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News Media Coverage of Crime and Violent Drug Crime: A Case for Cause or Catalyst?

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ABSTRACT

Evidence about the relationship between exposure to media violence and criminal activity remains mixed. While some scholars argue that exposure to violent media content "triggers" crime and aggression, others contend that media may influence crime, but only as a source of information about techniques and styles, not as a motivation for crime. This debate has critical implications for criminal justice academics as calls for policy are regularly made on the basis of research in this area. This article contributes to this literature by presenting detailed empirical evidence of how media coverage of violent crimes affects homicides perpetrated by drug traffickers in Mexico, and their crime style. With an empirical model that addresses possible bidirectionalities between drug homicides and media coverage, we tracked 32,199 homicides, their stylistic characteristics, and their coverage by the press. Our results show that when media covers drug homicides it influences the probability that other criminals use similar styles of crimes, but it does not change overall rates of homicidal activity. This is evidence against the "trigger" hypothesis, and in favor of "copycat" effects.

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Violent crime; media violence; aggression; Mexico; traffickers

Q3 Media effects on crime and crime style

A substantial and important portion of criminal justice research is concerned with environmental, situational, or systemic factors that cause, or are likely to cause, criminal behavior. As part of this rich literature, the role that media coverage has in shaping criminal behavior and beliefs about the justice system has long been an interest of criminal justice studies.

In essence, there are two sides to the debate. The first, identified with the "General Aggression Model" (DeWall, Anderson, & Bushman, 2011; Gilbert, Daffern, Talevski, & Ogloff, 2013), argues that exposure to media coverage of violent crimes "triggers" the development of aggressive attitudes or behaviors and desensitizes people to actual violence (Anderson, Bushman, Donnerstein, Hummer, & Warburton, 2015; DeLisi, Vaughn, Gentile, Anderson, & Shook, 2013; Laser, Luster, & Oshio, 2007). In opposition to this theory, some studies have found no solid evidence of such effects (e.g.

46 Ferguson, Olson, Kutner, & Warner, 2014; Schwartz & Beaver, 2016). These scholars
47 feeling more comfortable interpreting media coverage of violent crimes as a "rudder"
48 of crime, meaning a factor that can shape criminal behavior, influencing, for example,
49 the style of a crime (stylistic rather than motivation modeling which we henceforth
50 refer to as "copycat"), but does not cause it (Chadee, Surette, Chadee, & Brewster,
51 2017; Doley, Ferguson, & Surette, 2013; Ferguson et al., 2008; Savage & Yancey, 2008;
52 Surette, 2013). Media coverage of violent crimes provide stylistic inspiration (Coyne,
53 2007; Ferguson & Colwell, 2017; Surette, 2011, 2014, 2015).

54 Overall, evidence regarding the relationship between exposure to media coverage
55 of violent crimes, and violent crime remains weak and mixed (Doley et al., 2013;
56 Savage & Yancey, 2008). Although it was often previously assumed that clear links
57 between media violence and societal violence existed (Strasburger, 2007), such con-
58 cerns have not replicated in more recent research (e.g. DeCamp & Ferguson, 2017;
59 Markey, Markey, & French, 2015). To advance these debate, scholars have identified
60 the need to obtaining better measures (Coyne, 2007; Surette, 2014) such as to reduce
61 concerns related to methodological flexibility and false positive results, exploring fic-
62 tional and nonfictional media portrayals (Ferguson et al., 2008; Savage & Yancey,
63 2008), and delving into different types of crimes (Surette, 2013). Related to the latter
64 two points, it is not unreasonable to suggest that news media may have more influ-
65 ence on viewers than fictional media given that viewers may assume news media is
66 more factual. Further, it is possible that media impacts may exist for some types of
67 crimes, but not others.

68 This article contributes to advancing criminal justice literature by developing a
69 study that addresses these concerns. We test the relationship between exposure to
70 news media coverage of drug-related homicides, and further drug-related homicides,
71 using uniquely detailed, non-self-reported measures of media coverage and crim-
72 inal behavior.

73 Criminal justice researchers trying to quantitatively test the possible effects of
74 media coverage of violent crimes may find interesting insights in our study because of
75 the level of detail of the measures we gathered. We recorded 32,199 homicides carried
76 out by drug traffickers, and we measured the stylistic characteristics of each crime,
77 whether each of these homicides was covered by the media, and how it was covered.
78 This panel of 169 weeks allowed us to identify, for example, the share of drug traffick-
79 ers' homicides that were covered by the printed press, and whether the details of
80 such crimes, including stylistic characteristics, were published. Importantly, crime
81 rates and rates of copycat crime were not self-reported by offenders but were
82 obtained from official statistics or, in the case of copycat crimes, subsequent news
83 media coverage.

84 The results of our study provide empirical evidence to sustain that "rudder" theories
85 are better positioned to explain how exposure to media coverage of violent crimes
86 affects criminal behavior than are those that assume a causal connection regarding
87 motivation to commit crime. When media covers the homicides of drug traffickers,
88 such coverage is not associated with more homicides, but criminals do use similar
89 crime styles to the crimes that were covered by the press. In other words, traffickers
90

91 are more prone to be ruddered “copycats” than to be “triggered” into violence by the
92 media (Surette, 2013).

93 Our article should be understood as part of a trend to conduct research in places
94 outside the US, to enlighten relevant criminal justice debates. Cross-cultural research
95 can help elucidate the applicability of relevant theories across diverse groups to
96 ensure their practical applicability to such groups. For example, Brazil has been used
97 to study psychopathy in criminal and forensic psychiatric populations (De Oliveira-
98 Souza, Moll, Azecedo Ignácio, & Hare, 2008), England and Wales to identify triggers of
99 violence in prisoners and forensic patients (Freestone, Ullrich, & Coid, 2017), Australia
100 to validate the Ontario Domestic Assault Risk Assessment (Lauria, McEwan, Luebbers,
101 Simmons, & Ogloff, 2017), and China to test whether distributive justice and proced-
102 ural justice are predictors of job satisfaction among community correctional staff
103 (Jiang et al., 2016). Cross-national and cross-cultural assessment of theory can help to
104 understand the limitations of a given theory and whether it is broad enough to tran-
105 scend its culture of origin.

106 The rest of the article proceeds as follows. The first section discusses the existing lit-
107 erature about media coverage of violent crimes, and crime. The second section pre-
108 sent the hypotheses and presents overall design. The third section presents
109 the empirical test. The fourth and fifth sections present results and exploratory testing.
110 We conclude by discussing how the article contributes to the literature, and by sug-
111 gesting possible avenues for future research.

113 **Media coverage of violent crimes, and crime**

115 The question of whether media coverage of violent crimes may have effects on crime
116 rates or on crime styles remains highly controversial (Doley, Ferguson, & Surette, 2013;
117 Ferguson et al., 2008; Savage & Yancey, 2008).

118 Ovearll, two theories have been used to explain the effects that media coverage of
119 violent crimes may have on criminal behavior: The General Aggression Model (DeWall
120 et al., 2011; Gilbert, Daffern, Talevski, & Ogloff, 2013), and the “Catalyst” model of vio-
121 lent crime (e.g. Ferguson et al., 2008; Surette, 2013).

122 Supporters of the General Aggression Model (GAM) are confident that exposure to
123 media coverage of violent crimes is one of the factors that “triggers” human aggres-
124 sion though evidence for such effects is typically correlational rather than experimen-
125 tal (proponents of the GAM claim consistent evidence for experimental effects on
126 milder aggression, a position contested by other scholars, see Savage & Yancey, 2008).
127 Scholars who advocate for the GAM contend that the media is partially responsible for
128 deviant behavior (Laser et al., 2007), and contributes to violence by desensitizing peo-
129 ple to the pain and suffering of others (Helfgott, 2015).

130 Those who favor the GAM contend that prolonged exposure to violent media can
131 fundamentally alter a person’s personality, causing them to become callous and vola-
132 tile. For example, some believe that at least 30 separate occurrences of gun violence
133 were inspired by the Russian Roulette scene from the movie *The Deer Hunter* (Gunter,
134 2008), that the shooter in The University of Virginia Tech massacre was copying events
135 from an action movie (Nizza, 2013, although evidence for these claims were lacking),

136 and that there have been many instances of crime inspired by movies (Sparks, 2015).
137 Some scholars have also found evidence of increased engagement in physical fights
138 among individuals exposed to violent video games (Anderson, Gentile, & Buckley,
139 2007; Ivory, Ivory & Lanier, 2017), as well as aggression among listeners of profanity
140 Q4 laced games (Ivory & Kaestle, 2013), and violent song lyrics (Anderson et al., 2003).
141 Individuals with a preexisting history of delinquency of violence are sometimes consid-
142 ered particularly at risk for violent media effects (e.g. DeLisi et al., 2013). Certainly indi-
143 viduals involved in criminal enterprises are not passive viewers and media viewing can
144 be expected to interact with the political culture of Mexico, gang culture and the spe-
145 cific motivations of criminal gangs. However, given claims that significant proportions
146 of violent crime could be specifically attributed to violence in media (e.g. Strasburger,
147 2007), from this theoretical perspective we would still expect to find a relationship.

148 Notwithstanding the research above, many studies have shown a lack of correlation,
149 or lack of causality, between media depictions of violence, and aggression (Doley et al.,
150 2013; Ferguson et al., 2008; Gunter & Daly, 2012; Savage & Yancey, 2008; Surette, 2013).
151 Some studies have suggested that violent movies (Dahl & Dellavigna, 2009) and video
152 games (Markey, Markey & French, 2015) are associated with reductions in violent crime
153 in the United States. Some criminologists have specifically referred to linking violent
154 media to violent crimes as a "myth" (e.g. Fox & DeLateur, 2014).

155 Thus, in opposition to the General Aggression Model, some scholars have devel-
156 oped a series of theories that characterize media as a "rudder" of crime. These scholars
157 point out that we are living in the most peaceful epoch in human history, despite the
158 ubiquity of media coverage of violent crimes (Pinker, 2011). They also argue that if sig-
159 nificant and causal media effects could be found, they would not be large enough to
160 change aggregated crime rates in notable ways (DeCamp & Ferguson, 2017;
161 Surette, 2013).

162 Indeed, research has shown that media coverage of violent crimes does not reduce
163 empathy for the victims of real violence (Ramos, Ferguson, Frailing, & Romero-Ramirez,
164 2013) and does not create more aggressive behavior (Savage & Yancey, 2008). Also,
165 several studies have shown that playing violent videogames or watching violent TV
166 does not have a criminogenic impact (Breuer, Vogelgesang, Quandt, & Festl, 2015;
167 Ferguson, San Miguel, Garza, & Jerabeck, 2012; Ferguson et al., 2008; Ferguson &
168 Savage, 2012; Savage, 2004; Surette & Maze, 2015).

169 Those endorsing the "rudder" theory reject that individuals are "passive receptacles
170 of learning," and instead conceive individuals as "active shapers and processors of
171 media culture" (Elson & Ferguson, 2014; Ferguson, 2015). In other words, these models
172 hold that the interaction between media and behavior is determined by the media
173 user rather than media content (Phippen, 2017). Furthermore, proponents of these
174 theories argue that environmental factors and psychological predispositions are
175 behind disruptive behavior disorders, rather than exposure to media (Ferguson &
176 Dyck, 2012; Ferguson & Savage, 2012).

177 Adherents of the "rudder" theory suggest that, rather than causing crimes, media
178 might provide stylistic ideas to potential criminals, i.e. promote copycat crime (Doley
179 et al., 2013; Surette, 2011; 2015; Surette & Gardiner-Bess, 2013). In other words, media
180 is source to identify crime techniques, but not a crime motivator. The work of Surette

181 (2013), for example, analyzed anonymous surveys of 574 male and female correctional
182 inmates. Surette concluded that the media influenced the style of crimes by providing
183 instructional models to predisposed individuals, yet it did not prompt crime per se. He
184 coined the term "ruddering" to illustrate how media portrayals of criminality shape
185 the stylistic form the crime takes on, instead of acting as a direct motivator for the
186 crime itself.

187 188 **News media effects**

189 Interestingly, much of the coverage of the media effects debate has focused on fic-
190 tional media ranging from books (e.g. Devilly, Brown, Pickert, & O'Donohue, 2017)
191 through video games. Comparatively little has examined the direct impacts of news
192 media coverage of violent crimes on imitative or rudder theories of crime perpetra-
193 tion. A fair amount of research does examine the potential for new media to cultivate
194 beliefs about crime (Chadee, Sooknanan, & Williams, 2017; Oliver & Fonash, 2002),
195 although research on direct effects is rarer.

196 Some research has suggested that copycat effects may reflect internal motivations
197 more than passive imitation (Chadee, Surette, Chadee, & Brewster, 2017). This is gen-
198 erally supportive of the rudder rather than imitative perspective, insofar as that indi-
199 viduals seek opportunities to model desired behaviors rather than acquire motivations
200 themselves directly from media. However, more research would certainly be welcome.

201 202 **Drug-related homicides**

203 For the purposes of the current article we are particularly interested in the issue of
204 drug-related homicides. Such homicides occur in context with the larger drug trade,
205 typically carried out in relation to cartels and gangs fighting for turf to produce, trans-
206 port or sell illegal substances (Celis, Lujan, & Ponce, 2018). The relation between drug
207 crimes and homicide has been known for some time (Chauhan et al., 2011). Homicide
208 can be understood as a struggle for regional political control, a practice ordered by
209 cartel leaders rather than spontaneous violence of foot soldiers, and as an expansion
210 strategy from solely drug trafficking to other kinds of organized crime (Campbell &
211 Hansen, 2014). In this sense, drug-related homicides bear some resemblance to terror-
212 istic homicides (Phillips & Ríos, 2018).

213 Some evidence has suggested similarities between domestic related extremists and
214 gang-related extremists, and gang extremists may resemble domestic terrorists more
215 than they do nonextremist gang members (Pyrooz, LaFree, Decker, & James, 2018).
216 There is some speculation that media, and social media in particular, can be used in
217 the recruitment and radicalization of terrorist group members (Jones, 2017). Thus con-
218 sidering media effects related to gang-related homicides is not unwarranted (Ríos &
219 Rivera, 2018).

220 Similar incentives seem to exist for terrorists as for drug-related gang extremists
221 (Hoffman, 1997; Iqbal, 2015; Surette, Hansen, & Noble, 2009). Terrorists use press
222 coverage to promote an appearance of strength (Conrad & Greene, 2015; Kearns,
223 Conlon, & Young, 2014) with the goal of advancing their criminal objectives by
224
225

226 inducing fear or spreading propaganda (Abrahms & Conrad, 2017; Hoffman, 2010;
227 Wright, 2009). There is some evidence that these organizations may time their attacks
228 to coincide with the news cycle (Krueger, 2008), and follow media coverage to better
229 target their cross-border attacks (Asal & Hoffman, 2016). Furthermore, TV news outlets
230 play an important role in mediating terrorist messages (Iqbal, 2015). In fact, some
231 criminal organizations run their own newspapers, radio stations, or websites (Hoffman,
232 Shelton, & Cleven, 2013), and even have proved skillful in the use of social media and
233 cyber technology (Farwell, 2014). For example, social media has played an essential
234 role in the Jihadists' operational strategy in Syria, Iraq, and beyond (Klausen, 2015).

236 **Criminal justice policy**

237 One of the challenges for criminal justice policy has been in regards to what to do
238 with disparate information regarding media effects. On one hand, as indicated above,
239 research on media effects has not returned clear, consistent results that would be of
240 value for policy makers. On the other hand, professional guild organizations such as
241 the American Psychological Association or American Academy of Pediatrics appear to
242 be under pressure from policy makers to provide *the answer* as to whether media
243 effects are real or not. This pressure appears to have, at times, caused such organiza-
244 tions to make false statements regarding the consistency of effects (Confas, Carl, &
245 Woodley of Menie, 2018; Copenhagen, 2015).

247 One concern is that the issue of media effects may provide a red herring to criminal
248 justice policy insofar as media effects may distract policy makers from causes of homi-
249 cides with better empirical footing. In some cases, this may be purposeful, such as
250 when violent video games are blamed for mass shootings, despite clear evidence at
251 this point that violent video games play little causal role (Fox & DeLateur, 2014). The
252 same can be said for news media. Some scholars have suggested, for instance, that
253 news media should be discouraged from releasing the names of shooters (Lankford &
254 Madfis, 2018). Given this proposal has few evident downsides, it is easy to support,
255 however, the actual effectiveness on reducing mass shootings remains unknown.

256 As such, it is important that research provide clear data regarding the impact of
257 media portrayals of crime on actual homicides. Such research can help set public pol-
258 icy priorities for criminal justice.

260 **Research design**

261 An important difficulty contributing to the empirical intractability of the aforment-
262 tioned debate is the fact that the relationship between media coverage of homicides,
263 and further homicides may be a vicious cycle (Savage & Yancey, 2008). On one hand,
264 to the extent that the media is driven by sensationalism, the media has an incentive
265 to pay more attention to more gruesome stories (Chermak & Chapman, 2007). These
266 hypotheses are relevant because media front-pages prioritize content with higher
267 audience ratings and editor's approval (Coddington, 2014; Fink & Schudson, 2014;
268 Reiner & Newburn, 2007). In other words, the media operates under criteria of news-
269 worthiness (Gruenewald, Pizarro, & Chermak, 2009). Thus, homicides may induce
270

media coverage if crimes are strategically planned to be scandalous and provocative (Nacos, 2002; Wu, 2000). Furthermore, drug traffickers are clearly a newsworthy and lucrative topic for the media as they are one of the most popular topics of media attention within the crime genre (Rawlinson, 2016).

On the other hand, to the extent that criminals benefit from the direct/indirect reputational gains of coverage, criminals have an incentive to make their crimes increasingly gruesome. Because of these dynamics, it is difficult to tell who is really influencing who. We know that criminals, particularly when they operate in groups, may have an interest in committing overtly violent actions for strategic purposes. Criminals could be expected to copy "crime styles" featured by the press, not only because the media could function as an "instructional model" (Surette, 2011, 2014), but also because criminals may want media attention. Criminal groups may benefit from visibility because it helps them to intimidate their enemies (see for example Brown, 2017; Durán-Martínez, 2015).

To address the bidirectionality of media coverage and homicides, and to determine whether these relationship follows the logic of "trigger" or "rudder" arguments, four hypothesis will be tested.

Two basic hypotheses:

H1 (i.e. trigger): The higher the level of media coverage of homicides, the greater is the likelihood that criminals are violent.

H2 (i.e. rudder): The higher the level of media coverage of homicides, the greater is the likelihood that criminals use styles of violence similar to the ones that had been covered by the media.

Two reciprocal hypotheses:

H3: The more common homicides are, the greater the likelihood that media covers it.

H4: The most common certain styles of crimes are, the greater the likelihood that media covers those styles.

We will use drug traffickers operating in Mexico as our object of study because it is a novel case with much potential to directly address some of the gaps that scholars studying the relationship between media coverage of homicide and actual homicides, have identified. This case allowed us to (i) develop innovative ways to measure media coverage and homicide (Coyne, 2007; Surette, 2014), (ii) explore news coverage (Ferguson et al., 2008; Savage & Yancey, 2008), and (iii) delve into crime styles (Surette, 2013).

First, measures of drug trafficking homicides can be rich because traffickers have notably different stylistic patterns (Martin, 2012), and large geographical and temporal variance in their homicide rates (Coscia & Ríos, 2012; Molzahn, Ríos, & Shirk, 2012). Stylistic differences can be found in the use of specific types of weapons, victims' characteristics, types of torture, the display of dismembered or decapitated bodies at the crime scene, and in the use of other intentionally public displays, such as banners, that explain their motivations or intents (Durán-Martínez, 2015; Shirk & Wallman, 2015). Drug traffickers' crimes can be observed across 1068 municipalities, from border cities like Río Bravo and Tamaulipas that had only 18 homicides from 2008 to 2010, to places like Juárez that endured 6300 homicides during the same period.

316 Second, there is plenty of non-fictional media coverage of drug traffickers' homi-
317 cides. That makes our measures of media coverage to be centralized and comparable,
318 an advantage for studies of crime effects and copycats (Surette, 2014, 2015).¹ The goal
319 of these types of measurements is to avoid offenders rationalizing their homicides by
320 attributing it to the impact of the media, or to other third parties. In other words, the
321 goal is to find a way to measure whether stylistic patterns of a crime were copied,
322 objectively, without being duped by criminals attempting to shift the blame.

323 Finally, drug trafficking related homicide is a form of crime that has not been yet
324 formally explored under the lenses of the media effects debate. A burgeoning litera-
325 ture seeks to understand the levels of violence in Mexico (Calderón, Robles, Díaz-
326 Cayeros, & Magaloni, 2015; Dell, 2015; Osorio, 2015; Ríos, 2015; Shirk & Wallman, 2015;
327 Trejo & Ley, 2017) but research analyzing the dynamics within the conflict, such as the
328 relationship between media coverage and homicide has not been developed.

329 Overall, it is somehow surprising that there are not very many studies that meas-
330 ure the effects of media violence on drug traffickers and smuggles. First, considering
331 the increased relevance of this form of crime for the judicial system (Benson &
332 Decker, 2010; Decker & Curry, 2002; Mercille, 2011; Payan, 2016). Second, the exist-
333 ence of several unique state/weekly panel datasets that we collected to describe
334 homicide rates and crime styles among drug traffickers operating at Mexico allow
335 this measurement.

337 Data

338 To measure the effects of media coverage of violent crime on homicide, we con-
339 structed a state-level panel dataset for 32 states (all Mexican states) for 169 weeks²
340 from 6 October 2007 to 31 December, 2010.³ Every week we measured (a) the number
341 Q5 of drug traffickers' homicides (*Homicides*), (b) the share of drug reatted homicides that
342 were "claimed" using a narco-message (*credit-taking*), (c) media coverage, and (d)
343 media quality.

344 First, to measure "Homicides," we obtained official registries of drug traffickers'
345 homicides from Mexican authorities. These are homicides that were officially attributed
346 to drug traffickers by Mexican intelligence and security institutions (SNSP, 2011), and
347 that were monthly reported from October 2007 to September 2011. To construct this
348 variable, we gathered information about 32,199 drug traffickers' homicides. A homicide
349
350

351 ¹Gathering data on media coverage of violent crimes, and copycat criminals is generally complicated due to self-
352 justification and perception biases (Doley et al., 2013; Surette, 2011). Self-justification is problematic because
353 criminals may want to shift the blame to the media or third parties. Perception biases are a problem because even
354 if two crimes appear to be similar, scholars cannot be certain that they were inspired by the same source (Coleman,
355 2004; Stack, 2000; Surette & Maze, 2015). Initially, some studies tried to overcome this problem by "shocking"
356 individuals with fictional media coverage of violent crimes, and measuring their reactions (Kaplan, 1984). Yet, this
357 technique was questionable because it did not provide an accurate paradigm to understand how real-world violent
358 media is delivered and consumed in normal settings (Felson, 1996; Freedman, 2002).

²
³We use weeks as our frequency measure because media coverage measures were only available per week, starting
359 on 6 October 20107 (Reforma, 2017). We restricted our dataset to 2010 because starting 2011, Mexican media
360 signed an agreement to stop covering drug traffickers' homicides, thus probably limiting our capacity to properly
measure the quality of media coverage (Molzahn et al., 2012). All data will be a public for reproduction
and validation.

361 was considered as conducted by a drug trafficker if it met six criteria.⁴ Overall, drug
 362 traffickers' homicides represent 95% of all homicides perpetrated during those years
 363 and in those areas.

364 Because our media variables are reported weekly, we divided the total number of
 365 homicides by the number of weeks. When a week fell in between two months, we
 366 proportionally attributed the number of homicides according to the share of the
 367 weeks that fell in each month.

368 Second, to measure "Crime Style," we constructed a database that identifies a stylistic
 369 feature that sometimes accompany drug traffickers' homicides: credit-taking banners.
 370 Credit-taking is not a behavior typically discussed within traditional theories of
 371 media effects such as the GAM, but can be considered a stylistic behavior consistent
 372 with the Catalyst Model (Ferguson et al., 2008). In this sense credit-taking can be considered
 373 a stylistic behavior that is not essential to the practicalities of the crime itself,
 374 but which may related to motivations that are intrinsically satisfying to the perpetrator.
 375 As such, they are a measurable indicator of the degree to which perpetrators may
 376 be influenced in "style" or crime if not the commission of crimes themselves. This
 377 allows for a test of the Catalyst Model.

378 Credit-taking banners, also known as "narco-messages" (Atuesta, 2017; Campbell &
 379 Hansen, 2014; Durán Martínez, 2015; Martin, 2012; Mendoza Rockwell, 2016) are used
 380 by drug traffickers' to take-credit for their criminal actions, or to clarify their degree of
 381 responsibility for them. Narco-messages may also be displayed to intimidate potential
 382 victims, to communicate with local citizens, or to give instructions to investigators,
 383 policemen, or journalists (Martin, 2012).

384 The content of the banners may be a threatening maxim like "you cannot be on
 385 good terms with both God and the Devil," or an appeal to the public, like the claim
 386 that "this (battle for turf) is for the good of all," or like the banner dedicated to "the
 387 brave, noble, and loyal people" wishing them "Merry Christmas, ho, ho, ho." Narco-
 388 messages are a creepy stylistic innovation that stretches the boundaries of traditional
 389 graffiti with its bizarre mimicry of the formality of Mexican political campaign propa-
 390 ganda (Salopek, 2011).

391 The use of credit-taking messages is not exclusive to Mexico. Similar criminal mes-
 392 saging has happened in places such as Colombia, Southern Italy, and in numerous
 393 U.S. cities (see Cowell, 1992; Martin, 2012; Ortiz, 2013 as cited by Phillips & Ríos, 2018).

394 Criminal messaging is also a major part of terrorism, which has been described as
 395 violent propaganda or violent communication (e.g. Schmid & De Graaf, 1982). A line of
 396 research has examined why terrorist groups sometimes claim their attacks (Wright,
 397 2009). Among reasons for doing so are intergroup competition (Hoffman, Jengelly,
 398 Duncan, Buehler, & Rees, 2010), communication within the group (Brown & Hamilton-
 399 Giachritsis, 2005), and operation in democratic countries (Min, 2013). However, this lit-
 400 erature also established that there are reasons militant groups do not seek credit.

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⁴These were (i) use of high-caliber firearms, (ii) signs of torture or severe lesions in victims, (iii) bodies found at the crime scene or in a vehicle, (iv) victims that were taped, wrapped, or gagged, (v) murders that happened in a prison and involved criminal organizations, and (vi) if one of several "special circumstances" occurred. Among these "special circumstances" are whether the victim was abducted prior to assassination (known as a "levantón" in Spanish), ambushed or chased, whether the victim was an alleged member of a criminal organization, and whether a criminal organization publicly claimed responsibility for the murder (Ríos, 2013).

Such reasons could be that the attack was especially heinous, or the desire to avoid a bad reputation (Abrahms & Conrad, 2017; Hoffman, 2010).

To gather information about credit-taking banners, we performed massive amounts of queries with online search engines using "narco-message" as our keyword. Following Coscia and Ríos (2012), we relied on Google as our main search engine to obtain content. We extracted information about narco-messages from public blogs and forums run by citizens who collect information about narco-messages (e.g. Blog del Narco), specialized online websites, Youtube, and local/national newspapers.

Yet, unlike Coscia and Ríos (2012), we gathered a team of researchers to read, filter, and classify all the results. We did it to ensure that each narco-banner was not double counted if it was covered by different media sources using different language. Also, this allowed us to identify if the narco-messages was accompanied by a homicide, and if the narco-message was reported on detail.

Out of a total of 32,199 drug-related homicides we could find that 857 were accompanied by a narco-message. Out of 1731 narco-messages, 1293 were detailed. Out of all narco-messages, 56% came from the local newspapers, 31% from national press, 7% from local blogs/forums, 3% from Youtube, and 2% from international newspapers. Because 63% of all narco-messages were reported by local newspapers or local blogs/forums, we allow us to assume that people committing homicides at a locality are reading these local news. Every link used to extract information is available under request.

We cannot ensure that our dataset contains all the narcomessages ever displayed with a homicide in Mexico, but only those that were reported online somehow. This feature may bias our results particularly for areas with less internet access. Even if this is problematic, we believe that this does not significantly change time trends as internet access did not changed significantly during the 169 weeks that this study covers. The percentage of households with internet access only grew 4.9 pp from 2008 to 2010 (18.4% to 23.3%).

Our measure, "Credit-taking " is the share of weekly homicides that had a credit-taking banner. This was calculated as

$$\text{CreditTaking}_{it} = \frac{\text{Homicides with a Narcomessage}_{it}}{\text{Homicides}_{it}}$$

The measure takes values from 0 to 1, where 1 means that all homicides in week t , state i , were claimed with a banner. This measure distributes approximatedly normal.

Third, to create the variable "Media Coverage," we identified the amount of drug traffickers' homicides that were covered by independent counts of drug traffickers' homicides conducted by the press. Beginning in 6 October 2007, news-media organizations started counting the number of drug traffickers' homicides per week using a similar methodology as the government. The most comprehensive coverage was done by Reforma, a major Mexican newspaper with a large pool of local correspondents and alliances with local newspapers in every state. Other national newspapers like Milenio and El Universal also had their own independent counts, alliances, and correspondents (Ríos, 2013). The "Justice in Mexico Program" has also gathered statistics regarding press counts of traffickers' homicides from 2006 to 2017 (Milenio, 2017; Reforma, 2017). All of these "press counts" were reported digitally (at the newspaper website) and in the printed version.

Table 1. Descriptive statistics.

Variables	Mean	SD	Min	Max
Homicides	5.954	12.72	0	102
Credit taking	0.0795	0.258	0	1
Media quality	0.108	0.301	0	1
Media coverage	3.124	5.403	0	77.62

SD: standard deviation, $N = 5408$ for all variables. At future identifications, we logged Media Coverage to normalize its distribution.

Per each state i , and week t , the measure of "Media Coverage" is given by:

$$\text{Media Coverage}_{it} = |\text{Homicides}_{it} - \text{Homicides counted by the press}_{it}|$$

Out of the 32,199 drug traffickers' homicides, 23,737 were covered by the press. Coverage is very different across time and geography. For example, although the press covered 76% of the homicides in 2010, in 2007 it only covered 63%. Also, in Oaxaca, 26% of drug traffickers' homicides were not covered, meanwhile, in Querétaro, all of them were covered by the press. Before using this variable, we logged it to normalize its distribution.

Finally, to identify the quality or specific detail of media coverage (*Media quality*), we measured the share of narco-messages that were covered in detail. We classified coverage a narco-message⁵ as "detailed" when the specific content of the narco-message was reported. This means that messages like "Esto les va a pasar a los que no respeten a los grandes jefes de La Familia" (this will happen to those that don't respect La Familia's great chiefs) could be read in their entirety in the newspaper (El Sol de Toluca 2009). Coverage classified as "non-detailed" was when the reporter only hinted at the existence of a narco-message without explicitly quoting its content. The share of detailed messages over total messages is represented by the variable "Media Coverage Quality." Out of our total sample, 75% of the narco-messages were detailed. In this sense we refer to media quality to indicate the degree to which specific information regarding the media message was reported, not quality in the sense of validity. We do note that news articles might have been detailed in other respects (e.g. information on the victim), thus our variable refers only to one aspect of detail.

Descriptive statistics of our measures of homicides, media coverage, quality of media coverage, and credit-taking crime style are shown in Table 1. Of the total events covered by the media, on average only 11% are reported in detail. On average, the media fails to cover 3.12 homicides per week, per state; and there are 5.95 murders per week. Also, on average, drug traffickers take credit for 8% of their homicides. Our time series are mostly normally distributed.

Empirical test

To model bidirectionalities between media coverage of homicides, and actual homicides, we fit a panel regression of each dependent variable on lags of itself and on lags of the dependent variables using generalized method of moments (GMM). This

⁵All news used to compile the datasets were searched in Spanish language.

Table 2. Levin-Lin-Chu unit-root tests.

Variable	Adjusted t^*	p
Media quality	-46.787	.000
Media coverage	-19.886	.000
Homicides	-12.5255	.000
Credit-taking style crimes	-41.577	.000

Thirty-two panels in 169 periods; lags chosen by AIC, max = 4.

specification, generally known as panel vector autoregression (VAR; Abrigo & Love, 2015), is an efficient way to show the co-movements of multiple time series that originated in the macroeconomics literature as an alternative to multivariate simultaneous equations (Holtz-Eakin, Newey & Rosen, 1988; Sims, 1980).

Various alternative estimators based on GMM have been proposed to calculate consistent estimates for dynamic panels, especially in fixed T and large N settings (Canova, Ciccarelli, & Dallari, 2013). Yet, our specification allows to better deal with a large, balanced panel like the one we are making inference from.

Our panel VAR consists of n lags of media coverage and crime in the following two equations:

$$\mathbf{x}_{it} = \beta_0 + \sum_{j=1}^n \beta_{1j} \mathbf{x}_{i(t-j)} + \sum_{j=1}^n \beta_{2j} \mathbf{z}_{i(t-j)} + \mathbf{e}_{x_{it}} \quad (4)$$

$$\mathbf{z}_{it} = \beta_0 + \sum_{j=1}^n \beta_{1j} \mathbf{z}_{i(t-j)} + \sum_{j=1}^n \beta_{2j} \mathbf{x}_{i(t-j)} + \mathbf{e}_{z_{it}} \quad (5)$$

Note that β_1 's and β_2 's are matrices of coefficients for each state i , in week t . We have 32 states and 169 weeks. The terms z ("homicides" or "credit-taking," depending on the identification) and x ("media coverage" or "media quality," depending on the identification) are vectors of exogenous variables that may shift the reaction function up or down, and e_x and e_z are the vector error terms.

Data were nested at the state level. To account for this nesting, we clustered standard errors at the state level. We also used robust standard error estimations in all models. We tested each variable for unit roots or stationarity in using the Levin-Lin-Chu (LLC) (2002) test. Levin, Lin, and Chu (2002) recommend using their procedure for moderate-sized panels, with perhaps between 10 and 250 individuals and 25 to 250 observations per individual, with panels that are strongly balanced, and where the ratio of the number of panels to time periods tends to zero asymptotically. All these features are fulfilled by our database. We allowed an automatic selection of lags using the AIC criteria, with a two month limit (4 weeks). As Table 2 shows, in all cases, we reject the null hypothesis, thus concluding that our series are stationary.

To estimate the optimal number of moments and model, we followed Andrews and Lu (2001). We calculated MMSC-Bayesian information criterion (BIC), MMSC-Akaike's information criterion (AIC), and MMSC-Hannan and Quinn information criterion (QIC) for a series of panel vector autoregressions of order up to 4. We decided to set a maximum of four lags because journalistic sources agreed that one month (about 4 weeks) is typically considered the most a note can last in media attention. Our results, shared in the Appendix, show that homicide models tend to favor an identification of

541 maximun two moments, and credit-taking models tend to favor an identification of
542 maximun four moments. Therefore, following general conventions, we ran all models
543 with a maximun four lags, and for homicide models we ran models with two lags as
544 robustness tests (Appendix). We could improve efficiency by including a longer set of
545 lags as instruments. This, however, has the unattractive property of reducing observa-
546 tions. Note that, in all models, we removed fixed-effects per state using forward
547 orthogonal deviation or Helmert transformation.

548 As with any other dynamic model, the order of the variables is mostly untestable.
549 In other words, there is not a “right” ordering. Order has to be justified based on intu-
550 ition/theory. In our case, we used crime-related variables (homicides or credit-taking)
551 as the first variable in the model because we consider that it may be more exogenous
552 than media coverage. It has been well documented that media operates under criteria
553 of newsworthiness (Gruenewald et al., 2009), thus, homicides may induce media cover-
554 age if crimes are strategically planned to be scandalous and provocative (Nacos, 2002;
555 Wu, 2000). Yet, there is still plenty of debate with respecto to media effects on crime.
556 In any case, because we do not have a definite position as to which variable should
557 be first, we tested all models following a distinct order and the interpretation of all
558 our results remained constant.

559 Every panel VAR is accompanied by a Granger test, which is a joint significance test
560 to check the significance of the coefficient of our lagged impulse and response varia-
561 bles. A large empirical literature supports the use of Granger causality tests (Granger,
562 1969) for testing cycles (Gambacorta, Hofmann, & Peersman, 2014; Huang, Hwang, &
563 Yang, 2008; Jaeger & Paserman, 2008) and argues that it is a relevant tool for crime
564 studies with time series analysis (Hsu & McDowall, 2017; Ramirez, 2013; Saridakis,
565 2004). Note that residuals need to be orthogonal to control for other shocks

566 Our Granger test amounts to testing the joint significance of the coefficients on
567 lagged values of the impulse variable in a regression of the response variable on
568 lagged values of both response and impulse variables. If the signs of the impulse vari-
569 able are significant, we would have found evidence that the response is “Granger-
570 causing” it.

571 We also provide additional post-estimation analysis like non parametric impulse
572 reaction functions, forecast error in variance decomposition, and stability conditions.
573 An impulse-reaction function is a signal processing technique that allowed us to take
574 our defined dynamic system with an input signal, called an impulse, and to describe
575 the reaction of the system to it as a function of time. The forecast error in variance
576 decomposition is useful to measure how relevant each shock is to the explanation of
577 each variable in the equation system. Stability implies the model is invertible and has
578 an infinite-order vector moving-average (VMA) representation. We followed (Love &
579 Zicchino, 2006) to transform the system of equations into a recursive auto-regressor
580 vector with Choleski decomposition of variance-covariance matrix residuals. The
581 impulse variable will be reacting to the response variable if, conditional on lagged val-
582 ues of the response variable, lagged values of the impulse variable have predictive
583 power for the current value of the response variable.

584 It is important to restate that our empirical test, and the goal of this article, is to
585 identify if there are bidirectionalities between media coverage of violent crimes, and

Table 3. Crime and media.

Variable	(1) Homicides	(2) Media coverage	(3) Homicides	(4) Media quality
Media coverage (t-1)	-0.981	0.516***		
Media coverage (t-2)	-0.711	0.437***		
Media coverage (t-3)	-0.664	0.386***		
Media coverage (t-4)	-0.818	0.403***		
Media quality (t-1)			0.258	0.106***
Media quality (t-2)			0.047	0.074***
Media quality (t-3)			0.073	0.011
Media quality (t-4)			0.332**	0.040*
Homicides (t-1)	1.193***	0.056***	1.236***	0.004
Homicides (t-2)	-0.224***	-0.026	-0.265***	-0.004
Homicides (t-3)	0.103**	-0.013	0.070***	0.008**
Homicides (t-4)	0.014	-0.014**	0.014	-0.006*
Granger test ($p > \chi^2$)	8.145*	102.601***	10.707**	28.154***
Hansen's $J \chi^2$	9.531e-30***		5.801e-30***	

* $p < .1$; ** $p < .05$; *** $p < .01$.Instruments : $I(1/4)$.**Table 4. Media and crime style.**

Variable	(1) Credit Taking	(2) Media coverage	(3) Credit taking	(4) Media quality
Media coverage (t-1)	-0.074***	0.602***		
Media coverage (t-2)	-0.059**	0.459***		
Media coverage (t-3)	-0.054**	0.383***		
Media coverage (t-4)	-0.079***	0.396***		
Media quality (t-1)			0.070**	0.068**
Media quality (t-2)			0.039	0.016
Media quality (t-3)			0.020	-0.008
Media quality (t-4)			0.059**	0.030
Credit-taking (t-1)	0.130***	0.072	0.103***	0.040
Credit-taking (t-2)	0.112***	0.086*	0.107***	0.075**
Credit-taking (t-3)	0.045*	0.064	0.054*	0.014
Credit-taking (t-4)	0.035	0.057	0.015	-0.002
Granger test ($p > \chi^2$)	10.174**	8.174*	11.454**	6.712*
Hansen's $J \chi^2$	3.343e-30***		5.317e-31***	

* $p < .1$; ** $p < .05$; *** $p < .01$.Instruments : $I(1/4)$.

crime. More research would be needed to evaluate how other factors may be impacting media coverage. Future articles could address whether high levels of literacy, intense conflict among criminals, violence against the press, or other variables are also presenting correlations.

Results

Table 3 shows the relationship between media coverage and media quality, and homicides. For both reaction functions, we estimated panel vector auto-regressors with four lags (H1 and H3). Columns (1) and (2) show the relationship between media coverage and homicides. Columns (3) and (4) show the relationship between media quality and homicide rates.

Results show that neither media coverage, nor media quality, are significant predictor of homicides, and that media coverage and media quality inconsistently react to

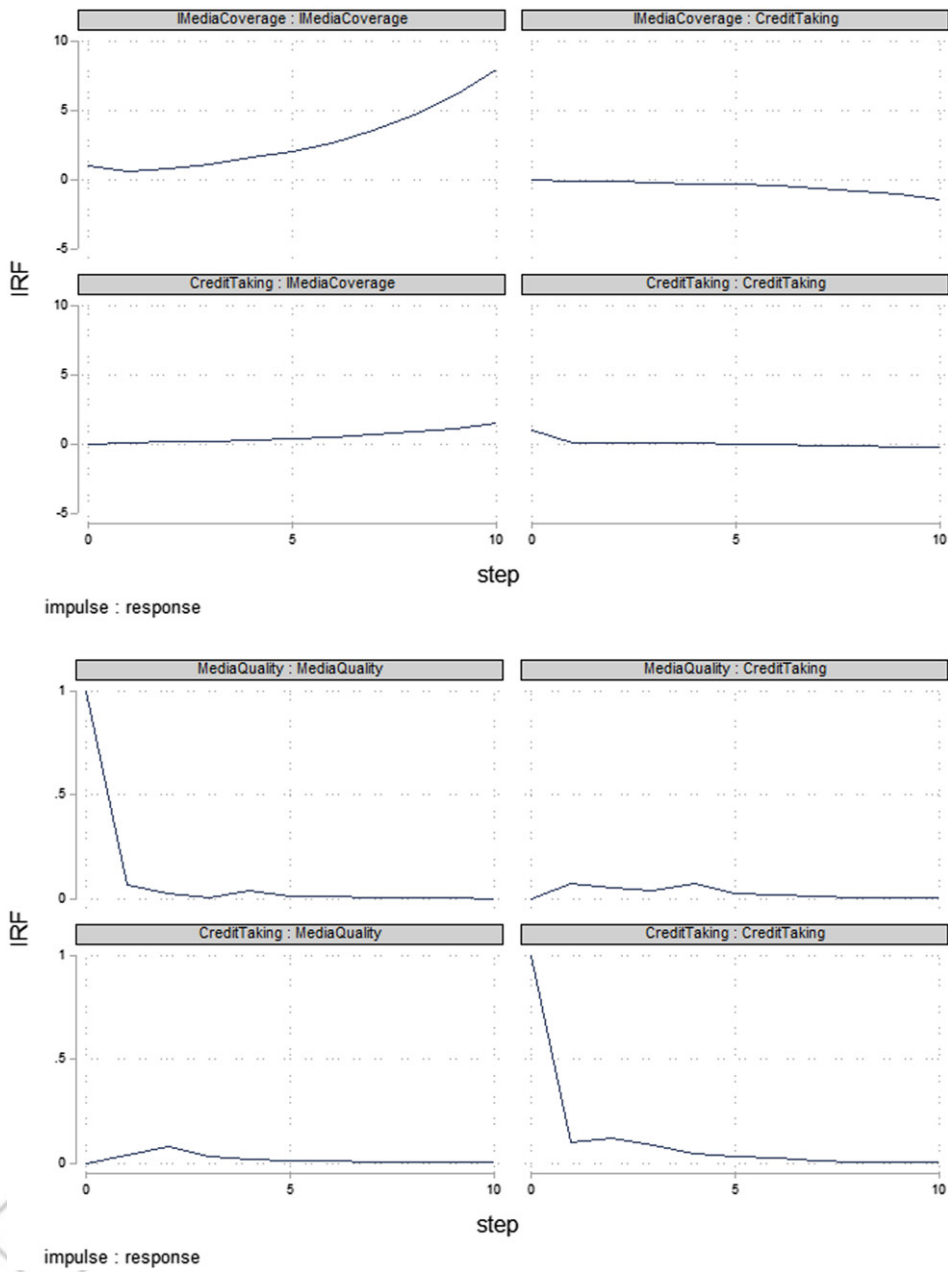


Figure 1. IRFs for credit-taking models.

Q11 homicides. Hansen’s *J* null is rejected in all models, and the Granger tests show that we could find some impact of media coverage and media quality on homidies, but this effect is not stable and small.

Table 4 presents the relation between media coverage and media quality and credit-taking style (columns 1 and 2), and between media coverage and credit-taking homicide style (columns 3 and 4) (H2 and H4).

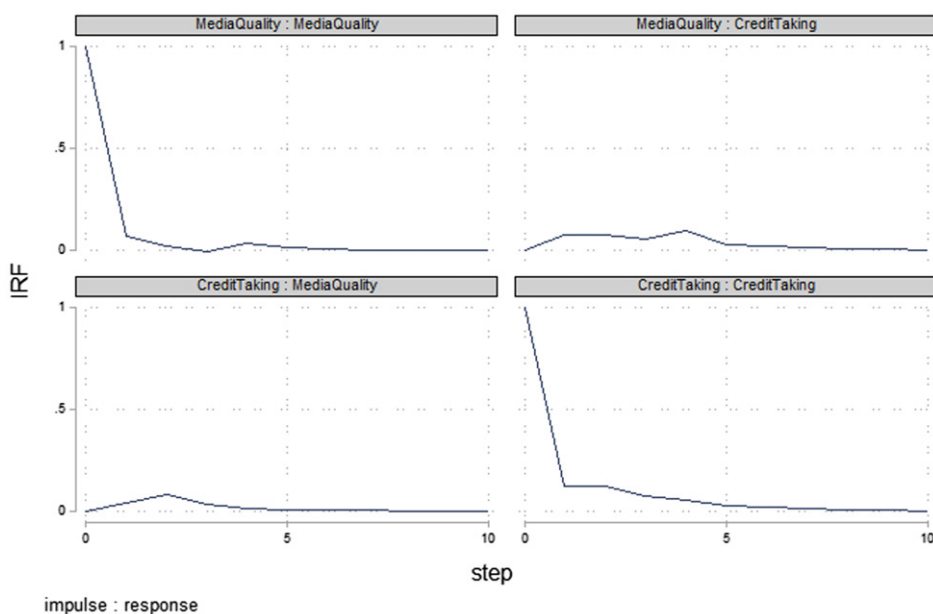


Figure 2. IRFs in areas of operations of drug cartels.

In sharp comparison with previous results, we can see that credit-taking style reacts in a statistically significant way to media coverage. Our Granger test supports these results. We reject the hypothesis that media coverage does not "Granger-cause" credit-taking. The relationship between media quality and credit-taking is weaker but still present. Hansen's J null is rejected in all models. Interestingly, we also see almost no effect of credit-taking on media coverage and media quality.

Overall, our results show that media coverage influences credit-taking style homicides perpetrated by drug traffickers operating at Mexico, but does not motivate more homicides. Violent media could be influencing the characteristics of homicide, but does not cause the homicide itself. This is consistent many recent findings showing that drug traffickers care about being covered by the media and devote energy to getting attention (Atuesta, 2017; Durán Martínez, 2015; Gambetta, 2009).⁶ Drug traffickers could care about media coverage because attention may help trigger specific state reactions (Durán Martínez, 2015). For example, media coverage provides power to drug traffickers' executions by spreading performative acts of fear (Lantz, 2016). Also, the mafia seeks media coverage to portray the toughness and power of its organization (Gambetta, 2009). Militarized international conflicts and civil wars have similar dynamics (Bell, Frank, & Macharia, 2013; Miller & Albert, 2015; Miller & Bokemper, 2016).

⁶In the case of Mexican drug cartels, it has been documented that the drug lord Joaquin Guzman, alias "El Chapo", thought that his media profile was an unfair image of his business (Rawlinson, 2016). Thus, attempting to run his own public relations campaign, he contacted famous actors, like Sean Penn, to direct a movie that was a fair representation of his motivations (The Economist (2016) cited by Rawlinson (2016)).

Table 5. Strength of drug cartel territorial entrenchment.

Variable	Operations		No operations	
	Credit taking	Media quality	Credit taking	Media quality
Media quality(t-1)	0.078**	0.069	0.063*	0.068
Media quality(t-2)	0.061*	0.016	-0.002	0.016
Media quality(t-3)	0.034	-0.014	-0.011	0.005
Media quality(t-4)	0.078**	0.028	0.019	0.034
Credit-taking (t-1)	0.128***	0.042	0.022	0.036
Credit-taking (t-2)	0.103***	0.075*	0.100**	0.075
Credit-taking (t-3)	0.039	0.015	0.087	0.011
Credit-taking (t-4)	0.017	-0.002	0.010	-0.004
Granger test ($p > \chi^2$)	9.864**	4.624	4.422	2.292
Hansen's $J \chi^2$	3.511e-31***		1.991e-31***	

* $p < .1$; ** $p < .05$; *** $p < .01$.

Instruments : $l(1/4)$.

Exploratory results

The following analyses involve those that were not an initial part of our analysis plan, but which were considered as we examined our data and are less theory-driven. As with all exploratory data, these should be considered as proof-of-concept rather than definitive proof as false positive results can be higher for exploratory analyses. However, we do include the 21-word statement of Simmons, Nelson, and Simonsohn (2012) to certify our intent was not to engage in HARKING: "We report how we determined our sample size, all data exclusions (if any), all manipulations, and all measures in the study."

To interrogate our data further, we tested for heterogeneous effects driven by the strength of the drug cartels' territorial entrenchment. If media coverage serves as inspiration for homicide style (H2), as we suggested in our previous models, then we could expect that media coverage may have a larger effect in areas where drug cartels are more entrenched. This is straightforward. We should expect that having more criminal organizations is a proxy of having more criminals that could be susceptible to be influenced by the media.

We determine whether a state is drug cartel territory by identifying the presence of drug cartels in different territories over time. To measure drug cartel presence, we relied on a published big-data framework that uses a text-analysis algorithm to extract web content about recorded criminal activities by subnational economy. The algorithm "reads" digitalized records, news content, blogs, and Google-News indexed content searching for instances in which drug cartel operations are mentioned. The Python crawler was created to extract JavaScript Object Notation using unambiguous query terms to perform text analysis. The final data, cleaned using a hyper-geometric cumulative distribution function, includes 2449 subnational economies, and 178 "actor terms" associated with traffickers and drug cartel organizations. Each actor was classified accordingly as part of one of the 13 drug cartels and as a residual category. We know about 13 drug cartels operating in Mexico for 19 years (1991–2010). Drug cartel organizations operate in 713 municipalities in Mexico, and along most of the US-Mexico border. A more detailed description of the methodology that we followed can be found in the published article (Coscia & Ríos, 2012).

766 This framework allowed us to obtain information about a phenomenon that would
767 otherwise require large scale, expensive intelligence exercises. Most importantly, this
768 procedure helped us to disentangle drug cartels performing violent crimes from drug
769 traffickers that are not violent. Many of the recorded drug cartel operations are non-
770 violent, and consist of peacefully trading, transporting, producing, or cultivating illegal
771 drugs. This data set has also been used to study criminal activity in many other pub-
772 lished articles (Castillo, Mejía, & Restrepo, 2014; Dube, García-Ponce, & Thom, 2016;
773 Holland & Ríos, 2017; Osorio, 2015).

774 Table 5 shows our estimates for the quality of media coverage and credit-taking
775 style for two subsets: areas that are geographical bastions for drug cartels, and areas
776 that are not. The results indicate that, if drug cartels are entrenched, homicides with
777 credit-taking style react in a statistically significant way to media coverage. Meanwhile,
778 locations where drug cartels are not entrenched only exhibit association between
779 media content and the stylistic decisions made by criminals during the first week.

780 The Granger test bolsters these findings, showing that media coverage helps to pre-
781 dict credit-taking homicides in places where drug cartels are entrenched, but fails to
782 do so otherwise.

784 Conclusion

785 Our research was inspired by an enduring controversy: whether exposure to violent
786 media content causes crime by "triggering" delinquency (Anderson et al., 2015;
787 Gentile, 2016; Laser et al., 2007; Phillips & Hensley, 1984) or just provides "rudders"
788 that guide individuals already intending to commit crime (Doley et al., 2013; Ferguson
789 et al., 2008; Ferguson & Colwell, 2017; Ferguson & Dyck, 2012; Savage & Yancey, 2008;
790 Surette, 2013). That is to say, the debate between the adherents of the General
791 Aggression Model (Bushman & Anderson, 2002; DeWall et al., 2011), and the advocates
792 of theories such as the Catalyst Model (Ferguson et al., 2008; Surette, 2013).

793 This debate has been difficult to resolve because measuring copycat crime presents
794 major empirical challenges (Coyné, 2007; Surette, 2014), but also because media con-
795 tent and crime rates may be modeled as a self-reinforcing cycle. This would mean that
796 media coverage may induce criminal events, and criminal events may also induce
797 increases in violent media content, if such events are interesting for audiences.

798 To provide insight into the controversy, we set out to determine whether media
799 coverage of homicides committed by drug traffickers operating at Mexico influenced
800 their homicide rates and crime styles.

801 We were able to overcome the empirical challenges noted in the literature by lever-
802 aging two exceptionally fruitful and unique datasets, and by using simultaneous equa-
803 tion modeling. In addition, we developed an empirical specification that address the
804 possibility that the relationship between media coverage and criminal behavior may
805 be bidirectional. Specifically, we defined reaction functions and Granger causality tests
806 to identify the relationships between media coverage, or media quality, and homicide
807 rates, or crime styles.

808 Our results give support to the "rudder" hypothesis, rather than to the "trigger"
809 approach. We show that detailed media coverage of homicide provides criminals with
810

811 stylistic inspiration (H2 accepted), but is not associated with an increase in homicide
812 rates (H1 rejected).

813 Specifically, we found evidence that credit-taking homicide style tend to react to
814 detailed media coverage. When the media covers homicides in larger detail, criminals
815 increase the use of credit-taking style.

816 Additionally, contrary to common assumptions, a strong Granger causality from
817 detailed media coverage to credit-taking homicide style shows that this relationship is
818 mostly unidirectional (H3 and H4 are rejected in most models).

819 We note that our results for credit-taking style were found stronger for media
820 coverage than for media quality. We generally find this to be reasonably consistent
821 results, acknowledging that there is always some randomness to study results.
822 However, this observation also does caution us to note that the overall effects of
823 media, even for “rudder” effects, are fairly small and should not be exaggerated.
824

825 ***Policy implications***

826
827 Given that our results support a rudder versus imitative understanding of news media
828 effects, this suggests that policies focused on news media coverage of crime are
829 unlikely to reduce the incidence of crime. This suggests that criminal justice authorities
830 ought exercise caution when identifying media as an avenue for reducing crime.

831 This is relevant to policy to the extent that policy makers are increasingly con-
832 cerned to identify means to reduce violent crimes. For instance, imitation or
833 “contagion” effects are sometimes proposed as potential causes of violence ranging
834 from gang violence to mass shootings. This can result in efforts to promote changes
835 to news media coverage in order to reduce the potential contagion effects of news
836 media. We are not necessarily opposed to such efforts, nor claim our study neces-
837 sarily detracts from them. However, we do have some concern that, if evidence for
838 news media causation of crime is weak, policy makers may nonetheless be distr-
839 acted by such concerns from more promising, but less sensationalized potential
840 causes of crime such as poverty, education and occupational disparities and
841 police corruption.

842 In this sense it is important to understand the magnitude of impact that policies
843 directed at news media are likely to have on behavioral outcomes. Particularly if the
844 magnitude of effects are small, it is important not to overpromise impacts which can,
845 paradoxically, cause harm by distracting policy makers from other concerns.

846 Our current analyses focused on news media rather than fictional media.
847 Previous research has generally not been able to identify fictional media as a risk
848 factor for violent crime. However, given that news media reports on real events
849 rather than fictional events, there may have been reasons to suspect that news
850 media might have had more causal impact. Our results suggest news media does
851 not have a causal effect on the creation of crime. Consistent with the Catalyst
852 Model, news media may have a rudder effect in terms of the nature of the crime
853 committed, but targeting news media appears unlikely to reduce crime itself. As
854 such, we recommend that other avenues for policy are likely to be more productive
855 if the goal is to reduce crime.

Limitations and future directions

Although this article has contributed to a better understanding of the relationship between the press and criminal behavior, future research would need to test for causal relationships beyond Granger causality. Experimental data with milder forms of aggression may also be illustrative in determining causality. As our data is developed from a correlational predictive model, it is possible that other predictors not included in our analyses may explain some of the relationships found.

Furthering this area of research is critical to the social sciences and to democratic governance because it exposes the tension between two important values: freedom of the press and the elimination of publicity-seeking crime. In addition to guiding us as we consider this issue, this research will assist with the development of better strategies to combat drug trafficking, copycat criminals, and deviant individuals who aspire to notoriety which understanding which factors have more or less impact.

Disclosure statement

No potential conflict of interest was reported by the author(s).

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Appendix

Table A1. Lag-order selection.

Lag	BIC	AIC	QIC
Homicide-quality			
1	-82.20	22.75	-13.95
2	-72.36	6.35	-21.18
3	-41.33	11.15	-7.21
4	-16.26	9.98	0.81
Credit-taking-quality			
1	-77.55	27.40	-9.31
2	-67.17	11.54	-15.99
3	-44.78	7.70	-10.66
4	-23.51	2.73	-6.45
Homicide-coverage			
1	-84.27	20.69	-16.02
2	-76.30	2.41	-25.12
3	-44.86	7.61	-10.74
4	-23.56	2.68	-6.50
Credit-taking-coverage			
1	-20.05	84.90	48.19
2	-25.39	53.32	25.80
3	-7.65	44.82	26.47
4	2.73	28.96	19.79

Table A2. Two-lag models.

Variable	(1) Homicides	(2) Media coverage	(3) Homicides	(4) Media quality
Media coverage (t-1)	114.399	-50.939		
Media coverage (t-2)	111.067	-49.408		
Media quality(t-1)			0.303	0.102***
Media quality(t-2)			0.103	0.074***
Homicides (t-1)	3.930	-1.167	1.245***	0.002
Homicides (t-2)	-4.034	1.707	-0.160***	-0.002
Granger test ($p > \chi^2$)	0.034	0.003	1.734	1.505
Hansen's $J \chi^2$	5.970e-29***		2.642e-31***	

* $p < .1$; ** $p < .05$; *** $p < .01$.

Instruments: $I(1/2)$.