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Peltzman on Ice: Evidence on Compensating Behavior Using a Natural Experiment from Ice Hockey.

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August 4, 2011

Abstract

We provide evidence of the Peltzman effect by tracking the professional path of each hockey player that ended up in the National Hockey League from 2001 to 2006. We take advantage of the fact that visor use has not always been compulsory throughout a player's career, which allows us to compare the change in behavior of users and non-users of visors when they are forced to use them. We find that whereas the average penalty minutes per game is 0.8, visors cause a substantial increase of 0.2 penalty minutes per game. Players become more aggressive when forced to wear a visor, partially offsetting its protective effect and creating potential spillover effects to other players.

JEL: K32, K23, H40

Keywords: Peltzman Effect, Ice Hockey, Compensating Behavior

The effectiveness of safety and protective devices, such as seat belts, airbags, helmets, pads, safety caps, and the like, hinges on the critical assumption that the behavior of individuals remains constant regardless of the use of such equipment and devices. However, it is uncertain whether this is the actual case, or whether individuals will change or adapt their behavior in response to any extra protection in such a way that the intended effect of the protective device will end up diluted or even lost. Theoretically, protective equipment will reduce the price of risky behavior and people will respond rationally by demanding more risk. The possible existence of this compensating behavior was first proposed by Peltzman (1975) and is commonly referred to as the "Peltzman effect". This issue has long been the

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subject of vigorous debate in economics. In fact, the Peltzman effect has strong implications which are not only circumscribed to local applications, but rather are at the heart of the debate related to government intervention in the economy as this effect is, in essence, directly related to moral hazard issues.

While many researchers have long tried to show the existence, or lack thereof, of the Peltzman effect, the existing empirical evidence remains inconclusive. Initial attempts on providing evidence on compensating behavior focus on laws requiring the use of seat belts in the United States and elsewhere. Since "driving intensity" of the individual cannot be measured when seat belt use is required, researchers have studied fatalities among car occupants and fatalities among non-occupants, as the latter are expected to be affected only indirectly by the less careful driving hypothetically caused by seat belts. Peltzman (1975),Crandall and Graham (1984), Harvey and Durbin (1986), Asch, Levy, Shea and Bodenhorn (1991), Garbacz (1992), Risa (1994), Loeb (1995), among others use time series and cross-section data and find some evidence on seatbelt laws and compensating behavior. Recently, Cohen and Einav (2003) use an instrumental variable approach instrumenting seatbelt use with changes in states regulations, and find that seat belt use decreases occupants fatalities, but has no significant effect on non-occupant deaths. This last finding does not support the existence of compensating behavior. [1]

The evidence on compensating behavior using seat belt data presents many shortcomings. The most problematic issue is that most of the studies focus on aggregate, imprecise and indirect measures of behavior, so that a truly clean exercise cannot be observed as it is difficult to control for aggregate time-varying variables. Moreover, the available evidence is unable to identify which specific individuals are 'treated' and which ones are not. As a consequence, the estimates produced may only be interpreted as average effects, while compensating behavior only arises locally from those who were actually treated (i.e., those for whom the regulation was binding). Another issue is related to the fact that compliance with laws may not be perfect, whereas usage can be very difficult to measure. Furthermore, seat belt use may be endogenous. Studies that apply instrumental variable procedures to address this problem tend to use changes in regulation as an instrument, but these laws may be associated with additional enforcement or they may be accompanied by safety campaigns, which conceivably may reduce accidents and all type of deaths through different channels. If this is the case, such seat belt rules may not satisfy the exclusion restriction and may be invalid instruments.[2] Finally, empirical approaches that use time series analysis cannot

[1] On a related paper, Cohen and Dehejia (2004) find moral hazard costs in terms of traffic fatalities when focusing on compulsory insurance laws in the United States. Furthermore, they also find that reductions in accident liability produced by no-fault liability laws have led to an increase in traffic fatalities.

[2] Additionally, some types of enforcement may increase the cost of other infractions. For instance, in

control for national shocks; whereas estimates using cross sections of locations may be biased by unobserved state characteristics. Overall, all these issues make it difficult to provide credible evidence in favor or against the existence and extent of compensating behavior.

Given the limitations of the car seat belt literature, researchers have looked for other empirical scenarios in which to test for compensating behavior. Car racing has provided another fertile area to do so, as compliance is automatic and it does not require further enforcement, which helps avoid some of the issues that affect the seat belt literature. However, the resulting empirical evidence still remains unpersuasive. For instance, Pope and Tollison (2010) find that the introduction of a neck and head security device in NASCAR was associated with an increase in caution laps, their proxy of driving intensity. However, their exercise faces many empirical difficulties. When the device was introduced, all racers but one were already using it. This suggests that racers were already responding to increased risks of accidents. Also, their findings may be capturing upward trends in the number of accidents due to the existence of faster cars. In a related paper, Sobel and Nesbit (2007) find that the probability of injury is negatively related to accidents, measured again by caution laps. While they use past injury rates as a proxy, this variable is unconvincing as it may be endogenous since it may be related to changes in NASCAR that may affect accidents, in particular, increases in car speed. Furthermore, it might also be the case that after a few years with high injury rates, NASCAR might have decided to fine-tune the rules in order to decrease accidents for subsequent seasons. In short, it is not clear whether drivers may be responding to the change in the probability of injury caused by the exogenous introduction of some protective device, as suggested by the Peltzman effect.

In this paper we take a different route in order to test for the presence of the Peltzman effect or compensating behavior. We do so by employing data from professional ice hockey which allows us to overcome most, if not all, of the existing limitations of previous empirical studies. In particular, we follow the career path of each hockey player that ended up in the National Hockey League (NHL) from 2001 to 2006 and take advantage of the fact that visor use has not always been compulsory throughout a player's career. While currently the use of hockey visors is not compulsory in the NHL, the top league in the world, its use has been made mandatory in different feeder leagues around the world at different points in time. This allows us to compare the behavior of individual NHL players who use a visor (control group) and players who do not do so (treatment group) with respect to their behavior in other leagues in which they were forced to wear a visor. Crucially, we are able to follow the

locations with secondary enforcement, drivers may be pulled over for committing unrelated infractions, and receive an additional fine if they are found not wearing seat belts. This may increase the cost of committing an infraction for those not wearing one.

playing path of each individual player and thus we are able to control for player fixed effects, which typically may be a major source of bias. The required identification assumption is that the behavior of players that use visors in the NHL and those who do not will follow similar trends as they change leagues, ignoring the effect of visors. These institutional features of professional ice hockey allow us to set an empirical research design that is analogous to a difference-in-differences approach.

Unlike most of the existing empirical evidence produced to this day, we do find that there is significant compensating or offsetting behavior among hockey players when forced to wear visors. We estimate that whereas the average penalty minutes per game is 0.8, visors cause a substantial increase of 0.2 penalty minutes per game. In fact, we find that players that wear visors play more aggressively, partially offsetting its protective effect and creating potential spillover effects to other players. We also find that the use of a visor does not significantly affect performance, measured by goals and assists per game. We conduct different exercises to check the robustness of our results. In particular, we provide evidence suggesting that our estimates are not driven by differences in adaptation as players change leagues; or equivalently, suggesting that the equal trends assumption holds. These results are meaningful as they imply that, contrary to common belief, mandatory use of visors do not raise consciousness about safety. On the contrary, visors decrease the cost of unsafe skating, which increases risk-taking and aggressiveness on the ice. In fact, some hockey commentators have argued that skating behind a visor provides players with some sense of invincibility that may actually lead them to skate more aggressively and recklessly. Additionally, players may become reckless with their sticks if everyone else has protection (see Schwarz (2001) for an example of these arguments in Lacrosse).

This paper is organized as follows. In section 1 we explain in detail the natural experiment that ice hockey allows us to take advantage of, which in turn provides a good setting to test for compensating behavior. In section 2 we describe the data and our empirical strategy. In sections 3,4 and 5 we present and discuss our empirical results. Finally, in section 6 we summarize and conclude.

1 Visors in Ice Hockey as a Natural Experiment

An on-going controversy in the National Hockey League (NHL), the top professional ice hockey in the world, has to do with the fact that the use of visors in helmets is optional. Visors are strong transparent fiber shields designed to protect a players' eyes and face. During our period of study, from 2001 to 2006, only a third of hockey players chose to wear visors. This number appears to be exceedingly low if one takes into consideration the fact that year after year there are horrifying and high profile cases of players that become gravely injured for the lack of use of a visor. There are two reasons commonly associated to this behavior. First hockey players believe that their performance may be compromised, as sweat and dirt in the visor may interfere with the player's vision. Second, ice hockey is commonly associated with a macho subculture in which it is important to send the signal that one is courageous enough so much so that wearing a visor may send a detrimental signal to other players. The extent to which the lack of visor use has to do with a perceived reduction in performance or to peer pressure is unclear. Interestingly, it may be argued that visor use can give a player a sense of additional security that, instead of reduce his performance, may actually help him play better as, for instance, wearing a visor may allow him to manage risk better.

Ice hockey provides an ideal natural experiment to test for compensating or offsetting behavior for two reasons. First, a particular feature that affects all players that end up in the NHL has to do with the fact that before, and sometimes after, playing in the NHL- where visor use is optional- players participate in North American or European feeder leagues which had mandatory visors by the time they were playing.[3] As a consequence, we are able to track individual playing careers for the different cohorts of NHL players during the 2001-2006 seasons and retrieve their play statistics for seasons played at leagues which mandated them to use a visor during their time they played there. Importantly, these changes in visor regulation give us an exogenous source of variation in visor use. Players must wear a visor for exogenous reasons unrelated to their characteristics, since conditional on playing in the NHL, every single player had at least one season in a feeder league that mandated him to wear a visor.

This feature of ice hockey allow us to set a research design similar to a difference-in-differences. We are able to observe which players stop using visors once they get into the NHL as well as which players use one regularly during the 2001-2006 seasons. Non-users are the treated players, since they are the ones affected by mandatory visor rules in other leagues. Regular users are the control group, since they always use some kind of face protection regardless of league-specific rules. The latter are not affected, or at least are less likely to be affected, by mandatory visor rules in other leagues. The 2001-2006 seasons in the NHL are those for which the treatment is not active. The seasons played in leagues with compulsory visor use are those for which the treatment is active. Moreover, we can control for player fixed effects as we are able to track the same set of players throughout their entire careers.

[3] Some of these feeder leagues changed their visor regulation during our period of study. Most notably, the AHL, CHL and EHL, some of the most important minor pro leagues mandated visors between 2003 and 2006. For our purposes, we are only interested in the seasons in which visors were already mandatory.

The comparison with players that are not affected by mandatory visor rules, but which are similar in other fixed characteristics allows us to control for league specific effects under the premise that similar players that only differ in their use of a visor in the NHL follow similar trends when playing in different leagues, ignoring the effect of visors. If this is the case, any difference in performance between both group of players in leagues with mandatory visors with respect to the NHL must be caused by the exogenous imposition of visors.

Another advantage of ice hockey is that it allows us to capture changes in behavior. We are able to measure individual aggressive or reckless behavior with precision by looking at the penalties committed in games. A penalty committed on the ice is a punishment for behavior that is deemed inappropriate. It is enforced by detaining the offending player within a so-called penalty box for a set number of minutes, during which, the player is not allowed to play. The offending team usually cannot replace the player on the ice, leaving them with one player short during the penalty period. The statistic used to track penalties is called "Penalty Infraction Minutes" (PIM) or "Penalties in Minutes" which represents the total amount of penalties measured in minutes accrued by a player during a period. This statistic is a very good proxy to measure potential compensating behavior of the player during a season, as this variable captures a very detailed and standardized set of behavior that is directly linked with recklessness, risky behavior, and aggressiveness of the player. Typical behavior that is penalized in the form of either a "minor" penalty or a "major" penalty which are assigned a specific and pre-defined time of penalty in minutes include charging, boarding, elbowing, kicking, head shots, attempt to injure other players, fighting, cross-checking, abuse to officials, and several others that are clearly spelled out in the NHL rules, as described in Appendix 1.[4]

2 Data and empirical strategy

2.1 Data

Our sample consists of data containing detailed characteristics of 763 professional hockey players who were active in the NHL between the 2001 and 2006 regular seasons. For each of

[4]Contrary to what one would expect, head injuries are not the most adequate variable to capture compensating or offsetting behavior. On the one hand, reckless or more aggressive behavior is not reflected in such injuries only, but in a more general pattern of behavior, precisely the one captured by penalties in minutes. On the other hand, because of this macho subculture that permeates ice hockey, injuries in general, and head injuries (e.g., concussions) in particular, tend to be concealed by players and thus, accurate statistics on actual head injuries, beyond the obvious ones (e.g., eye injuries), which may be biased, are difficult to obtain. Furthermore, a current controversy in the NHL is directly related to the fact that frequent times head shots have produced injuries that have not been penalized by referees as they were deemed legal.

these players we observe if they use a visor which, as mentioned above, is not mandatory in this league. We find that 250 of the players in our sample, nearly 30% of the total number of players, used a visor regularly while 513 players never did. For each player we collect a broad set of personal characteristics including date of birth, birthplace, weight, height, the year of his first season in the NHL, position, experience, and several others.

The key feature in this paper is that we are able to follow these players throughout their entire playing career prior to their arrival to the NHL and thus we can collect their corresponding playing statistics for the seasons in which they played in leagues that did require a mandatory use of visors, if this was the case. These leagues include European elite leagues, minor pro leagues, Canadian junior leagues, U.S. junior leagues, and college leagues. Appendix 2 shows the corresponding regulation regarding visor use for the different leagues included in this paper and the year at which mandatory visor use was first introduced.

In particular, for each of these players we collect a set of play statistics, including goals, assists, games played, and penalty in minutes for each of the following seasons, provided that the player took part in any of them: i) the 2001, 2002, 2003, 2005 and 2006 NHL seasons, which we identify by a dummy $M_l = 0$ and label "NHL seasons", and ii) any other season during the career of the player in a league other than the NHL that mandated him to use a visor, which we identify by a dummy $M_l = 1$ and label "other leagues' seasons".[5] It should be noted that we exclude seasons in leagues other than the NHL, for which either the player is not forced to wear a visor, or we do not have enough information to attribute visor use. In fact, while it may be the case that a league may have compulsory rules related to visor use, sometimes grandfather clauses exempting some players are applied when such regulation is first introduced. We take a conservative approach and exclude any data on players that belong to an exempted group for a particular season. Out of the 763 players in our sample, there were 716 who played at least one season in leagues other than the NHL in which they were required to wear a visor. We can safely assume that players used a visor during these seasons for exogenous reasons, given that compliance is automatic.

Our unit of observation is player-per-season. For the players who used visors in the NHL during 2001-2006, we are able to obtain data on 1012 player-per-seasons in the NHL, and 1139 player-per-seasons in the other leagues. On the other hand, for the players who did not use visors in the NHL, we observe 2315 player-per-seasons in the NHL, and 2620 player-per-seasons in other leagues. Table 1 summarizes the main variables of interest used in this study. The table is divided in two columns. The first column summarizes the data for those players who use a visor during their NHL career. The second column summarizes the data for players who did not. We show the statistics for the NHL 2001-2006 seasons

[5] We do not have data for 2004 as during this year there was a league lockout.

in one column, and the statistics for seasons in other leagues in the other. The table also summarizes the main characteristics of the players.

As shown in the table, players who used visors in non-NHL leagues scored more goals, gave more assists and were assessed less penalty minutes than players who never used visors. Since all players had to use a visor or worse, full face protection in these leagues, these differences cannot be attributed to the protective equipment, and suggest that players who use a visor in the NHL are systematically different from those who do not use one. Our data suggest that more aggressive players inherently do not use visors while high-performing players inherently do so. This simple observation implies that any cross sectional estimate of the effect of visors would be biased as users are a non random sample. The table also shows that players who use visors are more likely to come from Europe and be less tall and heavy, although there are no significant differences in their age or experience in the NHL.

As mentioned above, the key dependent variable used in this study is penalty in minutes per game. As mentioned above, this variable is defined as the total amount of time that a player was detained in a penalty box for rule transgressions on the ice rink, which are typically linked with aggressiveness and recklessness, as shown in Appendix 1. We normalize this measure by dividing it by the total number of games played. At the heart of our estimate lies the fact that, although users of visors are inherently less aggressive (1.11 penalty minutes as compared with 1.59 penalty minutes received by non-users in leagues with mandatory visors), they become more aggressive relative to non-users when these players remove their visors (penalty minutes decrease from 1.11 to 0.63 for users and from 1.59 to 0.96 for non-users in their NHL seasons). Thus, as long as the equal trends assumption holds, visor use must account for the 0.14 additional penalty minutes per game received by users in their NHL seasons. This simple calculation shows the importance of gathering statistics for feeder or European leagues as this effect would be impossible to find in cross sectional data.

2.2 Empirical Strategy

As the above discussion suggests, cross-sectional estimates of the effect of visors on other variables are likely to be biased, since players with particular characteristics that cannot be observed use visors. Exploiting the panel dimension of the NHL seasons cannot do the job, either. This is because only a limited number of players changed their visor status during this period, thus limiting variation. Moreover, these changes may occur for endogenous reasons. For instance, a player may realize that NHL hockey is more dangerous than he initially thought; he may choose to wear a visor during the subsequent season and he could choose to play more cautiously. Thus, variation in visor usage within the NHL may be endogenous, and estimates with panel data for the NHL seasons only may be biased.

In fact, one would like to compel players who are not using a visor to use one in order to compare their performance and statistics, relative to other players who did not change their visor status. This is exactly what occurs when players move from (or to) minor, junior, college or European leagues, where visors are mandatory, to the NHL, where such use is optional. This variation in league regulations regarding visor use is the key to this paper. It provides a large exogenous source of variation in visor use that allows us to overcome problems typically confronted in previous research and thus help us estimate a causal impact. It is exogenous because players are forced to use a visor, and this has nothing to do with their unobservable characteristics since all the players have to go through one or more of these leagues before playing in the NHL.

The basic equation we estimate is given by

$$y_{isl} = \alpha_i + \theta_l + \kappa_s + \beta NV_i \times M_l + \varepsilon_{isl}. \quad (1)$$

Here y_{isl} is any outcome for player i during season s at league l. NV_i is a dummy that takes the value 1 if the player never used a visor during the 2001-2006 NHL regular seasons, and 0 otherwise. [6] M_l is another dummy that takes the value 0 for the NHL seasons between 2001 and 2006 and 1 for seasons played in other leagues where the player was forced to use a visor at that time. Strictly speaking, our notation should include M with a player and a season subscript, as many leagues changed their visor rules during the years we observe and some leagues had grandfather clauses. However, we use M_l as a constant for a given league in our sample since, as explained above, we dropped the observations in non-NHL leagues in which the player was not forced to wear a visor. We estimate the model with a full set of player, league and season fixed effects (α_i, θ_l and κ_s). In some specifications we drop the fixed effects and replace them for observable player characteristics such as age, birth year, height, weight, birth place, other individual characteristics, and the dummy NV_i, which captures common characteristics of players not using a visor. Finally, ε_{isl} is the error term.

The OLS coefficient β of the interaction term $NV_i \times M_l$ consistently estimates the effect of using a visor on the outcome y, as long as players who use a visor and players who do not use a visor (but are similar in all other dimensions), follow similar trends when moving from (or to) non-NHL leagues to (or from) the NHL. At the heart of our identification assumption we have a typical difference-in-difference approach. The treated group consists of players who did not use a visor in the NHL, and hence are likely to be affected by a rule mandating it in other leagues, while the control group consists of players who used visors in the NHL and are less likely to be affected by such a rule. In this setting instead of pre-treatment and

[6] Our results are robust to different coding for this variable. In particular, we do not use changes in visor use for a particular player during his NHL seasons as this may be endogenous.

post-treatment periods, we have leagues in which the treatment is active that is, seasons in non-NHL leagues, as well as leagues in which the treatment is not active, namely the NHL.

Our identification assumption is apparent once one notices that β is given by (ignoring those players who changed their visor during 2001-2006, fixed effects and other controls)

$$\widehat{\beta} = \begin{array}{l} \left[\widehat{E}(y|M=1, NV=1, V=1) - \widehat{E}(y|M=0, NV=1, V=0)\right] - \\ \left[E(y|M=1, NV=0, V=1) - E(y|M=0, NV=0, V=1)\right]. \end{array} \quad (2)$$

Here V is a dummy equal to 1 if the player is using a visor and 0 otherwise. Thus, β estimates the local effect of a visor, on those who do not use one, $E(y|M=0, NV=1, V=1) - E(y|M=0, NV=1, V=0)$, as long as

$$\begin{array}{c} \widehat{E}(y|M=1, NV=1, V=1) - \widehat{E}(y|M=0, NV=1, V=1) = \\ \widehat{E}(y|M=1, NV=0, V=1) - \widehat{E}(y|M=0, NV=0, V=1), \end{array} \quad (3)$$

which is an equal trends assumption. Given the importance of this assumption, we will discuss it thoroughly and present some evidence showing that it holds.

While player fixed effects reduce the bias arising from the fact that users and non-users are inherently different in many fixed characteristics, they are not sufficient to guarantee identification. The interaction may be capturing heterogeneous effects of playing in the NHL, or differences in adaptation between users of visors and non users of visors. However, fixed effects make the equal trends assumption more plausible, since we just have to assume that similar players who are only different because one wears a visor at the NHL and the other does not, follow similar trends in non-NHL leagues relative to the NHL, ignoring the effect of visors. In the above equation, league effects capture common differences between groups as they move to play in different leagues, while season effects capture common trends in hockey affecting all players. We also allow for differential trends by league in some specifications. The last issue when estimating equation 1, is that the error term may be serially correlated. Thus we clustered errors by player, since league and season fixed effects will partially remove other types of serial correlation, and we do not have enough clusters at that aggregation level.

3 Results

Table 2 shows different estimates of model 1. The first row shows our estimates for the effect of a visor, which is the coefficient of the interaction term, β. In the first four columns we exclude player fixed effects and replace them with observable characteristics and a dummy for players who do not use visors, NV_i, which captures common characteristics among this group of players. In these columns, the coefficient of this dummy, reported in the row "No-visor type", can be interpreted as a measure of self-selection bias arising from group differences. The last two columns include player fixed effects. We include season and league effects except for the first column; player characteristics including age, experience, birth year, birth region and others; and in column 4 and 6 we allow for different time trends in each major category of leagues (NHL, minor pro, junior U.S., junior Canada, major junior, college and European leagues). All models have standard errors clustered at the player level.

As shown in the table, we estimate that visors increase penalty in minutes per game by 0.2. This is a large effect, especially when taking into account that the average penalty in minutes per game in the NHL was about 0.8 during our period of study. An average player plays about 50 games in the regular season, so using a visor causes about 5 penalty minutes per season. This is a local effect that tells us how penalty minutes would change for a player who does not use a visor if he is suddenly forced to use one. Our estimates suggest that there is compensatory behavior as players feel more protected. The cost of aggressive skating falls and players respond by playing more recklessly and aggressively, as implied by the increase in penalty minutes. This effect is significant at the 1% level, and it is fairly stable across specifications.

When player fixed effects are not included, the dummy for players who do not use visors in the NHL is positive and statistically significant. This dummy shows that there is considerable selection bias, and players who do not use a visor in the NHL are less cautious to start with. This is precisely why it is so important to use minor league statistics as part of our natural experiment as pure cross-sectional estimates will be biased.[7]

As mentioned above, a concern is that players using visors in the NHL may adapt differently compared to players who do not use them, or performance and behavior may evolve in a different way for visor users and non-users as they move from other leagues to the NHL. It could also be the case that different penalization standards or league characteristics may have an heterogeneous effect on both groups of players, and thus the equal trends assumption may not hold. In order to provide some evidence in support of the equal trends assumption, which is the key identification assumption required for our results, we conduct some robustness exercises in Table 3.

The first two columns in Table 3 exploit the NHL lost season of 2004 due to contract issues. During this year, there was no regular season in the NHL and several players migrated to professional leagues in Europe or minor pro leagues in North America which already had instituted mandatory visor rules. This specification is helpful because players were already

[7] In fact, cross sectional estimates using the NHL seasons as a pooled sample show a negative relationship between using a visor and penalty minutes per game, but since players who use a visor are more cautious to start with, this result is due to pure selection bias.

used to NHL hockey and were forced to change leagues because of an exogenous event. Overall, these leagues are not very different from the NHL, especially given the fact that the intention of the players was to keep in shape. In these two columns we estimate model 1, but we redefine the dummy variable M_l. In this case it takes the value of 1 when it corresponds to a player in his 2004 season, if he played in a professional league with mandatory visors. On the other hand, it is set to 0 for the NHL regular seasons during 2001, 2002, 2003, 2005 and 2006. As shown in the table, these estimates suggest that using a visor increases penalty minutes per game in about 0.5, which is considerably larger than our baseline estimates. In this case, the identification assumption becomes more plausible because we only need players to follow equal trends as they moved to other professional leagues during the NHL lockout.

In the next two columns of Table 3 we exclude college, U.S. junior and European leagues and keep Canadian feeder leagues only, which are considered to be the closest to the NHL. Our estimates are not affected by restricting our sample and remain positive and statistically significant. This result is important because it shows that our estimates are not driven by differences in adaptation by group to Canadian hockey, or its rules, which may differ from the European ones or College rules. These regressions also show that the effect is not driven by college leagues, where full face protection is required.

The above table also rules out other potential confounding factors. For instance, it may be argued that leagues other than the NHL may apply different penalization standards that may impact more aggressive players differently, thus biasing our original estimate. However, European leagues tend to be regarded as less tolerant towards violence than the NHL; while Canadian feeder leagues are regarded as more tolerant towards violence than the NHL. Thus, the fact that we get a positive and significant effect of visor use on penalty minutes using both comparison groups, suggest that our main result is not driven by systematic differences in penalization standards across leagues. That is, our results hold either when we compare NHL seasons to seasons in leagues that tolerate less violence (European leagues in columns 1-2), or when we compare NHL seasons to seasons in leagues which tolerate more violence (Canadian feeder leagues in columns 3-4).[8]

In the last two columns of Table 3 we control for heterogeneous adaptation to different leagues using a parametric approach. We rank the different categories of leagues according to how many of their players end up becoming NHL players. This ranking starts with the NHL itself and suggests that minor pro leagues are more alike the NHL. We then have the European leagues followed by major junior and junior leagues, and finally college leagues.

[8] According to the conventional wisdom Canadian feeder leagues appear to tolerate more violence than the NHL. At the NHL level, players are careful not to take penalties because they do not wish to put their team shorthanded, and they are under pressure from coaches not to risk injury (i.e. the cost of a penalty is higher at the NHL level). At lower levels, there is much more fighting and thus more penalty minutes.

These two columns present estimates in which we control for an interaction term between the rank number of a league and a dummy that identifies players who never used visors in the NHL seasons. This approach is analogous to what is done in a typical difference-in-difference analysis when including group specific trends as a robustness check. We find that this variable is not statistically significant, but its negative sign suggests that players who did not use a visor in the NHL, and were inherently more aggressive, became more aggressive as they moved from lower ranking leagues to the NHL or other professional leagues, ignoring the effect of visors. This finding implies that, if anything, this differential trend would create a downward bias in our estimate, making our baseline estimate a lower bound. Consistent with this, our estimates are larger than in our base case scenario once we control for the difference in trends.

As shown in the descriptive statistics of our sample, visor use is less prominent among Europeans, heavier and taller players and is also correlated to a player position. Thus, it could be the case that players who differ in these characteristics may adapt in distinct ways to the NHL. This possibility would violate the equal trends assumption and bias our estimates. In order to rule this out, we include interactions of the "treatment leagues" dummy, M_l, with observable players' characteristics, which control for potential differences in adaptation to the NHL between visor users and non-users. The first column in Table 4 shows estimates controlling for different adaptation by birth and year of first NHL season; that is, a full set of interactions between M_l and years of birth and first NHL season. The second column in Table 4 shows estimates controlling for different adaptation by position; that is, a full set of interactions between M_l and position dummies. The third column in Table 4 shows estimates controlling for different adaptation by weight and height; that is, a full set of interactions between M_l and these variables. The fourth column in Table 4 shows estimates controlling for different adaptation by birthplace; that is, a full set of interactions between M_l and dummies for players born in Canada, the U.S. or Europe. In the last column we include all controls simultaneously. In all the specifications our estimates remain positive and significant, but the coefficients become smaller. These results suggest that our results are not driven by differential adaptation to the NHL among players with different observable characteristics related to visor use.

We also conduct a series of placebo tests in order to verify the equal trends assumption in a non-parametric way. Table 5, presents different placebos in which we drop the NHL seasons, and explore whether there are significant differences between visor users and non-users, as they move from-and-to leagues that require mandatory use of visors. If there are no differences in adaptation and the equal trends assumption holds, one would not expect any significant difference between both types of players, as visor regulation is the same in these leagues where players had to use a visor or full face protection. To do this, we remove the NHL seasons and estimate the interaction of NV_i with dummies for different subsets of leagues. In the first two columns we use a dummy which takes the value 0 for seasons in minor pro leagues and 1 otherwise. The negative and not significant coefficient suggest that, consistent with the equal trends assumption, there are no differences between visor users and non-users as they move from European or junior leagues to the minor pro leagues. In the third and fourth column we estimate the same model but excluding European leagues, which are also professional leagues. The results are very similar and suggest that there is no significant difference between visor users and non-users as they move from junior or college leagues to minor pro leagues.

In the last two columns in Table 5, we perform a similar exercise but we use a dummy which takes the value of 0 for European leagues and 1 for junior and college leagues. Again, we do not find a significant difference between visor users and non-users behavior as they move from junior or college leagues to European leagues. Our evidence suggests that the equal trends assumption holds, at least for movements of players between these feeder and European leagues. The signs of the placebo tests suggest that, if anything, players who do not use visors and are more inherently aggressive, become even more so as they move to more competitive leagues, relative to players who use visors. Thus our baseline estimates are biased against our hypothesis and hence represent a lower bound.[9]

Our results also hold when including team effects in order to control for sorting by team, as Canadian-based teams may tend to be more aggressive than US-based teams. They also hold when excluding college seasons, and to different variable definition of the no-visor dummy, NV. These results are available upon request.

4 Do Visor Use Reduce Performance?

We are also able to test whether the use of visors has an impact on performance, measured by assists and goals per game. Table 6 shows our estimates when these variables are used as outcomes. We find no particularly robust or statistically significant effect of wearing a visor on performance. Player complaints about the use of visors reducing their performance because they get fogged or are uncomfortable appear to be somewhat exaggerated. As some

[9]In a related exercise not reported here, we exclude the NHL seasons and include a full set of interactions between dummies for each type of league and the dummy for players that do not use visors. Players who do not use a visor do not play any differently in any two of these leagues. That is, the coefficients of these interactions are not statistically different from one another. Results are available upon request. The placebo test also show that our strategy is not capturing differential effects of penalization standards, since we would get significant estimates if users and non-users were affected differently by league specific standards.

have argued, given that players skate with a visor during most of their careers, it should not be too difficult to get used to it in the NHL.[10] Interestingly, this result is reassuring for our identification strategy for two reasons: First, as it is consistent with the fact that players do not play more aggressively in order to compensate for any potential poorer performance resulting from visor use. Second, this result indirectly suggests there are no differences in adaptation by group to the NHL, at least in performance, which indirectly suggests that the equal trends assumption holds.

The corresponding coefficients on the dummy that accounts for players who do not use visors imply that these players would decrease performance independently of visor use. There is considerable self-selection bias in this dimension as well, with the better players using visors. Any simple OLS cross-section results that show that players who use visors perform better are not taking into consideration the endogeneity and upward bias implied by this result.

5 Discussion

The logical question which immediately follows from the results presented above is why do players skate without a visor in the NHL? We have shown that performance is not affected and that they can play more safely, while being able to take more risks and, perhaps, suffering less injuries. It seems that the decision to use a visor is not determined by a trade-off between safety and performance. Instead, our evidence suggests that using a visor may be related to a predominant culture in ice hockey by which players who wear a visor are considered weak or less manly whereas those who do not use visors are considered tough. In fact, according to the conventional wisdom, there is a negative stigma associated with using visors in the NHL that is responsible for the low rate of use. Our results appear to confirm such conventional wisdom.

Players who do not use visors in the NHL are inherently more aggressive or more reckless than those who do wear a visor. This suggests that skating without a visor may be acting as signal for toughness, while using one signals lack of it. Visors may be a credible signal because players that are not tough enough prefer protection, while tough players may be willing to sacrifice some of it. Not using a visor is a credible signal because only tough players can afford it. Moreover, it may be the case that players may benefit from others thinking that they are tough (e.g., either the crowd or other players). Examples of this sort of behavior can be also found in biology, where it is known as the “Handicap effect” and is

[10] However, it may be the case that while visors may reduce vision and performance, they may also allow players to take on additional risks, and both effects could cancel each other out.

also analogous to signaling theory in economics (see Zahavi (1975)).

Our data does not support an alternative explanation in which players do not use a visor because it reduces their relative performance, and as a consequence there are players skating without one because they lack a commitment technology to enforce its use. This insight is similar to the one presented by Schelling about helmet use in hockey (see Schelling (1978)). In fact, when players are polled by the players' union (NHLPA) about use of visors, they have repeatedly rejected the idea of making their use mandatory in the NHL.

Either in the Zahavi signaling model or the Schelling model, it may be Pareto improving to mandate visors. In the signaling case, mandating visors make their use uninformative about a players' type, or removes the stigma associated with it. Players would no longer use a visor because they are not tough enough, but because they have to. In the Schelling model it makes all players safer without altering their relative performance. There may be other situations in which an intervention is justified. For instance, this would be the case when government has better information than the agents, or if the agents systematically underestimate risk.

Many researchers have focused their attention on determining if compensatory behavior offsets all the gains from more protection on the number of injuries or fatalities. From a welfare perspective this is irrelevant. If an agent (e.g., a hockey player) can increase his safety, his utility will increase even if he exhibits compensatory behavior, a direct consequence of the envelope theorem. It may be the case that if visors became mandatory, players who did not use them because of the stigma they carried, would start playing more aggressively, as our findings suggest. As a consequence, the number of injuries to these players may increase, despite the greater protection, but the direct effect of the visor will always dominate the indirect effect of the offsetting behavior. Thus, players may become more frequently injured, but would also play more aggressively, skate faster, and perhaps move better in the ice. Their "utility" as players would increase.

However, the existence of compensatory behavior creates some additional complexities. First, if agents have complete information and rationally choose not to adopt a safety measure, in the absence of externalities, they would be worse-off if they are forced to do so. Second, there is a direct positive effect on agents (envelope theorem), but they may also be subject to other individuals' risky behavior. For instance, in the case of seat belt laws, pedestrians and other drivers suffer from the less cautious driving of others who are forced to wear seat belts. In ice hockey, players do not face the risk of their own behavior only, but also the risk created by other players who skate with visors. Thus, optimal policy should balance between the private gains from protection and the negative effect from externalities or spillovers created by others. The existence of spillovers means that mandating visors or, for that matter, any safety measures is not necessarily optimal, even if non-users are signaling their type or there is a commitment problem.

6 Concluding remarks

In this paper we provide empirical evidence on the Peltzman effect, or compensating behavior by using data at the individual level from ice hockey at both minor and major leagues. We take advantage of the fact that during the full playing career of a typical professional player the use of visors in helmets has not always been voluntary. By exploiting this variation we are able to apply a difference-in-difference approach, and control for player fixed effects to sort out the specific impact of visor on increased aggressiveness, as measured by the penalty times accrued by the player during games. Whereas increased aggressiveness occurs, thus showing the presence of the Peltzman effect, visor use does not significantly affects performance.

The implications of our findings are not limited to ice hockey or, for that matter, sports. More generally, we show that behavior changes as a result of perceived protection by the individual. We provide causal evidence that the imposition of safety measures can have perverse effects that can go against the originally intended objective of such measure to the point that the overall effect may be detrimental to society. In fact, such economy-wide implications of our research raise the question of whether or not governments should attempt to save us from ourselves.

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Table 1: Summary Statistics.

	Visor in NHL (2001-2006)		No visor in NHL (2001-2006)	
	NHL	Other leagues	NHL	Other leagues
Games played	56.87	40.66	47.00	41.53
	(25.26)	(19.39)	(28.25)	(20.42)
Goals per game	0.18	0.33	0.11	0.28
	(0.14)	(0.26)	(0.12)	(0.24)
Assists per game	0.32	0.50	0.19	0.43
	(0.20)	(0.34)	(0.16)	(0.31)
Penalty minutes per game	0.63	1.11	0.96	1.59
	(0.49)	(0.94)	(0.89)	(1.34)
Number of players	250	228	513	488

	Visor in NHL (2001-2006)	No visor in NHL (2001-2006)
Birth year	1976.32	1976.56
	(4.18)	(4.08)
Born in Canada	0.41	0.63
	(0.49)	(0.48)
Born in Europe	0.47	0.21
	(0.50)	(0.41)
Born in U.S.	0.12	0.16
	(0.33)	(0.37)
First season	1997.40	1997.92
	(4.29)	(4.43)
Weight	200.19	204.74
	(13.19)	(15.80)
Height	5.79	5.82
	(0.39)	(0.38)

Notes: This table shows summary statistics of the main variables used in the paper. Standard deviation is reported in parentheses below the mean. The columns labeled "NHL" include game statistics for the NHL regular seasons 2001,2002, 2003, 2005 and 2006, while the columns labeled "Other leagues" include game statistics for the same players when they were playing in leagues with mandatory visors either in Europe or in minor leagues in North America. There was no NHL regular season in 2004.

Table 2: Estimates of the impact of using a visor on penalty minutes per game.

	(1)	(2)	(3)	(4)	(5)	(6)
Effect of visor	0.165***	0.198***	0.198***	0.187***	0.200***	0.191***
	(0.059)	(0.066)	(0.062)	(0.062)	(0.057)	(0.056)
No-visor type	0.331***	0.250***	0.181***	0.181***		
	(0.041)	(0.043)	(0.044)	(0.045)		
R-squared	0.114	0.197	0.304	0.307	0.619	0.620
Observations	7347	7347	7334	7334	7347	7347
Season effects	N	Y	Y	Y	Y	Y
League effects	N	Y	Y	Y	Y	Y
Player attributes	N	N	Y	Y	Y	Y
Player effects	N	N	N	N	Y	Y
League trends	N	N	N	Y	N	Y

Notes: This table shows estimates of the impact of using a visor on penalty in minutes per game. In the first four columns we control for different player characteristics and the dummy NV_i. The coefficient on this dummy reported in the second row, captures common characteristics among non-users. The last column shows estimates including players fixed effects. Player attributes include age, experience, year of birth, draft year, weight and height. Robust standard errors clustered at the player level are shown in parentheses below each point estimate. For the reported coefficients, those with *** are significant at the 1% level; those with ** are significant at the 5% level; and those with * are significant at the 10% level.

Table 3: Robustness checks of the impact on penalty minutes.

	(1)	(2)	(3)	(4)	(5)	(6)
	Using only lockout years		Using only Canadian leagues		Including treated trends	
Effect of visor	0.403**	0.501***	0.229**	0.174**	0.314**	0.295**
	(0.170)	(0.181)	(0.091)	(0.083)	(0.133)	(0.122)
No-visor type	0.196***		0.189***		14.972	
	(0.038)		(0.043)		(18.824)	
R-squared	0.276	0.659	0.360	0.667	0.304	0.619
Observations	3630	3637	5353	5358	7334	7347
Player effects	N	Y	N	Y	N	Y

Notes: This table shows estimates of the impact of using a visor on penalty in minutes per game using alternative sub-samples. The first two columns show estimates in which we compare NHL performance to that of other leagues during 2004, when the NHL was in a lockout. The next two columns show estimates in which we do not use the data from European, college or U.S. junior leagues. Finally, the last two columns show estimates in which we control for treated players specific trends (both for time and league ranking). Odd columns show estimates without fixed effects controlling for common characteristics among non-users (NV_i). Even columns show estimates including players fixed effects. All the specifications include a full set of controls, including league effects, season effects and player characteristics. Robust standard errors clustered by player are shown in parentheses below each point estimate. For the reported coefficients, those with *** are significant at the 1% level; those with ** are significant at the 5% level; and those with * are significant at the 10% level.

Table 4: Robustness checks controlling for differences in adaptation.

	(1)	(2)	(3)	(4)	(5)
Effect of visor	0.207***	0.197***	0.155***	0.158***	0.153***
	(0.055)	(0.056)	(0.055)	(0.054)	(0.052)
R-squared	0.624	0.623	0.623	0.622	0.629
Observations	7347	7347	7334	7347	7334
By cohorts	Y	N	N	N	Y
By position	N	Y	N	N	Y
By weight and height	N	N	Y	N	Y
By birthplace	N	N	N	Y	Y

Notes: This table shows estimates of the impact of using a visor on penalty in minutes per game. All estimates include players fixed effects. We include interactions of a dummy that takes into account treatment leagues, M_l, with different characteristics of the players in order to control for potential differences in adaptation to the NHL. The latter include birthplace, year of first season, playing position, weight, height, and date of birth. Robust standard errors clustered at player level are shown in parentheses below each point estimate. For the reported coefficients, those with *** are significant at the 1% level; those with ** are significant at the 5% level; and those with * are significant at the 10% level.

Table 5: Placebo test of the impact of using a visor on penalty minutes per game.

	(1)	(2)	(3)	(4)	(5)	(6)
	Minor pro as "pre-treatment" I		Minor pro as "pre-treatment" II		European leagues as "pre-treatment"	
Effect of visor	-0.005	-0.006	-0.077	-0.202	0.001	-0.081
	(0.161)	(0.185)	(0.174)	(0.241)	(0.144)	(0.158)
No-visor type	0.370**		0.443**		0.390***	
	(0.162)		(0.177)		(0.111)	
R-squared	0.289	0.671	0.330	0.721	0.296	0.698
Observations	3929	3937	2544	2544	3462	3470
Player effects	N	Y	N	Y	N	Y

Notes: This table shows estimates of the differences in adaptation to leagues with similar visor regulation. The first two columns show estimates comparing visor users and non-users in minor pro leagues relative to that in feeder or European leagues. The next two columns show estimates comparing visor users and non-users in minor pro leagues relative to that in other feeder leagues, excluding European leagues. Finally, the last two columns show estimates comparing visor users and non-users in European leagues relative to that in junior and college leagues. All specifications include a full set of controls. Robust standard errors clustered by player are shown in parentheses below each point estimate. For the reported coefficients, those with *** are significant at the 1% level; those with ** are significant at the 5% level; and those with * are significant at the 10% level.

Table 6: Estimates of the impact of using a visor on performance.

	(1)	(2)	(3)	(4)	(5)	(6)
	Assists per game			Goals per game		
Effect of visor	0.013	-0.009	-0.010	0.001	-0.013	-0.005
	(0.015)	(0.015)	(0.032)	(0.012)	(0.013)	(0.027)
No-visor type	-0.090***			-0.041***		
	(0.012)			(0.008)		
R-squared	0.463	0.610	0.610	0.467	0.587	0.587
Observations	7334	7347	7347	7334	7347	7347
Player effects	N	Y	Y	N	Y	Y
Treated trends	N	N	Y	N	N	Y

Notes: This table shows the impact of using a visor on performance. The first three columns show estimates on assists per game, the last three columns show estimates on goals per game. All models include a full set of controls, including league effects, season effects, player characteristics and league specific trends. Robust standard errors clustered by player are shown in parentheses below each point estimate. For the reported coefficients, those with *** are significant at the 1% level; those with ** are significant at the 5% level; and those with * are significant at the 10% level.

Appendix 1: Infractions that result in Penalty in minutes

- Abuse of officials: Arguing with, insulting, using obscene gestures or language directed at or in reference to, or deliberately making violent contact with any on or off-ice official.
- Aggressor penalty: Assessed to the player involved in a fight who was the more aggressive during the fight. This is independent of the instigator penalty, but both are usually not assessed to the same player (in that case the player's penalty for fighting is usually escalated to deliberate injury of opponents, which carries a match penalty).
- Attempt to injure: Deliberately trying to harm an opponent.
- Boarding: Pushing an opponent violently into the boards while the player is facing the boards.
- Butt-ending: Jabbing an opponent with the end of the shaft of the stick. It carries an automatic major penalty and game misconduct.
- Charging: Taking more than three strides or jumping before hitting an opponent.
- Checking from behind: Hitting an opponent from behind. It carries an automatic minor penalty and misconduct, or a major penalty and game misconduct if it results in injury. Illegal check to the head: Lateral or blind side hit to an opponent, where the player's head is targeted and/or the principal point of contact
- Clipping: Delivering a check below the knees of an opponent. If injury results, a major penalty and a game misconduct will result.
- Cross-checking: Hitting an opponent with the stick when it is held with two hands and no part of the stick is on the ice. Delay of game: Stalling the game.
- Diving: Falling to the ice in an attempt to draw a penalty.
- Elbowing: Hitting an opponent with the elbow.
- Fighting: Engaging in a physical altercation with an opposing player, usually involving the throwing of punches with gloves removed or worse.
- Goaltender Interference: Physically impeding or checking the goalie.

- Head-butting: Hitting an opponent with the head. A match penalty is called for doing so.

- High-sticking: Touching an opponent with the stick above shoulder level. A minor penalty is assessed to the player. If blood is drawn, a double-minor is usually called. Referees may use their discretion to assess only a minor penalty even though blood was drawn. They may also assess a double-minor when blood is not drawn, but he believes that the player was sufficiently injured or that the offending player used excessively reckless action with his stick.

- Holding: Grabbing the body, equipment, or clothing of opponent with hands or stick.

- Holding the stick: Grabbing and holding an opponent's stick, also called when a player deliberately wrenches a stick from the hands of an opposing player or forces the opponent to drop it by any means that is not any other penalty such as Slashing.

- Hooking: Using a stick as a hook to slow an opponent, no contact is required.

- Instigator penalty: Being the obvious instigator in a fight. Called in addition to the five minute major for fighting.

- Interference: Impeding an opponent who does not have the puck, or impeding any player from the bench.

- Joining a fight: Also called the "3rd man in" rule, the first person who was not part of a fight when it broke out but participates in said fight once it has started for any reason (even to pull the players apart) is charged with an automatic game misconduct in addition to any other penalties they receive for fighting.

- Kicking: Kicking an opponent with the skate or skate blade. Kicking carries a match penalty if done with intent to injure, but otherwise carries a major penalty and a game misconduct.

- Kneeing: Hitting an opponent with the knee.

- Roughing: Pushing and shoving after the whistle has been blown or checking an opponent with the hands in his face.

- Slashing: Swinging a stick at an opponent, no contact is required.

- Slew Footing: Tripping an opponent by using your feet.

- Spearing: Stabbing an opponent with the stick blade.

- Starting the wrong lineup: When offending team fails to put the starting lineup on the ice at the beginning of each period.
- Substitution infraction: When a substitution or addition is attempted during a stoppage of play after the linesmen have signaled no more substitutions or if a team pulls its goalie and then attempts to have the goalie re-enter play at any time other than during a stoppage of play.
- Too many men on the ice: Having more than six players (including the goalie) on the ice involved in the play at any given time.
- Tripping: Using a stick or one's body to trip an opponent.
- Unsportsmanlike conduct Arguing with a referee; using slurs against an opponent or teammate; playing with illegal equipment; making obscene gestures or abusing an official.

Appendix 2: Leagues in Ice Hockey

Table 7: Hockey leagues and visor regulation.

Name	Short name	League type	Face protection
National Hockey League	NHL	Pro	Visors are non-mandatory
United Hockey League	UHL	Minor pro	Mandatory since 2004
American Hockey League	AHL	Minor pro	Mandatory since 2006
East Coast Hockey League	ECHL	Minor pro	Mandatory since 2003
Central Hockey League	CHL	Minor pro	Mandatory since 2004
Western hockey league	WHL	Major junior (CA)	Mandatory since 1976
Ontario hockey league	OHL	Major junior (CA)	Mandatory since 1976
Quebec Major junior hockey league	QMJHL	Major junior (CA)	Mandatory since 1976
British Columbia Junior Hockey League	BCJHL	Junior (CA)	Mandatory since 1981
Ontario Provincial Junior A Hockey League	OPJHL	Junior (CA)	Mandatory since 1981
British Columbia Hockey League	BCHL	Junior (CA)	Mandatory since 1981
Saskatchewan Junior Hockey League	SJHL	Junior (CA)	Mandatory since 1981
Atlantic Junior Hockey League	AJHL	Junior (CA)	Mandatory since 1981
Metropolitan Junior Hockey League	MetJHL	Junior (CA)	Mandatory since 1981
Ontario Junior Hockey League	OJHL	Junior (CA)	Mandatory since 1981
Canadian Junior Hockey League	CJAHL	Junior (CA)	Mandatory since 1981
United States Hockey League	USHL	Junior (U.S.)	Always been mandatory
North American Hockey League	NAHL	Junior (U.S.)	Always been mandatory
Western Collegiate Hockey Association	WCHA	College (NCAA)	Mandatory since 1980
Central Collegiate Hockey Association	CCHA	College (NCAA)	Mandatory since 1980
NCAA East Division	H-East	College (NCAA)	Mandatory since 1980
Eastern College Athletic Conference	ECAC	College (NCAA)	Mandatory since 1980
National Collegiate Athletic Association	NCAA	College (NCAA)	Mandatory since 1980
College Hockey Association	CHA	College (NCAA)	Mandatory since 1980
International Hockey League	IIHL	International	Mandatory since 1994
Sweden Elitserien	SEL	European elite	Mandatory since 1969
Finland SM-liiga	FNL	European elite	Mandatory since 1988
Russian Elite League	KHL	European elite	Mandatory since 1994
Switzerland National League A	Swiss-A	European elite	Mandatory by 2004
Deutsche Eishockey League	DEL	European elite	Mandatory since 1998

Notes: This table shows the different leagues used in our study, as well as their respective regulation regarding facial protection. In college leagues, players are required to use a full cage if they are under 18, and may choose between full cage or a visor if they are older. Mandatory visors were introduced in European and international leagues with a "grandfather clause" which exempted some players from using a visor. We take that into account when coding the variable M_l.