the sense that usage might be particularly intense in the period after first exposure and might decline after that as the novelty wears off.

Adding interactions: OGAME x RGAME as an instrument

The second column of Table 3 provides the descriptive statistics that are relevant for examining whether important interaction effects are present in studying. Comparing the third entry of column 2 to the first entry of column 2 reveals that, for students in the sample who do not bring a game themselves, average study time is .340 hours per day lower (3.420 versus 3.760) if their roommates brought a game than if their roommates did not bring a game. Comparing the fourth entry of column 2 to the second entry of column 2 reveals that, for students in the sample who do bring a game themselves, average study time is .809 lower (2.649 versus 3.458) if their roommates brought a game than if their roommates did not bring a game. These results are formalized in column 5 of Table 5a which shows the results of the first stage equation (1) after adding the variable $OGAME_i$ and the interaction term $OGAME_i$ x $RGAME_i$. The impact of having $RGAME_i$ equal to one is $-.353-.619=-.972$ hours per day if $OGAME_i$ is equal to one and this effect is significant at levels greater than .007. The impact of of having $RGAME_i$ equal to one is .619 lower (.298 versus .917) if $OGAME_i$ is equal to zero, although this difference is not statistically significant at traditional levels. With respect to the exogeneity condition, as in the previous section, analyses that parallel those for the $RGAME_i$ variable in Section 4.A. and 4.C. lead to the same conclusion – holding constant the value of $OGAME_i$ there is no evidence of differences in class attendance or study efficiency by $OGAME_i$ x $RGAME_i$ status. Again we choose not to show all of these results simply because of their repetitive nature.

The IV results associated with this specification in which $OGAME_i$ x $RGAME_i$ is included as an additional instrument are seen in column 5 of Table 7a. Including the interaction term increases the precision of the estimator slightly over the specification in which only $RGAME_i$ is used (column 1); the standard error decreases from .183 to .163. The point estimate also decreases slightly from .360 to .321. A test of the null that studying has no effect on grade performance is rejected at a significance level of .05. Thus, conclusions from this specification are quite similar to conclusions from the specification that uses only $RGAME_i$.

Non-game instruments

In this section we examine whether we can increase the precision of our estimator by taking advantage of information about two other potential instruments from the baseline BPS survey that are shown in panel 3 of Table 1 - how much a student's roommate reported studying in high school ($RSTUDYHS_i$) and how much a student's roommate reported that he expects to study in college ($REXSTUDY_i$). This information is available for the 176 individuals in our initial sample whose roommates also chose to participate in the BPS and provided legitimate information about these variables. Stinebrickner and Stinebrickner (2006) provide motivation for these instruments by finding that peer effects between first semester roommates are most likely to arise when students influence each other's time-use.

We focus on the effects of adding these new instruments to the specification involving the instrument $RGAME_i$ and the specification involving both $RGAME_i$ and $OGAME_i$ x $RGAME_i$. In the first stage regressions in column 1 and column 2 of Table 5b we find direct evidence that a student's time-use can be influenced by these new variables. For example, $RSTUDYHS_i$ is statistically significant at levels greater than .032 in each of the two specifications.

From an exogeneity standpoint, both the appeal and possible concerns about these instruments are essentially identical to those discussed earlier for the videogame instrument. With respect to the former, the random assignment feature combined with the fact that the instruments characterize aspects of study-effort of the roommate at the time of college entrance imply that students with different values of $RSTUDYHS_i$ and $REXSTUDY_i$ are identical in the population in all respects at the time of college entrance. With respect to the latter, the instruments would be problematic if, in addition to influencing a student's study quantity, $RSTUDYHS_i$ and $REXSTUDY_i$ also influence other behavior that is related to grade performance. As in Section 4, we treat this latter concern as an open empirical question that we are able to examine using the unique features of the BPS. As described in detail in Appendix B, we find no evidence that $RSTUDYHS_i$ and $REXSTUDY_i$ have an effect on these other behaviors.

Thus, as with the videogame instruments, it seems plausible to believe that $RSTUDYHS_i$ and $REXSTUDY_i$ are valid instruments. In column 1 of Table 7b we find that adding these additional instruments to the specification in column 1 of Table 7a leads to a substantial increase in precision of the IV estimator; the standard error decreases from .183 to .121. The point estimate decreases somewhat from .360 to .291 and we now reject the null hypothesis that studying has no effect on grade performance at all significance levels greater than .017. In column 2 of Table 7b we find that adding these instruments to the specification in column 5 of Table 7a also leads to a substantial increase in precision; the standard error decreases from .163 to .118. The point estimate again decreases somewhat from .321 to .295, and we again reject the null hypothesis that studying has no effect at all significance levels greater than .017. Thus, these results strengthen the conclusion that study quantity plays an important role in the grade production function.[20]

Section 6. Understanding the difference between the IV and OLS estimates

In this section we attempt to understand why the IV estimates in Table 7 are much larger than the OLS estimate, .038, from the first column of Table 6. We focus on the IV estimate of .360 from the first column of Table 7 which is obtained using the $RGAME_i$ instrument.

As discussed in the first paragraph of Section 5.B., part of the .322 difference between these estimates arises because of the errors-in-variables problem that is present from using the observed proxy STUDY in place of STUDY* in equation (2). As discussed in Stinebrickner and Stinebrickner (2004), in a textbook example, the OLS estimator would need to be multiplied by a factor of

$$(3)\quad \frac{\mathrm{Var}(STUDY_i)}{\mathrm{Var}(STUDY_i)-\frac{\sigma_v^2}{N}}.$$

to correct for this problem, where σ_v^2 is the variance of the unobservable in equation (1) and N is the number of time-use surveys that are used to compute $STUDY_i$. It is difficult in our case to know exactly what the bias

[20]As before, we found that the results changed very little when we added explanatory variables which indicate whether the student himself brought a video game, how much the student himself studied in high school, and how much the student expected (at the time of entrance) to study in college. For example, in results not shown in tables, the estimate (std. error) from the first column of Table 7b becomes .342 (.161). In the first stage of this specification, we find an own effect (std. deviation) of how much a student studied in high school (the own analog to $RSTUDYHS_i$) of .029 (.013). The own effect of how much a student expects to study in college is insignificant at traditional levels when included with the high school effort level.

factor is since N is not constant across students. However, using equation (3) we ascertain that the bias factor is roughly between 1.40 and 1.94.[21] Thus, the difference between the IV and OLS estimates that remains after accounting for the errors-in-variables problem is roughly between .286 and .307.

The direction of the bias due to the two potential endogeneity problems discussed in Section 5.B. is uncertain from a theoretical standpoint. However, the fact that the IV estimate is much larger than the OLS estimate suggests that there exists a negative correlation between $STUDY^*_i$ and u_i. One possibility for this discussed in Section 5.B. is that students who study more tend to be of lower permanent, unobserved ability (or differ in other permanent, unobserved ways) than other students. However, while it is impossible to provide conclusive evidence about this possibility, one gets a sense that this might not be the driving influence from examining the results in the first column of Table 5.A. which reveal no evidence of a relationship between our observable measure of ability (ACT) and study quantity.

This suggests that the difference between the IV and OLS estimates might arise because of the remaining potential endogeneity reason discussed in Section 5.B. – that students tend to increase their effort in semesters when the semester-specific elements of grades are low (i.e., that $STUDY^*_i$ is negatively correlated with the semester specific elements of u_i). The presence in our data of a second semester of grade and study quantity information presents us with an opportunity to examine whether there is evidence of this.

In order to differentiate between the first (t=1) and second (t=2) semesters, we index variables with a time subscript when necessary. We first disaggregate the unobservable in equation (2) into a person-specific, permanent component μ_i and a semester-specific component ε_{ti};

(4) $u_{ti} = \mu_i + \varepsilon_{ti}$.

Then the question of interest is whether $STUDY^*_{1i}$ is negatively correlated with ε_{1i}. When equation (2) is estimated by OLS, identification essentially involves comparing the average GPA_i of students who study an extra hour to the average GPA_i of students who do not study an extra hour, with the implicit assumption being that,

[21] An estimate of σ_v^2 can be constructed by differencing the individual daily study reports for a particular person. Estimates of VAR(STUDY) can be computed conditional on N from the sample. 1.40 is an estimate of the factor by which the OLS estimator would be biased if all students answered four time-use surveys. 1.94 is an estimate of the factor by which the OLS estimator would be biased if all students answered only one time-use survey.

conditional on X_i, all students would receive identical grades if they studied the same amount. Then, roughly speaking, in order to explain the entire difference between the OLS and IV estimates through the channel that students increase their effort in semesters when the semester-specific elements of grades are low, we should find that a one unit increase in study effort is associated with a decrease of between .286 and .307 in the average value of ε_{1i}.[22]

For the time being we think of ε_{ti} as being serially uncorrelated. This would make sense, for example, if ε_{ti} is primarily capturing random "luck" factors such as a student's match quality with his professors or whether the student gets sick at an inopportune time during the semester. The grade equation for semesters one and two, respectively, are given by

(5) $GPA_{1i} = \alpha_0 STUDY_1{}^*{}_i + \alpha_1 X_i + \mu_i + \varepsilon_{1i}$

(6) $GPA_{2i} = \alpha_0 STUDY_2{}^*{}_i + \alpha_1 X_i + \mu_i + \varepsilon_{2i}$.

Differencing equation (6) from equation (5) and rearranging yields

(7) $GPA_{1i} - GPA_{2i} - \alpha_0(STUDY_1{}^*{}_i - STUDY_2{}^*{}_i) = \varepsilon_{1i} - \varepsilon_{2i}$.

Note that the left hand side of equation (7) is observable in our data up to the observable proxies for $STUDY_1{}^*{}_i$ and $STUDY_2{}^*{}_i$ and an estimate of α_0.

To illustrate how we identify whether a one hour increase in $STUDY_1{}^*{}_i$ is associated with a substantial decrease in the average value of ε_{1i}, consider a case where there are only two levels of study quantity in the first semester ($STUDY_1{}^*{}_i=3.0$ or $STUDY_1{}^*{}_i=4.0$), $STUDY_1{}^*{}_i$ and $STUDY_2{}^*{}_i$ are observed for individuals in the sample, and α_0 is known. Averaging the left hand side of equation (7) over all individuals who have $STUDY_1{}^*{}_i=4.0$ yields an estimate of $E(\varepsilon_{1i}|STUDY_1{}^*{}_i=4.0)$ because the assumption that ε_{ti} is uncorrelated implies that $E(\varepsilon_{2i}|STUDY_1{}^*{}_i=4.0)=0$.[23] Similarly, averaging the left hand side of equation (7) over all individuals in our sample who have $STUDY_1{}^*{}_i=3.0$ yields $E(\varepsilon_{1i}|STUDY_1{}^*{}_i=3.0)$ because the assumption that ε_{ti} is uncorrelated

[22]Very roughly speaking, if the entire difference between the IV and OLS estimates could be explained through this avenue, then running OLS after subtracting between .286 and .307 from GPA_i for each additional hour of study quantity should yield an estimated effect of study quantity that is similar to the IV.

[23]That is, students who study a lot in the first semester may do so in response to bad "luck" shocks in the first semester, but, under the uncorrelated assumption, these students should, on average, have neither good luck or bad luck in the second semester.

over time implies that $E(\varepsilon_{2i} | STUDY_1{*}_i=3.0)=0$. Then, the difference between these estimates would represent an estimate of how much lower ε_{1i} is, on average, for individuals who study the extra hour.

For our situation where study quantity is a continuous variable, we examine how $E(\varepsilon_{1i}|STUDY_1{*}_i)$ changes with $STUDY_1{*}_i$ by noting once again that $E(\varepsilon_{1i}|STUDY_1{*}_i) = E(GPA_{1i}-GPA_2-\alpha_0(STUDY_1{*}_i-STUDY_2{*}_i)| STUDY_1{*}_i)$ and estimating an OLS regression of the form

(8) $GPA_{1i} - GPA_{2i} - .360\ (STUDY_{1i} - STUDY_{2i})=\text{constant} + \delta\ STUDY_{1i} + \eta_i$.

In order to make the left hand side observable, we have replaced $STUDY_1{*}_i$ and $STUDY_2{*}_i$ with their observable proxies and have taken advantage of our estimate of α_0.

Then, as desired, δ represents the increase in $E(\varepsilon_{1i})$ associated with a one hour increase in $STUDY_{1i}$.[24] We find an estimate (std. error) for δ of -.276 (.040). As described above, this is consistent with the notion that the difference between the OLS and IV estimates is generated primarily by a situation where students tend to increase their effort in semesters when the semester-specific elements of grades are low.

Of course, in reality, it is not the case that ε_{ti} should be interpreted literally as random "luck." For example, while students at Berea have rather limited flexibility about the classes they take during the first year due to a large number of required "general studies" courses, to some extent ε_{ti} will reflect difficulty in classes that is under control of students. To the extent that this is the case, the assumption that the transitory component of grades is uncorrelated across semesters may lose some of its attractiveness. Nonetheless, at the very least, this exercise sounds a cautionary alarm about the use of fixed effects estimators in substantive contexts where individuals may respond to period-specific information.[25] In this application, a fixed effects estimator would achieve identification using the within person variation in study quantity across the two semesters. From a

[24]What we would want, in reality, is the effect of a one hour increase in $STUDY_1{*}_i$. However, if the measurement error that arises from using $STUDY_{1i}$ is classical, then one would expect an attenuation bias for our estimator of δ. This would tend to strengthen our conclusion below.

[25]Given that very little work examines the effect of study effort on academic performance, it is not surprising that, to the best of our knowledge, there does not exist work that uses fixed effects in this specific context. However, the use of fixed effects is fairly common in related educational contexts where identification issues may be similar to those in this paper, including the study of the effect of paid employment on academic performance and other outcomes (Oettinger, 1999; Turner, 1996; Steinberg et al., 1982; Steinberg et al. 1993) and the study of the effect of extracurricular involvement on academic achievement (Lipscomb, 2007).

theoretical standpoint, it seems unappealing to assume that this variation is exogenous and the previous analysis suggests that this assumption is likely problematic. Indeed, there is no reason to think that the across student variation in study quantity, which is discarded by the fixed effects estimator, is more problematic than the within student variation.[26] As a result, not only is the use of a Fixed Effects estimator unlikely to satisfactorily deal with the endogeneity problems, but the Fixed Effects estimator may perform worse than the OLS estimator. Striking evidence that this is the case is shown in column 2 of Table 6. The estimated effect of studying, -.043, is negative, and a test of the null hypothesis that studying has harmful effect on grades cannot be rejected at levels of significance greater than .10.

Section 7. Conclusion

Many policy decisions depend on the extent to which college outcomes of interest are driven by decisions that take place after students arrive at college rather than by background factors that influence students before they arrive at college.[27] Thus, it is important that both the reduced form estimates in Section 3 and the IV estimates in Section 5 suggest that human capital accumulation may be far from predetermined at the time of college entrance. For example, being assigned a roommate with a video game is estimated to have the same effect on first semester grade point average as a 3.88 point decrease in ACT scores (an increase of 1.04 of a standard deviation in our sample and .82 standard deviations among all ACT test takers). Using the IV results in the first column of Table 7.A., an increase in study quantity of one hour per day (an increase of approximately .67 of a standard deviation in our sample) is estimated to have the same effect on first semester grade point average as a 5.21 point increase in ACT scores (an increase of 1.40 standard deviations in our sample and 1.10 standard deviations among all ACT test takers). While it is always difficult to know exactly how the results

[26]It is, of course, highly unlikely that variation in study effort across individuals is exogenous. However, some evidence in support of the notion that the across person variation could be less problematic than the within person variation comes from our evidence that ACT scores are unrelated to study quantity.

[27]Examples include: a) decisions about how to distribute education dollars across student ages; b) decisions about appropriate strategies for counseling students who perform poorly; c) deciding what types of students should be admitted to college (highly motivated or high ability) and its direct importance to merit vs. need based admission decisions.

from a particular school would generalize to larger populations, there is no obvious reason to believe that we should expect substantially different results elsewhere; the curriculum at Berea College is, by and large, similar to that of other liberal arts schools and, as discussed in Stinebrickner and Stinebrickner (2007), students at Berea have observable academic characteristics that are similar to those that attend other schools in the region.

While not the primary focus of this paper, this paper also makes an important contribution to the peer effects literature in general and to the peer effects literature that achieves identification by using college roommates in particular. The goal of the empirical peer effects literature has been to look for empirical evidence which documents that peer effects can matter. This paper provides depth to that literature by not only providing some of the strongest evidence that peer effects can matter, but also by providing perhaps the first direct evidence about an avenue (time-use) through which peer effects operate. This paper also makes a contribution to a substantial literature outside of economics by establishing that video games can have a large, causal effect on academic outcomes.

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Table 1
Descriptive Statistics

	All n=210	**Male All** n=95	**Male RGAME =0** n=45	**Male RGAME =1** n=50	**Female** n=115	**Female RGAME =0** n=88	**Female RGAME =1** n=27
Outcome	**Panel 1**						
GPA - First semester Grade Point Avg	3.004 (.652)	2.853 (.677)	2.979 (.663)	2.740 (.677)	3.129 (.605)	3.159 (.598)	3.031 (.628)
Characteristics at entrance, X	**Panel 2**						
MALE	.452						
ACT	23.380 (3.709)	22.463 (3.842)	22.155 (3.931)	22.740 (3.779)	24.139 (3.431)	24.205 (3.527)	23.925 (3.149)
BLACK	.171	.189	.200	.180	.157	.159	.148
MAJOR1 - Agriculture	.076	.115	.111	.120	.043	.045	.037
MAJOR2- Business	.176	.168	.133	.200	.182	.204	.111
MAJOR3- Elem. Education	.10	.084	.111	.06	.113	.137	.044
MAJOR4- Humanities	.223	.157	.133	.18	.278	.261	.333
MAJOR5- Science & Math	.209	.252	.222	.28	.173	.156	.235
MAJOR6 - Professional	.119	.094	.133	.06	.139	.147	.111
MAJOR7 - Social Sciences	.071	.084	.088.	.08	.060	.056	.074
Omitted Major Physical Educ.	.024	.042	.066	.02	.008	0.0	.037
HEALTH_BAD fair/poor health	.067	.052	.066	.04	.078	.057	.148
HEALTH_EXC excellent health	.371	.40	.333	.46	.347.	.363	dd.29
COMPUTER-IN-ROOM either student or roommate brought a computer	.704	.736			.678		

Table 1
Continued

	All n=210	Male All n=95	Male RGAME =0 n=45	Male RGAME =1 n=50	Female All n=115	Female RGAME =0 n=88	Female RGAME= 1 n=27
Characteristics of roommates	**Panel 3**						
RGAME - Roommate brought either a “videogame” or a “computer game” to school	.367	.526			.235		
RCONSOLE - Roommate brought a “videogame” game to school	.190	.305			.095		
RCOMPUTER - Roommate brought a “computer game” to school	.257	.347			.182		
RSTUDYHS - roommate’s hours of study per week in high school, n=176	10.279 (10.119)	10.115 (12.230)			10.416 (10.078)		
REXSTUDY - roommate’s expected hours of study per day during college, n=176	3.464 (1.826)	3.298 (2.003)			3.602 (1.663)		
College choices and behaviors of students	**Panel 4**						
STUDY	3.427 (1.631)	3.240 (1.688)	3.591 (1.748)	2.924 (1.583)	3.583 (1.573)	3.693 (1.595)	3.226 (1.473)
PATTEND proportion of classes attended	.958 (.060)	.958 (.071)	.968 (.060)	.948 (.078)	.959 (.051)	.960 (.049)	.953 (.056)
CLASSHOURS daily hours in class	3.429 (1.250)	3.444 (1.297)	3.515 (1.310)	3.380 (1.925)	3.417 (1.214)	3.438 (1.103)	3.349 (1.546)
SLEEP daily sleep hours	7.284 (1.394)	7.443 (1.399)	7.203 (1.298)	7.658 (1.463)	7.153 (1.383)	7.137 (1.292)	7.206 (1.673)

BEDTIME time student went to sleep#	.763 (1.287)	.631 (1.289)	.627 (1.307)	.635 (1.287)	.865 (1.282)	.797 (1.284)	1.097 (1.273)
percentage of study time that takes place in dorm room	58.554 (30.229)	55.666 (30.328)	54.818 (33.294)	56.428 (27.725)	60.995 (30.066)	62.459 (30.768)	55.75 (27.359)
percentage of study time that takes place in dorm room with tv on	11.986 (19.021)	10.690 (16.744)	8.181 (14.580)	12.989 (17.818)	13.522 (20.747)	13.912 (20.599)	12.125 (21.662)
weekly hours using computer for academic purposes	7.055 (6.934)	7.078 (7.472)	6.110 (7.565)	7.928 (7.362)	7.035 (6.493)	7.045 (6.434)	7.0 (6.831)
daily hours partying	.120 (.329)	.113 (.320)	.153 (.409)	.077 (.210)	.126 (.337)	.102 (.296)	.204 (.443)

**significant at .05
#dependent variable is created so that it is zero at 12:00 midnight. Positive numbers represent hours after midnight. Negative numbers represent hours before midnight.

Table 2 Reduced form: The direct effect of RGAME and other roommate variables (instruments) on GPA

Independent Variable	estimate (std error)	estimate (std error)	estimate (std error)	estimate (std error)	estimate (std error)
RGAME	-.241 (.089)**				-.200 (.118)*
RCONSOLE		-.300 (.106)**		-.277 (.107)**	
RCOMPUTER			-.167 (.101)*	-.122 (.101)	
OGAME					-.044 (.116)
OGAMExRGAME					-.080 (.170)
MALE	-.079 (.086)	-.074 (.086)	-.125 (.084)	-.065 (.086)	-.063 (.091)
BLACK	-.209 (.120)*	-.171 (.121)*	-.223 (.122)*	-.188 (.121)	-.194 (.121)
ACT	.062 (.012)**	.065 (.012)**	.061 (.013)**	.065 (.012)**	.062 (.013)**
$MAJOR_1$	.906 (.293)**	.959 (.293)**	.879 (.296)**	.949 (.293)**	.881 (.296)**
$MAJOR_2$	.868 (.277)**	.922 (.278)**	.829 (.280)**	.905 (.278)**	.864 (.279)**
$MAJOR_3$	.739 (.287)**	.803 (.286)**	.732 (.291)**	.773 (.287)**	.751 (.288)**
$MAJOR_4$	.889 (.277)**	.931 (.278)**	.847 (.280)**	.919 (.278)**	.878 (.279)**
$MAJOR_5$	.741 (.274)**	.767 (.274)**	.715 (.277)**	.774 (.274)**	.738 (.276)**
$MAJOR_6$	.731 (.285)**	.780 (.285)**	.714 (.288)**	.764 (.285)**	.720 (.286)**
$MAJOR_7$	1.002 (.295)**	1.047 (.296)**	.982 (.299)**	1.044 (.295)**	.995 (.297)**
HEALTH_BAD	.045 (.164)	.051 (.163)	.041 (.166)	.061 (.163)	.070 (.166)
HEALTH_EXC	.149 (.085)*	.129 (.085)	.152 (.087)*	.146 (.086)*	.146 (.086)*
COMPUTER-IN-ROOM	.071 (.089)	.014 (.087)	.066 (.092)	.047 (.091)	.090 (,092)
CONSTANT	.793 (.398)**	.676 (.400)**	.824 (.403)**	.695 (.400)*	.776 (.400)*
	R^2=.289	R^2=.293	R^2=.274	R^2=.294	R^2=.294

*significant at .10
**significant at .05

Table 3 Selective descriptive statistics for sample stratified by whether a student brought a videogame (OGAME) and whether the student's roommate brought a videogame (RGAME)

	proportion of sample	**STUDY mean (std. dev.)**	**GPA mean (std. dev.)**
RGAME=0, OGAME=0	0.42	3.760 (1.474)	3.128 (.590)
RGAME=0, OGAME=1	0.21	3.458 (1.932)	3.039 (.689)
RGAME=1, OGAME=0	0.18	3.42 (1.826)	2.932 (.699)
RGAME=1, OGAME=1	0.19	2.649 (1.100)	2.754 (.639)

The table shows the average value (std. deviation) of STUDY and GPA for the four groups. For example, the second row shows that the group of students who brought a videogame themselves and had a roommate who did not bring a videogame studied 3.458 hours, on average, and had an average GPA of 3.039.

Table 4a
The effect of video game RGAME on other behaviors, n=210

Independent Variable	Dependent Variable PATTEND proportion of classes attended estimate (std. error)	Dependent Variable CLASSHOURS daily hours in class estimate (std. error)	Dependent Variable SLEEP daily sleep hours estimate (std. error)	Dependent Variable BEDTIME time student went to sleep# estimate (std. error)
RGAME	-.014 (.009)	-.114 (.188)	.275 (.208)	.143 (.199)
MALE	.003 (.009)	.059 (.182)	.209 (.202)	-.276 (.192)
CONSTANT	.962 (.006) **	3.444 (.25)**	7.089 (.138)**	.833 (.130)**
	R^2=.012	R^2=.0016	R^2=.019	R^2=.011

*significant at .10
**significant at .05
#dependent variable is created so that it is zero at 12:00 midnight. Positive numbers represent hours after midnight. Negative numbers represent hours before midnight.

Table 4b
The effect of RGAME on additional behaviors, n=210

Independent Variable	Dependent Variable percentage of study time that takes place in dorm room estimate (std. error)	Dependent Variable percentage of study time that takes place in dorm room with tv on estimate (std. error)	Dependent Variable hours per week using computer for academic purposes estimate (std.error)	Dependent Variable daily hours partying estimate (std. error)
RGAME	-2.111 (4.670)	3.515 (2.933)	.963 (1.069)	.007 (.050)
MALE	-4.677 (4.498)	-3.812 (2.825)	-.254 (1.032)	-.015 (.048)
CONSTANT	61.456 (3.058)**	12.756 (1.921)**	6.820 (.699)**	.125 (.033)**
	R^2=.008	R^2=.008	R^2=.012	R^2=0.011

*significant at .10
**significant at .05

Table 5a
First Stage Regressions
The effect of instruments (and other variables) on study hours
using game instruments

Independent Variable	estimate (std error) n=210	estimate (std error) n=210	estimate (std error) n=210	estimate (std error) n=210	estimate (std error) n=210
INSTRUMENTS					
RGAME	-.668 (.252)**				-.353 (.330)
RCONSOLE		-.586 (3.00)**		-.503 (.306)*	
RCOMPUTER			-.537 (.285)*	-.456 (.288)	
OGAME					-.298 (.324)
OGAMExRGAME					-.619 (.475)
RSTUDYHS					
REXSTUDY					
OTHER VARIABLES					
MALE	-.155 (.244)	-.200 (.247)	-.273 (.239)	-.164 (.247)	-.045 (.254)
BLACK	.417 (.341)	.486 (.346)	.361 (.346)	.425 (.346)	.501 (.339)
ACT	-.019 (.036)	-.013 (.036)	-.022 (.036)	-.015 (.036)	-.014 (.036
$MAJOR_1$	1.423 (.828)*	1.510 (.838)*	1.345 (.835)	1.47 (.835)*	1.249 (.827)
$MAJOR_2$	1.421 (.783)*	1.505 (.793)*	1.305 (.790)*	1.44 (.791)*	1.394 (.778)*
$MAJOR_3$	1.120 (.811)	1.273 (.818)	1.085 (.820)	1.159 (.818)	1.210(.803)
$MAJOR_4$	1.637 (.784)**	1.691** (.793)	1.519 (.790)*	1.649 (.791)**	1.565 (.778)**
$MAJOR_5$	1.575 (.776)**	1.590 (.784)**	1.510 (.782)*	1.616 (.781)**	1.555 (.769)**
$MAJOR_6$	1.777 (.806)**	1.872 (.814)**	1.72(.813)**	1.812 (.811)**	1.698 (.799)**
$MAJOR_7$	2.128 (.836)**	2.197 (.845)**	2.072 (.843)**	2.184 (.842)**	2.078 (.828)**
HEALTH_BAD	.209 (.463)	.203 (.467)	.202 (.468)	.239 (.466)	.389 (.464)
HEALTH_EXC	.095 (.241)	.039 (.242)	.113 (.246)	.103 (.245)	.078 (.240)
COMPUTER-IN-ROOM	.212 (.253)	.060 (.249)	.219 (.261)	.185 (.261)	.344 (.258)
CONSTANT	2.403 (1.125)**	2.192 (1.143)*	2.494 (1.135)**	2.261 (1.139)**	2.277 (1.11)**
	R^2=.092	R^2=.080	R^2=.079	R^2=.092	R^2=.125

Note: Uses the entire sample of individuals with randomly assigned roommates.
*significant at .10
**significant at .05

Table 5b
First Stage Regressions
The effect of instruments (and other variables) on study hours
using both game and roommate study instruments

Independent Variable	**estimate (std error) n=176**	**estimate (std error) n=176**
INSTRUMENTS		
RGAME	-.658 (.268)**	-.425 (.350)
RCONSOLE		
RCOMPUTER		
OGAME		-.215 (.343)
OGAMExRGAME		-.539 (.505)
RSTUDYHS	.028 (.013)**	.028 (.013)**
REXSTUDY	.049 (.074)	.040 (.074)
OTHER VARIABLES		
MALE	-.204 (.263)	-.087 (.276)
BLACK	.549 (.350)	.589 (.349)
ACT	-.016 (.038)	-.013 (.038)
$MAJOR_1$	1.230 (.816)	1.097 (.821)
$MAJOR_2$	1.015 (.772)	1.050 (.771)
$MAJOR_3$	.891 (.789)	.977 (.796)
$MAJOR_4$	1.410 (.782)*	1.384 (.779)
$MAJOR_5$	1.375 (.762)*	1.395 (.757)*
$MAJOR_6$	1.604 (.797)**	1.546 (.797)*
$MAJOR_7$	2.006 (.827)**	1.922(.826)**
HEALTH_BAD	.221 (.478)	.374 (.482)
HEALTH_EXC	.010 (.258)	.006 (.259)
COMPUTER-IN-ROOM	.066 (.261)	.180 (.270)
CONSTANT	2.222 (1.212)*	2.132 (1.211)
	R^2=.179	R^2=.198

Note: Uses the subset of these students whose roommates are also members of the sample and are not missing values of RSTUDYHS and REXSTUDY. *significant at .10 **significant at .05

Table 6
Estimates of the effect of studying on grade performance:
Ordinary Least Squares and Fixed Effects

Independent Variable	**OLS** **n=210** **estimate (std. error)**	**Fixed Effects** **n=210** **estimate (std. error)**
STUDY	.038 (.025)	-.043 (.027)*
SEX	-.132 (.084)	
BLACK	-.220 (.122)*	
ACT	.062 (.013)**	
$MAJOR_1$	.834 (.298)**	
$MAJOR_2$	.793 (.282)**	
$MAJOR_3$	.725 (.292)**	
$MAJOR_4$	.796 (.283)**	
$MAJOR_5$	.643(.280)**	
$MAJOR_6$	.664(.292)**	
$MAJOR_7$	.901 (.304)**	
HEALTH_BAD	.019(.166)	
HEALTH_EXC	.127 (.086)	
COMPUTER-IN-ROOM	.018 (.088)	
CONSTANT	.719 (.408)*	-.050 (.047)
	R^2=.273	

*significant at .10
**significant at .05

Table 7a
Estimates of the effect of studying on grade performance:
Instrumental Variables
using game instruments

Independent Variable	IV instrument: RGAME n=210 estimate (std. error)	IV instrument: RCONSOLE n=210 estimate (std. error)	IV instrument: RCOMPUTER n=210 estimate (std. error)	IV instruments: RCONSOLE, RCOMPUTER n=210 estimate (std. error)	IV instruments: RGAME, OGAME x RGAME n=210 estimate (std. error)
STUDY	.360 (.183)**	.511 (.308)*	.312 (.239)	.415 (.209)**	.321 (.163)**
OGAME					.099 (.154)
SEX	-.023 (.129)	.027 (.175)	-.040 (.133)	-.005 (.142)	-.065 (.116)
BLACK	-.356 (.183)*	-.420 (.243)*	-.336 (.185)*	-.379 (.200)*	-.351 (.177)**
ACT	.069 (.018)**	.072 (.022)**	.068 (.017)**	.070 (.019)**	.067 (.016)**
$MAJOR_1$	.393 (.474)	.185 (.652)	.459 (.498)	.318 (.520)	.486 (.426)
$MAJOR_2$	.356 (.454)	.151 (.629)	.422 (.481)	.282 (.499)	.426 (.415)
$MAJOR_3$	.335 (.452)	.152 (.613)	.393 (.468)	.268 (.495)	.371 (.427)
$MAJOR_4$	.298 (.474)	.064 (.669)	.373 (.513)	.214 (.523)	.379 (.429)
$MAJOR_5$	.174 (.462)	-.046 (.647)	.244 (.495)	.094 (.508)	.241 (.423)
$MAJOR_6$	.091 (.510)	-.077 (.811)	.335 (.623)	.122 (.616)	.180 (.459)
$MAJOR_7$	.235 (.555)	-.178 (.731_	.177 (.561)	-.006 (.563)	.332 (.501)
HEALTH_BAD	-.029 (.226)	-.052 (.282)	-.022 (.213)	-.037 (.245)	.-.051 (.222)
HEALTH_EXC	.115 (.117)	.109 (.145)	.117 (.110)	.113 (.126)	.123 (.111)
COMPUTER-IN-ROOM	-.005 (.121)	-.016 (.150)	-.001 (.113)	-.009 (.131)	-.029 (.124)
CONSTANT	-.073 (.709)	-.445 (1.101)	.045 (.779)	-.207 (.783)	-.029 (.124)

*significant at .10
**significant at .05

Table 7b
Estimates of the effect of studying on grade performance:
Instrumental Variables
using both game instruments and roommate study instruments

Independent Variable	IV instruments: video game RGAME, RSTUDYHS, REXSTUDY n=176 estimate (std. error)	IV instruments: video game RGAME, OGAMExRGAME RSTUDYHS, REXSTUDY n=176 estimate (std. error)
STUDY	.291 (.121)**	.295 (.118)**
OGAME		-.011 (.138)
SEX	-.010 (.126)	-.004 (.127)
BLACK	-.334 (.176)*	-.336 (.178)*
ACT	.072 (.018)**	.072 (.018)**
$MAJOR_1$	.576 (.410)	.565 (.407)
$MAJOR_2$	.475 (.380)	.469 (.380)
$MAJOR_3$	.467 (.389)	.463 (.393)
$MAJOR_4$	.411 (.403)	.403 (.401)
$MAJOR_5$	.366 (.389)	.359 (.388)
$MAJOR_6$	.143 (.427)	.132 (.422)
$MAJOR_7$	.243 (.468)	.230 (.461)
HEALTH_BAD	-.020 (.219)	-.017 (.224)
HEALTH_EXC	.158 (.118)	.158 (.120)
COMPUTER-IN-ROOM	.029 (.118)	.295 (.123)
CONSTANT	-.062 (.638)	-.076 (.634)

Note: The first, second, and fourth columns use the entire sample of individuals with randomly assigned roommates. The third, which takes advantage of roommates' reports of how many hours they studied per week in high school (RSTUDYHS) and how many hours they expect to study per day in college (REXSTUDY) uses the subset of these students whose roommates are also members of the sample and are not missing values of RSTUDYHS and REXSTUDY.
*significant at .10
**significant at .05

Appendix A: Survey questions

Survey Question A

We are interested in certain items that you or your roommate might have at college. Which of the following items did you or your roommate bring to school at the beginning of the academic year? Please put a check in row one if you brought the item and a check in row two if your roommate brought the item

	Video Games	**Computer Games**	**Computer**
You	_____	_____	_____
Roommate	_____	_____	_____

Survey Question B.

In the last 7 days (one week), how many times were your classes scheduled to meet?_____
Please count up carefully the number of scheduled class meeting for each one of the seven days and add them together. (If your schedule for a particular day included one math class meeting, one GST class, a biology lab, and a music class you would count 4 for that day. Add together these numbers for each day to get a total for the week.

How many of these classes did you actually attend? ________

Survey Question C.

We are interested in where you studied. For a typical week during the Fall semester, tell us the percentage of your study time that took place in each of the following places.
Note: Numbers on the five lines should add up to 100
In dorm room (or at home if live off campus) with TV on _______
In dorm room (or at home if live off campus) without TV on _______
In library, empty classroom, quiet study lounge, or other quite place _______
In TV lounge, other (non-quiet) lounges _______
Other places _______

Survey #5 (Please complete both sides of this sheet) CPO 1971 3

Question A.

Reminders: Be sure to put an arrow (-->) next to the time that it is right now. And label this arrow with the words **YESTERDAY** and **START**.

Beginning with the **What were you doing** box next to the arrow, fill in your activities starting 24 hours ago (yesterday) and ending right before you began completing this survey.

Please use the 13 words listed in BOLD on the right of this page to describe your activities.

Time Period	What were you doing?	Time Period	What were you doing?
MORNING		EVENING	
6:00 AM		6:00 PM	
6:20 AM		6:20 PM	
6:40 AM		6:40 PM	
7:00 AM		7:00 PM	
7:20 AM		7:20 PM	
7:40 AM		7:40 PM	
8:00 AM		8:00 PM	
8:20 AM		8:20 PM	
8:40 AM		8:40 PM	
9:00 AM		9:00 PM	
9:20 AM		9:20 PM	
9:40 AM		9:40 PM	
10:00 AM		10:00 PM	
10:20 AM		10:20 PM	
10:40 AM		10:40 PM	
11:00 AM		11:00 PM	
11:20 AM		11:20 PM	
11:40 AM		11:40 PM	
AFTERNOON		NIGHT	
12:00 noon		12:00 midnight	
12:20 PM		12:20 AM	
12:40 PM		12:40 AM	
1:00 PM		1:00 AM	
1:20 PM		1:20 AM	
1:40 PM		1:40 AM	
2:00 PM		2:00 AM	
2:20 PM		2:20 AM	
2:40 PM		2:40 AM	
3:00 PM		3:00 AM	
3:20 PM		3:20 AM	
3:40 PM		3:40 AM	
4:00 PM		4:00 AM	
4:20 PM		4:20 AM	
4:40 PM		4:40 AM	
5:00 PM		5:00 AM	
5:20 PM		5:20 AM	
5:40 PM		5:40 AM	

LIST OF WORDS in bold

In Class
Attending class, attending labs, attending required class sessions

Studying (Outside of class time)
(refer to **pg 2** for more details)

Athletics
(Intercollegiate or Intramural - games or practice)

Clubs

Exercising

Recreation
(reading which is unrelated to courses, listening to music, watching movie, spending time with friends, etc.)

Shopping

Eating

Sleeping

Partying

Personal

Working (in Labor position)

Other
(Please describe on your sheet)

Appendix B. Do the additional instruments (from Section 5) satisfy the exogeneity requirement?

Tables Appendix.1a and Appendix.1b present results analogous to Tables 4a and 4b for the $RSTUDYHS_i$ variable. Appendix.2a, and Appendix.2b present results analogous to Tables 4a and 4b for the $REXSTUDY_i$ variable. We find little evidence that behaviors other than study-effort are influenced by the presence of a roommate with particular values of $REXSTUDY_i$ and $RSTUDYHS_i$. The $RSTUDYHS_i$ variable is not significant at .10 in any of the eight regressions in Appendix.1. The $REXSTUDY_i$ variable is significant at .10 in only one of the eight regressions in Appendix.2 with students in the sample who have roommates who expected to study one more hour per day in college going to be bed about six minutes later per night.

Table Appendix.1a
The effect of RSTUDYHS on other behaviors, n=176

Independent Variable	Dependent Variable PATTEND proportion of classes attended estimate (std. error)	Dependent Variable CLASSHOURS daily hours in class estimate (std. error)	Dependent Variable SLEEP daily sleep hours estimate (std. error)	Dependent Variable BEDTIME time student went to sleep# estimate (std. error)
RSTUDYHS	.0001 (.0004)	-.001 (.009)	-.007 (.010)	-.006 (.008)
MALE	.0007 (.009)	.005 (.194)	.307 (.217)	-.125 (.200)
CONSTANT	.956 (.008)**	3.452 (.164)**	7.226 (.184)**	.891 (.130)*
	R^2=.0006	R^2=.0002	R^2=.014	R^2=.011

*significant at .10
**significant at .05
#dependent variable is created so that it is zero at 12:00 midnight. Positive numbers represent hours after midnight. Negative numbers represent hours before midnight.

Table Appendix.1b
The effect of RSTUDYHS on additional behaviors

Independent Variable	Dependent Variable percentage of study time that takes place in dorm room estimate (std. error)	Dependent Variable percentage of study time that takes place in dorm room with tv on estimate (std. error)	Dependent Variable weekly hours using computer for academic purposes estimate (std.error)	Dependent Variable daily hours partying estimate (std. error)
RSTUDYHS	.199 (.226)	.905 (.804)	-.006 (.053)	-.001 (.002)
MALE	-5.823 (4.622)	-3.838 (2.968)	–.120 (1.096)	-.001 (.050)
CONSTANT	59.828 (3.959)**	11.024 (3.550)**	7.104 (.938)**	.126 (.043)**
	R^2=.014	R^2=.018	R^2=.0002	R^2=0.011

*significant at .10
**significant at .05

Table Appendix.2a
The effect of REXSTUDY on other behaviors

Independent Variable	**Dependent Variable PATTEND proportion of classes attended**	**Dependent Variable CLASSHOURS daily hours in class**	**Dependent Variable SLEEP daily sleep hours**	**Dependent Variable BEDTIME time student went to sleep#**
	estimate (std. error)	**estimate (std. error)**	**estimate (std. error)**	**estimate (std. error)**
REXSTUDY	.0009 (.002)	-.001 (.053)	.022 (.059)	.097 (.055)*
MALE	.0008 (.009)	.005 (.195)	.316 (.218)	-.118 (.200)
CONSTANT	.955 (.011)**	3.444 (.232)**	7.071 (.260)**	.503 (.235)**
	R^2=.0007	R^2=.0000	R^2=.012	R^2=.020

*significant at .10
**significant at .05
#dependent variable is created so that it is zero at 12:00 midnight. Positive numbers represent hours after midnight. Negative numbers represent hours before midnight.

Table Appendix.2b
The effect of REXSTUDY on additional behaviors

Independent Variable	**Dependent Variable percentage of study time that takes place in dorm room**	**Dependent Variable percentage of study time that takes place in dorm room with tv on**	**Dependent Variable hours per week using computer for academic purposes**	**Dependent Variable daily hours partying**
	estimate (std. error)	**estimate (std. error)**	**estimate (std.error)**	**estimate (std. error)**
REXSTUDY	.964 (1.258)	.905 (.804)	.299 (.298)	-.020 (.013)
MALE	-5.588 (4.643)	-3.838 (2.968)	-.019 (1.097)	.014 (.050)
CONSTANT	58.441 (5.554)**	11.024 (3.550)**	5.940 (1.31)**	.182 (.060)**
	R^2=.008	R^2=.018	R^2=.006	R^2=0.012

*significant at .10
**significant at .05