Smartphones and Child Injuries

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Abstract

From 2005 to 2012, injuries to children under five increased by 10%, possibly because smartphones distract parents from supervising their children. I exploit the expansion of AT&T's 3G network in both difference-in-differences and a triple difference framework and find that hospitals experienced a 5% increase in emergency department visits for children ages 0-5, but none for children ages 6-10, after getting 3G. Using a model of parent-child interactions, I derive testable predictions of the distraction hypothesis. The empirical work confirms the model's predictions, which I put forth as evidence that smartphones have causally increased child injuries. JEL Classication Numbers: J13, L82, O15

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1 Introduction

Nonfatal, unintentional injuries to children under five increased by 10% from 2006-07 to 2011-12 (CDC, 2012). Table I, using data described below, shows that emergency department visits increased substantially for children under five but hardly at all for children five and older. This increase is puzzling because many investments over the past few years have gone towards improving child safety. This rapid increase in child injuries is a public health concern and worthy of policy consideration, but currently there is no understanding of what caused this sudden increase.

Could the rapid adoption of smartphones explain this puzzle? In a Wall Street Journal article, Worthen (2012) advances the hypothesis, supported by many specialists, that smartphones distract parents from superivising their children, which increases the risk of injury. But he notes that no study has provided causal evidence linking smartphone use to child injuries. In this paper, I fill this gap.

Understanding how smartphones affect child injuries provides insights on the role parents play in their children's human capital formation. Parents help children develop skills through investments of time (Becker and Tomes, 1976), and the highest return investments are those that involve high-quality, stimulating interactions (Price, 2008; Gertler et al, 2014; Sacerdote, 2007; Bjorklund, Lindahl, and Plug, 2006). If smartphones distract parents during these activities, they may not realize a high return on their investment, and they may even be so distracting that parents forgo investing in human capital at all (Olken, 2009). Because smartphones—i.e. cell phones with the ability to browse the internet, stream videos, send and receive emails, and run various software applications—are a recent innovation that provide unprecedented access to information and distractions, our understanding of their impacts on parent-child relations are limited and need to be explored.

Specifically, I am interested in how smartphones lead parents[1] to make decisions that increase the risk of child injury. Studying child injuries is a useful proxy for human capital because they both follow similar processes. Parents are important inputs in reducing injuries and forming human capital, and distractions diminish their impact on both. Moreover, there are several added benefits of examining child injuries. They are easily measured and compared across individuals, and they are immediately observable. Furthermore, they are

[1] Throughout the article I use the term parents, but the concept includes any caregiver responsible for supervising children, such as older siblings, nannies, etc.

salient and costly, such that if parents are distracted enough to let their children be injured, they may also be distracted during critical learning opportunities whose effects are not realized until the long-run.

Identifying the causal effect of smartphone use on child injuries is difficult. Hospitals do not collect data on what parents are doing when the child is injured, and any data would be subject to reporting errors if surveyed. Also, because parents select into device use, research that relies on observing parent-child interactions (e.g. Radesky et al, 2014) confound causal effects with selection driven by parenting preferences. Ideally, one would use random assignment to address the question, like Byington and Schwebel (2013) do in a lab setting to look at show smartphones increase personal injury risk in a simulated street-crossing experiment.

Instead of directly investigating the impact of smartphones on injuries, I look at a narrower question: did hospitals experience a causal increase in emergency department visits after getting access to the 3G network? At the iPhone 3G's release in 2008, consumers could use it only on AT&T's network. Not all cities had access to its 3G network, but many gained it over time. I use the advent of Apple's iPhone 3G combined with the rollout of AT&T's 3G network to provide exogenous variation in the ownership and use of smartphones. Because 3G coverage is independent of individual parental characteristics and other accident-causing factors, differences between covered and non-covered areas reflect the influence of smartphones on injuries.I match the rollout data to hospitals tracked by the National Electronic Injury Surveillance System (NEISS), created by the Consumer Product Safety Commission (CPSC), which tracks all consumer product related injuries in emergency departments at a nationally representative sample of hospitals.

Using the hospital-level variation in 3G access, I find hospitals experience a 9% increase in emergency department visits for children under five after receiving AT&T's 3G network. Because this estimate fails to control for confounding factors that are correlated with 3G entry, I also perform a triple difference analysis, using children between the ages of five and ten as the control group because of their weaker dependence on parents for supervision. The triple difference results show that 3G increased ED visits for children under five by 5.3%.

To understand the causal mechanism, I model the parent-child interaction and emulate Moretti's (2011) methodology, creating comparative statics for aggregate data. The model accounts for two effects that a smartphone can have on the parent-child interaction. First, they increase the opportunity cost of supervising children. Second, they decrease the parent's opportunity cost of participating in activities with the child. In the first case, injuries increase through neglect, and in the second injuries increase mechanically from more children playing. Using the model to drive the empirical work, I find strong evidence that smartphones are indeed distracting parents. I find that injuries increase in riskier activities, when parental supervision can make a decisive role in preventing accidents. These effects are absent in activities where the parents are not the primary supervisors and in activities where supervision makes no difference on outcomes. The evidence from these results strongly supports a scenario where parents are distracted by their smartphones and decrease supervising their children.

This paper contributes to a literature on the effects of media on the family and society. Methodologically, I follow the literature, as surveyed by Price and Dahl (2012), using natural variation in the availability of the media to estimate the causal effect. Economists have looked at how television and video games affect child human capital development and have found positive (Kearney and Levine, 2015; Suziedelyte, 2015) or no effects (Gentzkow and Shapiro, 2008). Although I do not have direct measures of cognitive effects, I contribute to the literature on media and the environment affecting children. For instance, evidence from outside the U.S. shows that television empowered women and decreased fertility (La Ferrara, Chong, and Duryea, 2012; Jensen and Oster, 2009). Research shows that empowered women and the quanity-quality tradeoff both imply that these effects should also positively affect children. Furthermore, Olken (2009) shows that radio and television decreased participation in Indonesian social organizations, and soem worry that smartphones may have a similar effect on social capital by diverting the attention of those who still participate. My results indicate this may be an issue, since parents are allowing themselves to be distracted, resulting in their children being injured.

Regarding cell phones specifically, I examine a new effect previously unexplored in the literature. One of the most extensive literatures on cell phones is the effect they have on car accidents through distracted driving (for a survey, see McCartt, Hellings, and Bratiman; 2006). Although this is an important issue, most of the time people use their phones is outside of a vehicle, and the literature has little to say on the effects of these uses. Of course, studying these other outcomes is difficult because of measurement and identification. A large contribution I make is introducing a new identification strategy that may help address other questions on the affect of smartphones on daily life.

2 Identification Strategy - AT&T's 3G Expansion

To understand how smartphones influence child injuries, I look at the effect of 3G on hospital emergency department (ED) visits for children under five. I use the expansion of AT&T's network because it was the exclusive iPhone carrier until 2011[2]. The iPhone started the modern smartphone era, and it quickly gained a considerable market share after its release. In the iPhone 3G's first year, consumers bought over 20 million units and downloaded over 1.5 billion applications from the App Store (Apple 2009). In December 2009, Apple held the second largest share of the smartphone market, accounting for 25% of smartphone operating systems (ComScore 2010). This figure, however, understates the iPhone's share of 3G phones because it includes mostly phones that could not access 3G. Indeed, RIM, the maker of Blackberry, had the largest market penetration with 40% of operating systems, yet RIM's products could not access 3G networks and were targeted more toward business users. Apple's primary competitor in operating systems is Google with its Android platform, but in this same report it had only 5% of the market. Adoption of the iPhone is clearly the major change in the market during this period.

One might worry about how other networks will affect the results. First of all, competition will drive a strong correlation in 3G access across network providers. If a competing network offers 3G coverage, and customers switch away from AT&T, then we expect AT&T to respond by improving coverage. Nevertheless, the presence of other networks could introduce some measurement error in which hospitals are covered or the timing of the expansion. But other networks can only provide non-Apple phones, and Apple's market share is five times greater than Google's at the end of 2009. Moreover, some of those Google phones are also on the AT&T network. Hence, the AT&T coverage provides a much larger shock to the market and is the right network to use.

Another important question to consider is why markets receive 3G at different times. The primary explanation is costly installation. Establishing a network requires installing enough infrastructure to provide seamless coverage, and this infrastructure requires large capital outlays. For instance, for AT&T to build a 3G network, in less than two years it

[2]Apple released the iPhone 3G, 3Gs, 4, 4S during this period, and sales of iPhones increased consistently during the entire period.

spent $30 million in Houston, $40 million in Chicago, $50 million in Dallas, and $65 million in San Francisco (AT&T 2009a-c, 2010). At a minimum, a firm would not enter the market unless the present expected value of revenues exceeded the cost of installation. Uncertainty about adoption reduces the expected revenues, and uncertainty could vary across markets. Furthermore, AT&T has to also consider that the technology is evolving and that upgrades (such as 4G) will also require costly installations. Thus market entry is an optimal stopping problem, which creates variation in the timing of entry across markets.

The variation in AT&T's 3G coverage coincides with the increase in child ED visits. In Figure I, I plot the fraction of observations (hospital-months) that have 3G coverage by year. In 2008, just under 30% of hospital-months had 3G coverage, and in the final years about 80% of hospital-months were covered. About 20% of the hospitals even today do not have 3G coverage. I also plot the aggregate number of injuries for each year in the same graph, and a very strong correlation emerges. The correlation is insufficient to prove the causal hypothesis, but it gives proper motivation for the rest of the analysis.

For my identification strategy to be valid, it must be that the presence of 3G is uncorrelated with other unobservable factors that influence injuries. In the Appendix Figure AI, I show the geographical distribution of hospitals next to the distribution of hospitals that had 3G when Apple released the iPhone, and a urban-rural pattern is clearly present. A hazard analysis, reported in Appendix Table AI, confirms this pattern and reveals that early 3G adopters had higher population levels and density, but did not have higher household incomes, than later adopters. These patterns make sense, since high population means more customers and higher density means less infrastructure per capita. City fixed effects capture the effects of population and other unobserved, time-invariant factors. The month-year fixed effects control for the macro shocks, such as the recession, that affect all cities at the same time. Controlling for these fixed effects, the presence of 3G is exogenous to other factors affecting ED visits.

3 Data

Data for nonfatal child injuries come from the National Electronic Injury Surveillance System (NEISS) run by the Consumer Product Safety Commission (CPSC). Using a nationally representative set of 100 hospitals, the CPSC records all emergency department visits in which a consumer product was associated with the injury. The broad definition of "consumer product"[3] allows the data to cover 67% of unintentional injuries. NEISS records the age of the patient, the location where the injury occurred, the product involved, and the hospital where the patient was treated. I aggregate injuries at the hospital-month-age level using a sample spanning January 2003 to December 2012.

The big strength of the data is that it is a large, high-frequency panel. Controlling for hospital fixed effects eliminates permanent, unobservable differences across hospitals correlated with the 3G expansion. Furthermore, I can control for hospital-specific trends in injuries. Privacy protections limit using external data on time-variant city or hospital effects to test for confounding effects.

Because the NEISS contains sensitive patient information, the CPSC cannot release the hospital or city where the patient was treated. For the current analysis, the 3G roll-out data was coded separately and sent to the CPSC, who then merged the coverage data onto the NEISS and removed any information linking them to the hospital's location. I am able to identify the same hospital across observations within the data—hence the ability to use hospital fixed effects—but I do not know where the hospital is in the U.S. The CPSC agreed to provide the data because my treatment variable is binary and 3G expands in waves, creating limited opportunity to identify hospitals. Continuous variables, on the other hand, can very easily be used to map observations to locales. Thus, I cannot add variables such as population or smartphone use rates. This restriction prevents me from estimating the elasticity of injuries to the population of smartphone users. But it does not inhibit finding causal effects of 3G on ED visits.

The data on the product involved and where the injury occurred provide leverage in analyzing the mechanism. By classifying products as high/low-risk or heavy/light-parental supervision, I can test the implications that make predictions about how injury rates should change if distraction is the mechanism. While the product data is rich in detail, the location data (i.e. whether the injury occurred at home, in a public space, etc.) is not as complete, with 25% of the injuries listing "unknown" for the location. I include all of these observations in the main results, but I cannot use them in the subgroup analyses that rely on knowing the location. I assume the unknown locations are random and that omitting them from the location-specific subsets does not bias the results.

Data for the AT&T 3G coverage comes from AT&T press releases available on its website.

[3] The CPSC defines consumer product as "any article produced or distributed for use by a consumer in or around a home, school or recreational area" (CPSC, 2014)

With the completion of almost every 3G installation, AT&T issued a press release announcing the cities where it had extended coverage. Using the date of the press release and the cities named, I can reconstruct the rollout of coverage. To validate the press releases and fill gaps, I also grabbed historical data for the AT&T Coverage Viewer[4] from the Internet Archive Way Back Machine[5]. To construct my treatment variable, I found when each hospital received 3G coverage from AT&T. Coverage is a binary variable, and because hospitals treat many patients from the surrounding areas and that many patients in the immediate vicinity may work in the surrounding areas, my treatment variable equals one as soon as a city within 30 miles of the hospital receives 3G.

4 Empirical Results

4.1 Difference-in-Differences

The most straightforward test for the effect of 3G on child ED visits is to do a difference-in-differences analysis exploiting the differential exposure to 3G. I estimate the following regression

$$\sinh^{-1}(y_{hta}) = \delta_h + \delta_t + \delta_a + \beta 3G_{ht} + \varepsilon_{ht}. \tag{1}$$

The hospital dummies (δ_h) control for permanent differences across hospitals, and the month-year dummies (δ_t) control for transitory shocks that affect all hospitals in the same period. The age dummies (δ_a) account for differences across the ages included in the sample, which is limited to patients under the age of five. I use the inverse hyperbolic sine (IHS) as the dependent variable to resolve concerns with months where a hospital records zero ED visits for an age group. In a regression framework, the IHS behaves like the log transformation (Burbidge et al. 1988), but the IHS is defined at zero. The coefficient can be interpreted as a percentage change. The results are robust to other specifications, which I include in Appendix Table AII.

Evidence from estimating equation 1 indicates that hospitals experienced an increase in ED visits for children under five after getting 3G coverage. The results, reported in Table II, show that injuries increased by 9% in the 3G period. The standard errors are clustered at the hospital level, and there is still enough power for the estimate to be significant at

[4] http://www.wireless.att.com/coverageviewer/popUp_3g.jsp

[5] http://www.archive.org/web

the 5% level. The coefficient's magnitude suggests that 3G can explain 90% of the increase described in the introduction.

As a robustness check, I include hospital-specific trends in the regression. In Table II, I report the results from three different specifications of the hospital-specific trends: linear, quadratic, and cubic. In the quadratic case, the effect remains, but under the linear and cubic regressions, the size and significance of the coefficient decrease. The sensitivity of the results to the specification of trends may indicate that a confounding factor drives the results.

Indeed, the weakness of the difference-in-differences approach is a failure to account for the time-variant, hospital specific shock (ε_{ht}). For example, the expansion of 3G coincides with the same time period as the recession, which had a differential effect across cities. If harder hit areas experience an increase in ED visits—either through more injuries or a greater likelihood of bringing kids to the ED—and if the local severity of the recession is correlated with the timing of 3G expansion, then the regression attributes recession effects to the presence of 3G. Another confounding factor could be migration: cities with 3G are more likely to gain households, and cities without might lose households, therefore the increase could be a result of demographic change correlated with where AT&T placed its 3G. For any other local changes that increase injuries—for example, new playgrounds that provide more opportunities for children to get hurt, or hospital improvements that increase the demand for ED services—and are correlated with the treatment, the regression will overestimate the effect. Because it is unlikely the timing of the hospital-specific shocks matches the 3G rollout, the differential rollout mitigates these concerns, but it is still important to show these concerns do not confound the analysis.

To address the concern of time-variant shocks, I use a triple difference analysis. The key to this approach is the assumption that 3G has an effect on young children but no effect on older children. If injuries increase because smartphones distract parents, then older children provide a natural comparison group because they rely less on parents when evaluating risky situations. By splitting the observations into under five years old and five and older, I estimate the following regression

$$\sinh^{-1}(y_{hta}) = \delta_h + \delta_t + \delta_a + \delta_h * Under5_a + \delta_t * Under5_a + \beta_1 3G_{ht} + \beta_2 3G_{ht} * Under5_a + \varepsilon_{hta}. \quad (2)$$

This regression includes all injuries to children 10 and younger, with the $Under5_a$ dummy indicating whether the age group is young enough to be affected by 3G. Including the older children in the 3G main effect controls for shocks that coincide with the 3G rollout. For this regression to provide solid evidence in favor of a causal effect, we should find no effect of 3G on older children ($\beta_1 = 0$) and a positive effect for younger children ($\beta_2 > 0$).

Even under the triple difference approach, there is strong evidence for a causal effect. The results are reported in Table III, and they indicate that the effect falls on the younger children. Older children experience a 3.8% increase in injuries, but this coefficient is not statistically significant. Younger children, on the other hand, see a 5.3% increase above the older children, significant at the 1% level. This finding is robust to all hospital-specific time trends, reported in the same table, and other regression specifications reported in Appendix Table AIII. Of the estimated effect in the original difference-in-differences regression, 42% can be explained by factors that affect everyone, and the remaining 58% is attributable to 3G.

The triple difference results eliminate many confounding factors because they show no evidence of increase for injuries to older children. If migration or improved facilities were the cause, we would observe a change for children of all ages. Indeed, any alternate hypothesis must explain why the effect differs by age.

4.2 Parallel Trends

The key assumption behind the validity of the difference-in-differences estimator is that the treatment and control group experienced parallel trends. If hospitals that get 3G would have experienced an increase in injuries in the treatment period regardless of the network, then the regression misattributes the treatment effect to differences in trends. The triple difference results are a strong argument against this concern. However, one could argue that the factors influencing injuries to children under five experience different trends than those that affect older children, and therefore the triple difference is not sufficient. To test the parallel trends assumption, I estimate a regression that includes dummies for six-month bins before and after 3G entrance. If the pre-3G dummies are not equal to zero, then there is evidence that the treatment effect is capturing pre-existing trends.

The results from this regression does not provide evidence against the parallel trends assumption. I plot the coefficients and their standard errors in Figure II. The omitted group is the six months prior to the entrance of 3G, such that all coefficients are relative to the injuries occuring in this period. All of the pre-period coefficients are indistinguishable from zero; furthermore, there is no distinct trend in the magnitudes, and the coefficients even flip signs. On the other hand, all of the post-period coefficients are positive and most are statistically significant. This evidence supports a causal interpretation.

4.3 Feasibility of Magnitudes

So far, I have focused on making a causal case for injuries increasing after 3G arrives, but I have ignored the magnitudes. The triple difference results indicate that hospitals had 5.3% more ED visits for children under five after 3G entered. But is this even a feasible effect size given the market penetration of smartphones?

A back-of-the-envelope calcluation shows this is a reasonable effect. Assuming the effect is constant for the whole country, I take the 2005-06 average injuries for each age group under 5, found in Table I, and multiply them by 5.3%. This calculation says that we would find an extra 79,913 injuries by 2011, when the 3G coverage is complete. According to the PewResearch Internet Project (Pew Research Center, 2011), in 2011, 7% of Americans owned a smartphone and had a child under the age of 5. Taking the adult population of the U.S. to be 250 million, then the implied injury rate is 4.6 out of every 1,000 parents of children 5 and under who use a smartphone experience an injury. To put this number in perspective, the injury rate for cars is about 10.6 per 1,000 drivers (NHTSA, 2010); the injury rate from cars is more than twice the injury rate for smartphone users, yet the car injury rate is not high enough to prevent millions of drivers from taking the risk every day. Hence, while the increase is significant relative to the overall injury rates, it does not seem unreasonable to believe relative to the population of smartphone users.

5 A Model of Parental Superivision — Mechanism

The difference-in-differences results established strong evidence for a causal effect of 3G on hospital ED visit, and in this section I pursue the causal mechanism. Specifically, the hypothesis is that smartphones are distracting parents from watching their children, which leads to more injuries and more ED visits. Because individual-level data linking smartphone use and child injuries is unavailable, I follow similar methodology as Moretti (2011) and outline intuitive comparative statistics that I can use on aggregate data. I model how parents choose to interact with and supervise their children. Then I show how smartphones affect these decisions and derive testable implications that I can take to the data.

I begin by modelling the supervision decision for a single parent and then move into the aggregate case. When children play under parental supervision, there are two states (s): bad (B), where the child is injured playing, and good (G), where he is not. The state is random but conditional on the parent's level of supervision (z). The parent can choose a high (H) or low (L) level of supervision, with the probability of realizing the good state equal to e_z. Injuries occur less when the parent provides more supervision, hence $e_H > e_L$.

The parent's payouts depend on the the level of supervision and the state. The preference ordering is as follows

$$u_{LG} > u_{HG} > u_{HB} > u_{LB}.$$

Conditional on no injuries occuring, the parent prefers less supervision to more because of the other activities the parent could be doing instead of supervising (e.g. reading a book, talking to friends). Of course, conditional on the level of supervision, the good state is better than the bad state. Finally, conditional on the child being injured, the parent prefers higher supervision to lower supervision to avoid guilt from knowing more supervision might have prevented the injury.

A risk-neutral parent will choose high supervision if

$$e_H u_{HG} + (1 - e_H)u_{HB} > e_L u_{LG} + (1 - e_L)u_{LB}.$$

For expositional purposes, and without loss of generality, I set $u_{LB} = 0$. Then the expression can be rearranged such that the parent will choose high supervision if

$$u_{LG} < e_L^{-1}[e_H u_{HG} + (1 - e_H)u_{HB}] = U_H.$$

Thus, the straightforward result is supervision decreases as the parent's payout from lower supervision increases.

Because parents are not always interacting with their children, I move to the decision to participate in activity requiring supervision. Parents have an outside option–such as work, or independent lesiure–such that the payout from not interacting with the child is U_O. Hence, the risk-neutral parent will decide to interact with the child if

$$U_O < max\{u_{LG}, U_H\}.$$

Participation increases as the utility from not supervising increases.

Having modeled the individual parent decisions, I show how parental deicisons translate into aggregate injuries. Assume a continuum of parents of size one, where parents are indexed by i. The probability of realizing the good state conditional on the level of supervision (e_z) is constant across parents. But each parent i has a specific set of payout jointly distributed parameters $\{U_{Oi}, U_{Hi}, u_{LGi}\}$. The payout from lower supervision can be expressed as $u_{LGi} = \bar{u}_{LG} + \varepsilon_i$, where $\bar{u}_{LG}$ is the average payout from low supervision and ε_i is a random variable with mean zero. We can think of this ε_i as how much the parent values the non-supervision option. The distribution function of the difference between $U_{Hi} - \varepsilon_i$ is $F(u_H - \varepsilon)$. The parent will choose to supply low supervision if $U_{Hi} < u_{LG_i}$, which is equivalent to $U_{Hi} - \varepsilon_i < \bar{u}_{LG}$. Hence, the fraction of parents supplying low supervision is $F(\bar{u}_{LG})$, and the fraction supply high supervision is $1 - F(\bar{u}_{LG})$. Finally, let $G(u_O)$ be the distribution of U_O conditional on it being less than U_H; i.e. $G(u_O) = P(U_O < u_O | U_O < U_H)P(U_O < U_H)$. The fraction of parents participating is then $G(\bar{u}_{LG})$

The fraction of parents with injured children depends on the fraction participating in the activity and the distribution of supervision conditional on participation. The fraction of injuries can be expressed as

$$
\begin{aligned}
D &= G(\bar{u}_{LG})[(1 - e_H)(1 - F(\bar{u}_{LG})) + (1 - e_L)F(\bar{u}_{LG})] \\
&= G(\bar{u}_{LG})(1 - e_H + (e_H - e_L)F(\bar{u}_{LG})).
\end{aligned}
$$

This expression is the foundation for the testable implications.

5.1 Testable Implications

I model the introduction of smartphones as an increase in the average payout from lower supervision ($\bar{u}_{LG}$). The key feature of a smartphone is that it lowers the cost of entertainment and work. It decreases the cost of entertainment by providing easy access to games, videos, music, and websites through a mobile phone. It decreases the cost of work by providing access to email and documents. Users could access all of these things before through a computer or television, but with a smartphone, users could now do these things from nearly anywhere. Under high supervision, parents never use smartphones when with their children, so it is natural not to change the payout from high supervision. In the low supervision case, when the child is not injured the parent is better-off with the smartphone because of the access to entertainment and work opportunities that the phone affords.

The effect of smartphones on injuries is thus

$$\frac{\partial D}{\partial \bar{u}_{LG}} = G'(\bar{u}_{LG})(1 - e_H + (e_H - e_L)F(\bar{u}_{LG})) + (e_H - e_L)G(\bar{u}_{LG})F'(\bar{u}_{LG})$$
$$> 0$$

That the expression is greater than zero is clear to see: $e_H - e_L > 0$ by definition, and by the properties of cumulative distribution functions the rest of the terms are also positive. This results leads to the first testable implication.

Implication 1: *By increasing the payout from lower supervision, smartphones increase injuries.*

A slight modification of the model can produce a result justifying the triple difference approach. As children get older, the effect of parental supervision decreases for two reasons. First, younger children are less able to identify risk and do so slower than older children (Hillier and Morrongiello, 1998). Parents have a strong effect on preventing unintentional injuries (Morrongiello and Dawber, 2000; Power, Olvera, and Hays, 2002; Schwebel and Brezausek, 2004), and being distracted will weaken this effect. Even the policies of child-care services recognize that younger children are at a greater risk of injury without proper supervision. For example, to be accredited by the National Association for the Education of Young Children (NAEYC), a daycare must have higher staff-to-child ratios for younger children (NAEYC, 2013).

The second reason why distractions will have a smaller effect on older children is that older children participate in fewer supervised activities. Because children gain more awareness as they age, parents give them more freedom. Furthermore, older kids spend a significant part of the day in school, where smartphones will have little to no effect on the teacher's behavior because of external enforcement. This means that even if parents adjust their supervision based on the child's age, the effect is still larger for younger kids.

In terms of the model, the probability of avoiding injury conditional with low supervision ($e_L(a)$) depends on the child's age a, such that $e'_L(a) > 0$ and $\lim_{a \to \infty} = e_H$. That is, as children age, they are better able to supervise themselves, such that the gap between supervision and no-supervision closes. Then the effect of smartphones varies by age:

$$\frac{\partial D}{\partial \bar{u}_{LG} \partial a} = -e'_L(a)[G'(\bar{u}_{LG})F(\bar{u}_{LG}) + G(\bar{u}_{LG})F'(\bar{u}_{LG})]$$
$$< 0.$$

Therefore, the modification produces a second testable implication.

Implication 2: *Smartphones increase injuries more for younger children than for older children.*

The next implications are derived from noting that the increase in injuries consists of two parts. The first part, $G'(\bar{u}_{LG})(1 - e_H + (e_H - e_L)F(\bar{u}_{LG}))$, is the participation effect; it is the change in injuries occuring as a result of smartphones inducing parents to interact more with their children. For example, now that the parent can check e-mail or read the news while at the playground, more children will play at the playground, and injuries will increase by virtue of more children playing. The second part, $(e_H - e_L)G(\bar{u}_{LG})F'(\bar{u}_{LG})$, is the distraction effect; it is the change in injuries from parents looking at phones instead of children.

Regarding policy relevance, it is important to know if the distraction effect is non-zero. To isolate this effect, we need to find activities where the participation effect is zero. That is, we need activities where smartphones will not induce children to participate more but could cause parents to be distracted. If ED visits in a given activity increase, but the participation effect is zero, then it must be that the distraction effect is non-zero. This leads to the following implication:

Implication 3: *If smartphones increase injuries by distracting parents, then injuries will increase even if participation remains constant.*

Finally, the model generates a placebo test. The size of the effect depends on the importance of parental supervision. If extra supervision does not change the probability of injury—that is, if $e_H = e_L$—then there should be no change in injuries. If we find activities where supervision has no effect, but still find an increase in injuries following 3G's entrance, then there is reason to supsect the validity of a causal effect.

Implication 4: *Placebo — smartphones have no effect on injuries where supervision is ineffectual.*

Together, these model implications form a test for whether smartphones are the mechanism driving the increase in child injuries.

5.2 Empirical Evidence

In my empirical work, I use the 3G network as a proxy for smartphone access. The availability of 3G affects the value of a smartphone because without it owners cannot fully exploit the phone's features. Some consumers will purchase a smartphone before 3G in anticipation of the network's availability, but even then the consumer cannot use it as much as owners who have the network already. Hence, the average utility from not supervising will increase more in 3G areas.

The work in the difference-in-differences section establishes Implications 1 and 2. In harmony with the first implication, the difference-in-differences results show that ED visits for children under five increased after hospitals got 3G. Implication 2 justifies the triple difference approach, and consistent with the model I find ED visits increased for children under five but not five and older.

Not only does Implication 2 justify the triple difference approach, but it also provides a richer test of whether smartphones induce the injuries. This result implies that splitting children between younger and older than five is arbitrary: all children could be affected, but the effect will be larger for younger children. Furthermore, it means that grouping children obscures the heterogeneous treatments occuring at each age. To address the heterogeneity, I run the same triple difference regression separately for each age group younger than 10, using injuries to 10 year-olds as the control group. I plot the coefficients for the 3G treatment from each regression in Figure III, and the pattern is clear: the effect is decreasing with age, as the model implies.

Exploring the next two mechanisms requires using the data on where the injury ocurred and in which activities. To test for differences in parental influence, I hold risk constant by looking at injuries that occur on playground equipment. With the NEISS data, I can split injuries between those that happen at school and those that do not. Injuries that occur at school are unlikely to be influenced by smartphones since the teachers face external incentives to not use their phones when they should be supervising. Furthermore, teacher supervision is divided across many children, such that an overall decrease in supervision is a much smaller decrease when considered in per child terms. On the other hand, if the distraction effect exists, then non-school playground injuries could increase since parents are more likely to supervise their children in these situations.

The comparison between school and non-school playground injuries shows a dramatic difference. As shown in Figure IV, on non-school playgrounds, injuries increase across all ages by about 4% after 3G enters. At schools, however, there is no change. Indeed, there is evidence of a decrease for most ages. Because school and non-school playgrounds are likely to be in the same physical condition, this eliminates concerns that the increase is a result of failure to make investments in equipment during the recession. Unfortunately, these results alone cannot tell us whether the increase in injuries comes from the distraction effect or the participation effect, since either could cause this increase. Indeed, the equal effect across all ages could be because the participation mechanism is non-zero since the distraction effect implies a larger impact for younger kids.

To test Implication 3, which isolates the distraction effect, I need to identify activities whose participation rates will be unaffected by the introduction of smartphones. Because children should not be bathing, sleeping, or using stairs more if their parents have smartphones, I use injuries related to these activities as examples of injuries where the participation effect is nonexistent. One might object that injuries on beds or in baths should be unaffected by smartphone use. Because parents cannot do much to prevent injuries once the children are asleep, beds provide a good test of the mechanism since injuries would only increase for little children if the parent left them unattended and awake on a bed. Similarly, injuries in bathtubs would increase if parents were distracted while the child was playing in the tub, and injuries on stairs would increase if a young child approaches stairs while the parent is distracted.

Figure V plots the coefficients from these three regressions. The patterns are striking: only the youngest children, who need the most supervision in these activities, experience an increase after 3G enters. Of particular interest is the comparison for injuries to children younger than one (infants), children between one and two (toddlers), and the rest. In the bath, injuries increased more for toddlers than for infants, and not at all for the rest of children; this pattern is consistent with parents giving significant attention to infants but becoming more confident with toddlers, and therefore more likely to use a smartphone. Stairs follow the same pattern, which again makes sense because infants are less mobile and therefore at less risk of falling down stairs. With beds, on the other hand, infants experience a very large increase in injuries and the effect quickly decreases with age; this is consistent with parents placing infants on a bed while checking a phone and believing the child will remain stationary, but then the child rolls off. These patterns are consistent with Implication 3 and suggest that parents are distracted.

Finally, I use the placebo test given by Implication 4. I look at an activity where parental supervision makes little difference but participation might increase: sports-related injuries. Spectator input has little effect on the moment-to-moment outcomes in sports, even in the small audiences that view children's sports. Thus, parental supervision has no effect on injuries. However, children might play sports more if their parents are more willing to take them to play, which would increase injuries. In the sports-only sample[6], in Figure VI, 3G has no effect on injuries. Because sports are one of the areas where the participation effect would be most evident, this suggests that participation is not driving the injuries.

6 Discussion

I use the rollout of 3G combined with a model of parental supervision to make a case for a causal effect of smartphone-induced child injuries. Hospitals experienced an increase in emergency department visits after getting 3G coverage, but only for children under five. Furthermore, the activities associated with these injuries are consistent with parents being distracted.

One concern with the analysis is that data privacy issues prevent me from using controls to eliminate confounding factors; however, this concern is small. The most convincing piece of evidence against an alternative hypothesis is the triple difference results that show children five and older at the same hospitals experienced no significant increase in injuries. Furthermore, the confounding factor would have to follow the same patterns established by the model and verified by the empirical evidence. This paper has convincingly made the case for smartphones causing injuries, and hopefully future work will generate greater precision on the size of the effect.

The results have ambiguous welfare implications since I do not model the child's utility. The model I used in this paper explicitly considered only the parent's utility from the interaction. If this payout incorporates the child's utility like a unitary household model, then we would conclude that the whole household is ex-ante better off, even with the increased risk of injury. But if the parent does not consider the child's well being or does not realize the phone is a distraction, then the parent might be gaining at the expense of the child. Even if we chose the correct set of modelling assumptions, some might find it appalling to believe

[6] The sports included in the NEISS data are: bowling, boxing, croquet, football, golf, lacrosse, archery, horseback riding, horseshoes, mountain climbing, billiards, surfing, water skiing, volleyball, soccer, table tennis, wrestling, scuba diving, tetherball, ice hockey, handball, snowmobiles, field hockey, snow tubing, water tubing, skeet shooting, roller skating, skating, badminton, fishing, rugby, ball sports, street hockey, ice boating, cheerleading, ice skating, martial arts, fencing, shuffleboard, weight lifting, hockey, swimming, water polo, dancing, curling, snow skiing, tennis, snowboarding, softball, and baseball. Results are robust if restricted only to the five most popular American sports: football, baseball, basketball, soccer, and hockey.

that any use of a phone could rationalize increasing the risk of harming a child. On the other hand, others could argue that children are currently overprotected and could benefit from more skinned knees and broken arms, and therefore smartphones improve welfare.

Although the welfare conclusions are ambiguous, the results certainly raise questions as to other domains where smartphones have an effect. Because injuries occur immediately and can require costly care, one would think parents would be extra diligent in avoiding distractions. But that does not seem to be the case. When the consequences of distractions are farther in the future, such as inattention during key learning opportunities, parents may be even slower to adjust. Future research may include looking at how smartphones affect parental investment in children, student learning in classrooms, and employee productivity.

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Table I: Change in National Annual Emergency Department Visits in NEISS Data by Age Group from 2005 to 2012

Age	2005-06 Average	2011-12 Average	Pct. Increase
0	224,443	239,015	6%
1	352,350	386,498	10%
2	370,600	400,494	8%
3	299,590	332,668	11%
4	260,815	288,085	10%
5	243,403	251,342	3%
6	218,943	222,375	2%
7	212,250	217,986	3%
8	218,486	223,527	2%
9	228,344	245,280	7%
10	256,700	266,439	4%

Notes: The figures are two year averages; i.e. from January 1 of the first year to December 31 of the second.

Table II: Difference-in-differences estimates of the effect of 3G on child injuries

	(1)	(2)	(3)	(4)
3G	0.0919**	0.0391	0.0645**	0.0323
	[0.0446]	[0.0250]	[0.0248]	[0.0249]
Hospital-Specific Trend	None	Linear	Quadratic	Cubic
N	55,445	55,445	55,445	55,445
R^2	0.817	0.843	0.846	0.847

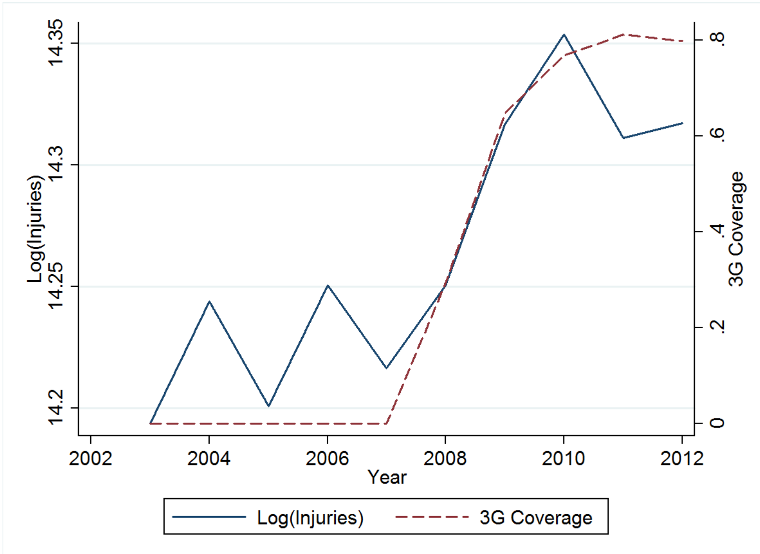
Notes: Standard errors are clustered at the hospital level. ** p<0.05

Table III: Triple difference estimates of the effect of 3G on child injuries

	(1)	(2)	(3)	(4)
3G*(Age<5)	0.0533***	0.0533***	0.0533***	0.0533***
	[0.0175]	[0.0175]	[0.0175]	[0.0175]
3G	0.0386	-0.0071	0.0235	-0.0064
	[0.0403]	[0.0403]	[0.0403]	[0.0403]
Age<5	0.532***	0.532***	0.532***	0.532***
	[0.0363]	[0.0363]	[0.0363]	[0.0363]
Hospital-Specific Trend	None	Linear	Quadratic	Cubic
N	121,979	121,979	121,979	121,979
R^2	0.810	0.820	0.823	0.824

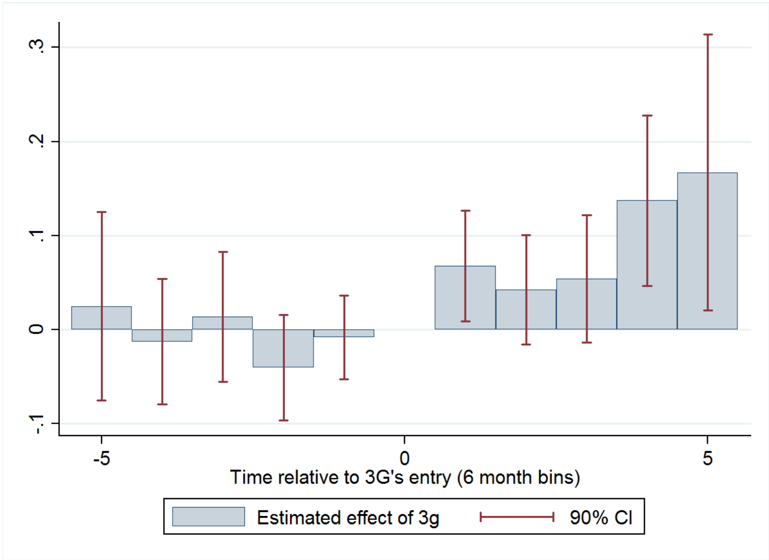
Notes: Standard errors are clustered at the hospital level. *** p<0.01

Figure I: Nonfatal injuries involving consumer products for children under 5 in the US and Hospital 3G coverage, 2001–2012



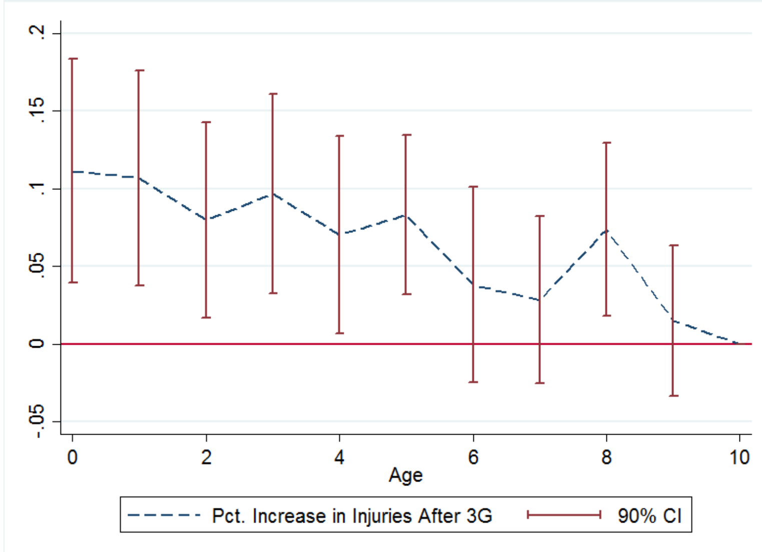
Notes: Injury data comes from weighted counts in the NEISS data. The 3G trend is the share of hospital-monthss in the NEISS data that had 3G in a given year.

Figure II: Checking Pre-existing Trends: The Effect of 3G Over Time on Injuries to Children 5 and Under



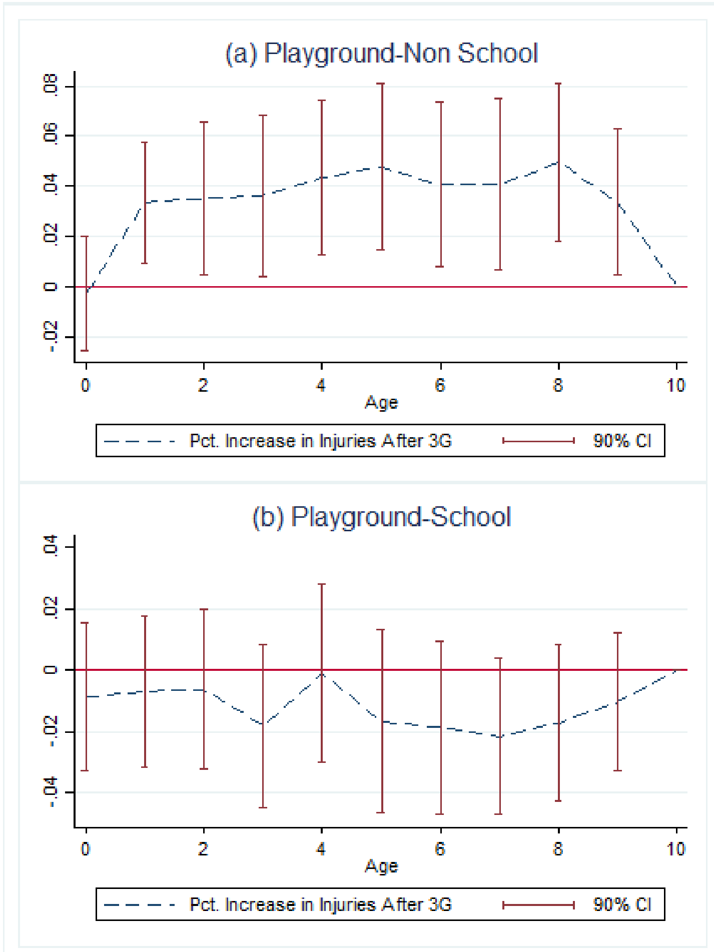
Notes: Coefficients come from a specification similar to Figure III (see notes) except instead of a 3G dummy the regression uses time dummies grouped in six-month bins. Positive values of T correspond to months after the market received 3G, and negative indicate before.

Figure III: Effect of 3G on Hospital ED Visits by Age Group



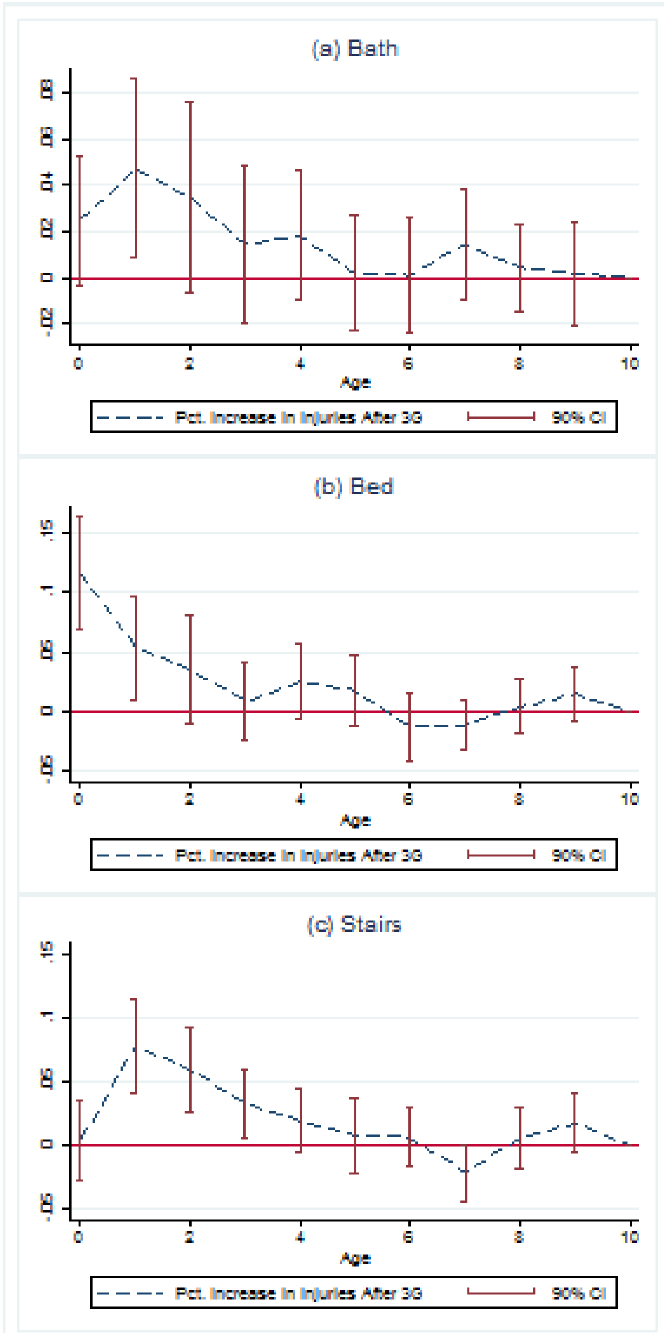
Notes: Coefficients are the treatment effects in a triple difference regression pairing each age group with 10 year olds as the control group. Standard errors are clustered at the hospital level.

Figure IV: Effect of 3G on injuries in activities where risk is the same but parental supervision is different



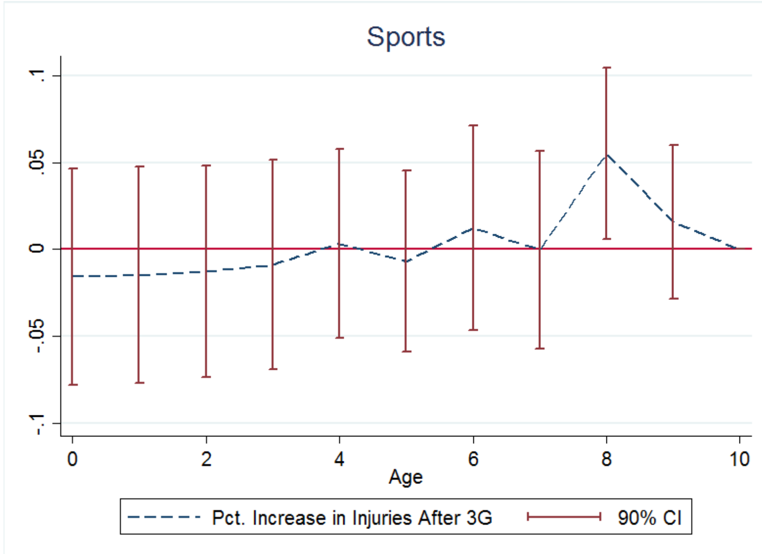
Notes: See Figure III notes for details on the regressions. Each panel uses a subset of the NEISS data where injuries were associated with the activity listed in the heading. In Panel (a), the injuries were recorded as occurring on a playground but not at school, and in Panel (b) the injuries were at school playgrounds.

Figure V: Effect of 3G in activities where participation should not change but supervision might



Notes: See Figure IV notes. Panel (a) includes bath-related injuries, (b) bed-related, and (c) stair-related.

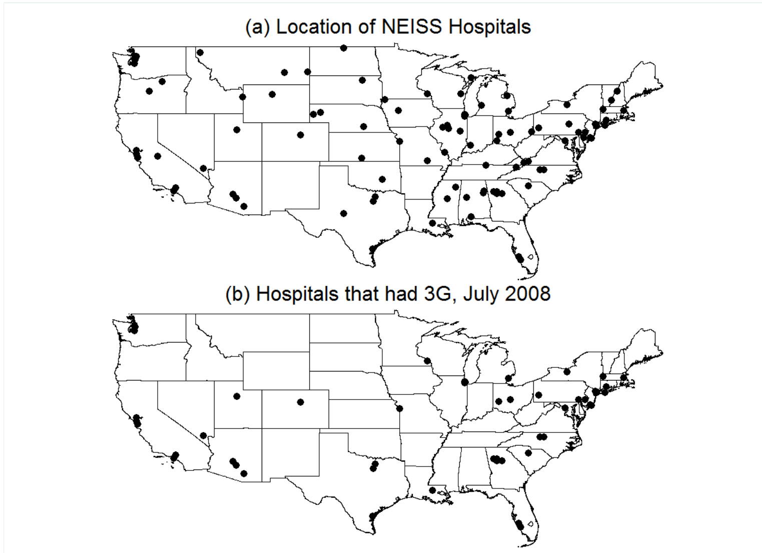
Figure VI: Effect of 3G in activities where parental supervision has little effect



Notes: See Figure IV notes. See footnote 6 on the definition of which sports are included.

7 Appendix

Figure AI: Geographic distribution of NEISS Hospitals and Initial 3G Coverage



Notes: Panel (a) includes the location of all hospitals in the NEISS data. Panel (b) uses the subset of hospitals that had 3G at the time of the iPhone 3G's release.

Table AI: Factors that predict 3G entry in a county where a NEISS hospital is located

	(1)	(2)	(3)	(4)	(5)
log(Population 2010)	1.620***			1.344**	1.380**
	[0.130]			[0.182]	[0.179]
log(Population Density)		1.441***		1.198*	1.204*
		[0.0889]		[0.127]	[0.127]
log(Med. HH Income)			4.066***	1.534	
			[1.900]	[0.751]	
Population Under 5					0.863
					[0.107]

Notes: The reported coefficients are hazard ratios from Cox hazard models. All regressions have 97 observations. Data come from U.S. Census Bureau (2014). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table AII: Difference-in-difference specification check

	IHS	Log(Count+1)	Log(Count)	Count/Mean	Poisson
3G	0.0919**	0.0805**	0.0988***	0.150***	0.0959**
	[0.0446]	[0.0361]	[0.0367]	[0.0513]	[0.0449]
N	55,445	55,445	47,129	55,445	55,445

Notes: Each column except for the last provides a different specification of the dependent variable: IHS is the inverse hyperbolic sine used in the paper; Log(Count+1) adds one to all observations so that a hospital-month-age with zero injuries recorded is included in the regression; Log(Count) takes the log of the observation and omits zeroes; Count/Mean uses the raw count as the dependent variable and the coefficient is then scaled by the sample mean to get a percent increase at the mean. The last column is a poisson regression using the count as the dependent variable. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table AIII: Triple difference specification check

	IHS	Log(Count+1)	Log(Count)	Count/Mean	Poisson
3G*(Age<5)	0.0533***	0.0486***	0.0496**	0.110***	0.0308*
	[0.0175]	[0.0144]	[0.0198]	[0.0257]	[0.0185]
3G	0.0386	0.0318	0.0492	0.0396	0.0652
	[0.0403]	[0.0323]	[0.0330]	[0.0318]	[0.0454]
N	55,445	55,445	47,129	55,445	55,445

Notes: See Table AII notes.

Table AIV: Coefficients for Figures

	Figure III	Figure V		Figure VI			Figure 7
	All	Playground-Non School	Playground-School	Bath	Bed	Stairs	Sports
Age 0	0.111**	-0.00262	-0.00889	0.0247	0.116***	0.00326	-0.0157
	[0.0438]	[0.0140]	[0.0147]	[0.0174]	[0.0285]	[0.0192]	[0.0380]
1	0.107**	0.0337**	-0.00699	0.0475**	0.0535**	0.0783***	-0.0148
	[0.0421]	[0.0148]	[0.0150]	[0.0236]	[0.0266]	[0.0227]	[0.0378]
2	0.0798**	0.0353*	-0.00629	0.0346	0.0347	0.0593***	-0.0127
	[0.0384]	[0.0184]	[0.0159]	[0.0252]	[0.0276]	[0.0205]	[0.0370]
3	0.0965**	0.0362*	-0.0182	0.0146	0.00882	0.0325**	-0.00873
	[0.0390]	[0.0197]	[0.0161]	[0.0208]	[0.0202]	[0.0159]	[0.0367]
4	0.0702*	0.0435**	-0.00108	0.0181	0.0254	0.0194	0.00337
	[0.0386]	[0.0188]	[0.0177]	[0.0173]	[0.0197]	[0.0156]	[0.0330]
5	0.0830***	0.0477**	-0.0168	0.00198	0.0179	0.00741	-0.00676
	[0.0313]	[0.0202]	[0.0182]	[0.0151]	[0.0183]	[0.0182]	[0.0318]
6	0.0381	0.0408**	-0.0188	0.00101	-0.0132	0.00652	0.0124
	[0.0382]	[0.0200]	[0.0173]	[0.0156]	[0.0173]	[0.0145]	[0.0357]
7	0.0284	0.0409*	-0.0217	0.0144	-0.0114	-0.0214	-0.000204
	[0.0327]	[0.0207]	[0.0156]	[0.0146]	[0.0130]	[0.0136]	[0.0347]
8	0.0737**	0.0495**	-0.0172	0.00419	0.00394	0.00524	0.0551*
	[0.0339]	[0.0191]	[0.0155]	[0.0118]	[0.0137]	[0.0146]	[0.0300]
9	0.0149	0.0339*	-0.0104	0.00194	0.0149	0.0176	0.0158
	[0.0294]	[0.0176]	[0.0137]	[0.0137]	[0.0135]	[0.0142]	[0.0269]

Notes: Coefficients for all figures in the paper. *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$.