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ORIGINAL PAPER

A response to "Media representation of hemlock woolly adelgid management risks: a case study of science communication and invasive species control," published in biological invasions online on September 18, 2018

Scott M. Salom · Gina Davis · Joseph Elkinton · Jeremiah Foley · Nathan Havill · Carrie Jubb · Albert Mayfield · Tom McAvoy · Rusty Rhea · R. Talbot Trotter III · Mark Whitmore

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Introduction

The hemlock woolly adelgid (HWA), *Adelges tsugae* (Annand), has become one of the most important and impactful non-native forest insect pests in eastern North America (Foster et al. 2014). Over the past 30 years, the decline of hemlock trees has generated a passionate and energized response by landowners, public and private land managers, and the general public, leading to federal, state, and grass-roots efforts and support to help fight this invader. The management effort for HWA has been covered repeatedly in the popular media, especially when the pest and its impact have spread into new locations. Leppanen et al.

S. M. Salom (✉) · J. Foley · C. Jubb · T. McAvoy
Department of Entomology, Virginia Tech, 216 Price Hall, Blacksburg, VA 24061-0319, USA
e-mail: salom@vt.edu

J. Foley
e-mail: folejr@vt.edu

C. Jubb
e-mail: cjubb@vt.edu

T. McAvoy
e-mail: tmcavoy@vt.edu

G. Davis
Forest Health Protection, Northern and Intermountain Regions, USDA Forest Service, 3815 N. Schreiber Way, Coeur d'Alene, ID 83815, USA
e-mail: ginadavis@fs.fed.us

J. Elkinton
Department of Environmental Conservation, University of Massachusetts, 310 Ag Engineering Bldg., Amherst, MA 01003, USA
e-mail: elkinton@ent.umass.edu

N. Havill · R. Talbot Trotter III
Northern Research Station, USDA Forest Service, 51 Mill Pond Rd., Hamden, CT 06514, USA
e-mail: nphavill@fs.fed.us

R. Talbot Trotter III
e-mail: rttrotter@fs.fed.us

A. Mayfield
RWU 4552 Insects, Diseases and Invasive Plants, Southern Research Station, USDA Forest Service, 200 W.T. Weaver Blvd., Asheville, NC 28804, USA
e-mail: amayfield02@fs.fed.us

R. Rhea
Forest Health Protection, Southern Region, USDA Forest Service, 200 W.T. Weaver Blvd., Asheville, NC 28804, USA
e-mail: rrhea@fs.fed.us

M. Whitmore
Department of Natural Resources, Cornell University, 106 Fernow Hall, Ithaca, NY 14853, USA
e-mail: mark.whitmore@cornell.edu

(2018) evaluated the popular media's coverage of the invasion, with an emphasis on how the media treated the effectiveness and potential non-target impacts of management options. The authors conducted a Google search that identified 674 potential articles, of which 104 were used in an analysis of media reporting. The authors coded the popular media articles using various key word categories related to management effectiveness and non-target impacts. For our comments, we focus on their discussion of management using biological control. Leppanen et al. (2018) concludes that the popular media fails to adequately discuss uncertainties and value judgements associated with the efficacy and potential non-target impacts of HWA biological control agents. They imply that the popular media overstates the success of HWA biological control agents and downplays the risks, and they place the blame for these perceived ethical lapses primarily on scientists and managers.

We agree with Leppanen et al. (2018) that it is unhelpful to overstate the effectiveness of biological control agents, and that the potential risks to non-target species need to be taken very seriously. However, Leppanen et al. (2018) greatly exaggerates the incidence of these factors by conducting a deeply flawed content analysis of the popular media, and by misrepresenting the scientific literature.

A. Media reporting of management effectiveness

A key point worth highlighting is that we are aware that there are indeed some popular media articles that include unsupported positive statements about the effectiveness of HWA biological control. There are advocates for HWA biological control (individuals and organizations) who maintain, *without* scientific evidence, that certain biological control agents are successfully reducing the impact of HWA, and some of this has been picked up by media outlets. We do not condone this, and the scientists and government agencies involved in assessing the efficacy and safety of biological control agents or in preparing outreach publications do not control what these particular advocates say in their own materials or to the press.

Additionally, from a scientist's perspective, reporting of scientific findings in the media is frequently a challenge, as we often read the article for the first time *after* it is published, and only then have the opportunity to identify errors. There is an enormous range of knowledge among the individual journalists, ranging from well-read in science to those just learning about the subject for the first time. A second point is that writers usually have deadlines and often are unable to provide those interviewed with an opportunity to review the article for accuracy. When the senior author of this response has been allowed to review an article, mistakes in facts, misquotes or misinterpretation of statements have been found and corrected. These two explanations were never considered by Leppanen et al. (2018), which chose to focus on the role of scientists.

In regard to the analysis of the media reports, Leppanen et al. (2018) did not follow established content analysis methods developed by social scientists to ensure that results are reproducible and valid. Leppanen et al. (2018) cites Krippendorf (2004) as the basis of their method. This book, in the section titled "Coder Training", states:

> Ideally, the individuals who take part in the development of recording instructions should not be the ones who apply them, for they will have acquired an implicit consensus that new coders cannot have and that other scholars who may wish to use the instructions cannot replicate. Ideally, the recording instructions themselves should incorporate everything that transpired during their development, and the finalized instructions should be tested for reliability with a fresh set of coders. Coders need to learn to work with the recording instructions as their sole guide. They should not rely on extraneous sources of information (e.g., the evolution of the instructions, the intentions of the researchers, emerging yet hidden conventions, and gentlemen's agreements), nor should they confer among themselves as to why they do what they do. Extraneous information undermines the governance of the recording instructions, and communication among coders challenges the independence of individual coders; both make replicability unlikely….. Self-applied recording instructions are notoriously unreliable.

In contrast, the method used by Leppanen et al. (2018) involved coding methods that were developed by the same individuals that conducted the content review, in an iterative process that included revisiting articles as a group to gain consensus. For reproducible content analysis, it is critical that the coders remain isolated from the conceptual development of the study, and the coders should certainly not be guided into gaining consensus. Violation of these basic guidelines leaves plenty of opportunity for the "intentions of the researchers" to shape the results.

Another basic tenet of content analysis is that the coding instrument should contain explicit instructions and definitions for assigning categories to article content such that the results can be replicated by other groups of coders (Krippendorf 2004). There is no indication in Leppanen et al. (2018) that the coders received such an instrument, or that their coding categories were clearly defined. For example, how were coders instructed to distinguish nearly synonymous categories like "promising" versus "hopeful," or "helping/reducing damage levels" versus "limited"? For that matter, Leppanen et al. (2018) does not explain their definition of what they consider a "successful" biological control agent to the reader of their paper, let alone to the coders.

Fortunately, Leppanen et al. (2018) provides a supplementary file that lists the popular media articles that were coded, with information about which text was used to place the mention of management options into a specified category. A cursory examination of this file raises serious doubts about the repeatability of their analysis. For example, Leppanen et al. (2018) coded one article[1] as describing biological control as "successful" based on the following text: "They are also continuing to measure the success of the more than 560,000 microscopic hungry predator beetles that eat the adelgids." A reasonable interpretation of this sentence (and the rest of the article) is simply that the success of biological control was being measured. There is no statement that biological control was a success. Perhaps biological control efficacy should have been coded as "unknown" in this case?

Leppanen et al. (2018) scored another article[2] as referencing "successful" biological control based on the text: "In order to do that, they are also introducing a beetle to the forest that only eats the Hemlock Woolly Adelgid." However, this is simply a statement of the specificity of the predator, not a statement of the predator's success.

Another article[3] coded by Leppanen et al. (2018) was a press release from the University of Tennessee promoting work by Leppanen et al. (2018) coauthors C. Leppanen and D. Simberloff. This article was coded as *not* including a reference to biological control, despite this text: "[Temperature events] like this challenge current hemlock woolly adelgid control efforts. Those efforts have not yet proven successful, possibly because the pests' reproduction has not been sufficiently synchronized with their predators' cycles". A coder with clear instructions and definitions presumably would have categorized this instance of biological control (= "predators") as "unsuccessful". Several other popular articles coded by Leppanen et al. (2018) report on a management strategy that utilized chemical application and biological control together. If an article implied that the entire strategy was effective, how were the coders explicitly instructed to split out the success of chemical versus biological control? Leppanen et al. (2018) states that there is "little room for differing interpretation of language" with their method, but ambiguities such as these suggest this is not the case.

An additional serious problem with Leppanen et al. (2018) is that there were *no statistical tests performed on any of the main effects reported.* The results are simply described as "the majority" or "usually" with no statistical support. In addition to rendering the results uninterpretable, a lack of statistical analysis precludes nested treatment of non-independent "instances" of management coded in the same article. Leppanen et al. (2018) tabulates these as separate data points (e.g. 6 of the 47 articles that mentioned biological control each had two "instances" coded separately), thereby further obscuring any significance of a main effect.

The graphical presentation of "Media representation of effectiveness" in Fig. 1, further misrepresents the incidence of statements in the media that purportedly mention "success" in HWA management. Of the four classes depicted in the bar graph, only "effectiveness not mentioned" is composed of a single, well-defined categorical code, and the other three classes are unspecified groupings of categories. The

[1] http://www.thedailytimes.com/news/treatments-slowing-down-invasive-beetle-in-great-smoky-mountains-national/article_bd2078fd-d0b8-583a-9f36-1b68021ed337.html.

[2] http://www.local8now.com/content/news/Great-Smoky-Mountains-National-Park-in-constant-battle-with-invasive-species–388168552.html.

[3] http://tntoday.utk.edu/2016/02/25/hemlock-woolly-adelgid-winter-activity-possibly-linked-climate-change/.

"unknown/contradictory" class seems to include the categories "with mixed or contradictory results" (3), "unknown" (5), and "pending because current activities are experimental" (1). The class "unsuccessful" may include the categories "unsuccessful" (3) and "likely unsuccessful" (1). Most problematic is the class "at least some success," which appears to lump several other undefined categories. As best that we can determine, this broad class represents media statements coded as: "successful" (15), "likely successful" (6), "helping/reducing damage levels" (5), "possibly successful" (1), "promising" (8), "limited" (3), and "hopeful" (2). The last four categories lumped into this proportion are vague terms that can be interpreted as a lack of success (e.g. "limited') or are aspirational about future success (e.g. "promising", "hopeful"). If these four categories were included with the "unknown" class, and the remaining three were placed in a new "successful" class, there would be 37.7% (20) of the statements classed as "successful", 43.3% (23) as "uncertain" and 7.5% (4) as "unsuccessful". While we suspect that these proportions might be more reflective of media reporting of HWA biological control effectiveness, this exercise is not meant to provide an actual estimate, but to illustrate how vague coding definitions can make conclusions arbitrary and unrepeatable. In the end, the unspecified and inclusive use of the phrase "at least some success," the vague coding definitions, and the overall absence of a rigorous repeatable content analysis leaves the extent to which popular reporting does or does not amplify HWA biological control effectiveness is unknown.

In their Discussion, Leppanen et al. (2018) attempts to make a causal link between the supposed overstating of biological control success in the popular press to the peer-reviewed literature, however the author's arguments for this link include flawed logic and misreading of the literature. The best case that they can make is the statement: "Non-native biocontrol agents have been released in eastern North America with evidence of success pending (Jubb et al. 2018); however, the majority of media articles that address effectiveness indicate some success (Fig. 1)." Even if we ignore the unsupported use of the word "majority," this is a false comparison because scientists and managers define the success of a classical biological control agent differently than is represented in Fig. 1. Biological control success is the last stage of a long, step-by-step process (van Driesche and Hoddle 2000). This includes identifying potential agents in the native habitat of the pest, collecting these organisms and transferring them to an approved quarantine facility, laboratory assessments of host range and potential management impacts, regulatory approval for release from federal and state agencies, documenting the establishment of the agent, and finally determination of impact on the target pest. Although there are several criteria by which success can be measured, a biological control agent in natural systems is not considered successful unless it 1) significantly reduces numbers of the pest species, leading to 2) demonstrable improvement in economic impact and/or ecosystem health (van Driesche and Hoddle 2000). As presented orally by Jubb et al. (2018) [note, that there is no published proceedings for this meeting as cited by Leppanen et al. (2018)], evaluations of the success of HWA biological control are currently in progress, and focused on whether predators are consistently reducing HWA densities. Concepts such as "likely successful," "possibly successful," "promising," and "hopeful" are categorically different. Therefore, drawing a comparison between the definition that scientists use for biological control success and the undefined categories lumped into "at least some success" in Fig. 1 constitutes a fallacy of inconsistency.

Next, Leppanen et al. (2018) states: "Studies in enclosures (e.g., McClure et al. 2000; Lamb et al. 2006; Mausel et al. 2008; Vieira et al. 2013) often cited as proof of control in the field suggest only potential for control." Yet, Leppanen et al. (2018) fails to cite *any* peer-reviewed papers that state that HWA predator enclosure studies are "proof of control in the field." We are also not aware of any papers that make this claim. The next sentence is also inaccurate when it states: "Reports citing success in refereed literature (e.g., McClure 1995a, b, 1996, 1997; Cheah and McClure 1996, 1998; Sasaji and McClure 1997; McClure and Cheah 1999; McClure et al. 2000) are correlative and lack data indicating population-level effects in the field." Review of the documents cited in this statement indicates that five of them are *not* refereed publications (McClure 1995b, 1996; 1997; Cheah and McClure 1996; McClure et al. 2000), and the remaining four that were peer-reviewed do not make statements asserting biological control success. McClure (1995a) reported the results of lab and field evaluations of feeding by a species of mite on HWA wax coverings conducted in Japan, since this species had not yet been released in the United States. Therefore, the article made no statement about biological control success. The other two articles (Sasaji and McClure 1997; McClure and Cheah 1999) mention "potential" or "hope" for effectiveness of *Sasajiscymnus tsugae* (Sasaji and McClure) as a biological control agent, but do not declare it a success. We are not aware of any peer-reviewed articles that report HWA biological control success using the scientific definition outlined above.

Finally, Leppanen et al. (2018) cites Sumpter et al. (2018) as the "only field evaluation in the introduced range." This statement is also not correct. There have been multiple other field studies evaluating the effect of biological control agents on HWA densities (e.g. Lamb et al. 2006, Mausel et al. 2008, Mayfield et al. 2015), including one currently being implemented at multiple sites from Georgia to New Jersey (discussed by Jubb et al. 2018). Leppanen et al. (2018) also misrepresented Sumpter et al. (2018), stating it reported that established predators failed to have an impact. Sumpter et al. (2018) clearly states that the biological control agents *did not establish at any of the sites in the study*, making lack of predator impact moot.

B. Addressing potential non-target impacts

Leppanen et al. (2018) asserts that the popular media does not adequately address potential non-target impacts of HWA biological control. This is a legitimate concern that is shared by many in the research community. However, Leppanen et al. (2018) is structured around an argument that seems to blame scientists of an ethical lapse through intentionally withholding or ignoring information about risks associated with biological control (e.g., see the last three paragraphs of their Discussion). This is a strong assertion to make, and one that is not supported by their spurious assessment of the peer-reviewed literature. Ironically, arguments backed by flawed logic and unsupported claims are the types of statements that may be amplified by the popular media, the issue Leppanen et al. (2018) are fundamentally addressing.

The HWA biological control agents that are currently being evaluated and released have undergone rigorous host range testing. Yet, Leppanen et al. (2018) casts doubt about the thoroughness of these studies by stating that "usually fewer than six" non-target species were tested when assessing the host ranges of HWA biological control agents. This statement is misleading. For *Laricobius nigrinus* (Fender), a biological control agent introduced from the western United States to the eastern United States, a total of seven species were tested in quarantine (Zilahi-Balogh et al. 2002). Selection of the non-target species was done based on the centrifugal phylogenetic method developed by Wapshere (1974), the same method which is used by the vast majority of studies during the safety assessment of potential biological control agents. While the implication in Leppanen et al. (2018) is that seven non-target species is an inadequate number to test, there are simply very few species closely related to HWA within the introduced range that would realistically serve as a prey for *L. nigrinus*. The limited number is indicative of the biology of the system, not the thoroughness of the study. The genus *Laricobius* (Rosenhauer) is only known to feed on members of the Adelgidae in the field. Three of the non-target species tested were in the family Adelgidae: *Adelges piceae* (Ratzeburg), *Adelges abietis* (L.), and *Pineus strobi* (Hartig), each representing major clades within Adelgidae (Havill et al. 2007). Two species of aphids and one scale insect were also tested, adding breadth to the analyses. The results indicated that *L. nigrinus* is a highly specific predator, only completing its life cycle on HWA (Zilahi-Balogh et al. 2002).

Another non-native HWA predator discussed by Leppanen et al. (2018) was *Laricobius osakensis* (Montgomery and Shiyake). In this case, six (not fewer than six) non-target species were tested: three adelgids, one aphid, and two scale insects (Vieira et al. 2011), with results supporting *L. osakensis* as a highly specific HWA predator. The assessment of both *L. nigrinus* and *L. osakensis* followed the strict standards set and approved by USDA, APHIS (United States Department of Agriculture, Animal and Plant Health Inspection Service) and NAPPO (North American Plant Protection Organization).

Leppanen et al. (2018) also includes other erroneous statements. They report that no studies investigated resource competition between HWA predators, yet Flowers et al. (2005) investigated the competitive interactions of two HWA specialists, *Sasajiscymnus tsugae* (Sasaji and McClure) and *L. nigrinus*, and the generalist *Harmonia axyridis* (Pallas). The only significant competitive interactions detected were among conspecifics, whereas intraspecific combinations showed no evidence of interference.

Leppanen et al. (2018) also points to hybridization between *L. nigrinus* (introduced from western North America) and *Laricobius rubidus* (LeConte) (a native eastern species) as a non-target effect that should have been, in their opinion, discussed more in the popular media. Indeed, hybridization has occurred between *L. nigrinus* and the closely related eastern North American native, *L. rubidus* (Havill et al. 2012). The primary prey of *L. rubidus* is the pine bark adelgid, *P strobi*. Upon discovery of hybridization, intensive studies were initiated. Fischer et al. (2015) reported finding *L. rubidus* as well as hybrids on hemlock. Proportions of hybrids did not exceed 15% of the *Laricobius* populations at sites where *L. nigrinus* had been present for up to nine years. In three other studies, Havill et al. (2012), Jones et al. (2015), and Wiggins et al. (2016) reported 12, 11, and 11% hybridization rates in *Laricobius* populations, respectively. Similarly, Mayfield et al. (2015) observed an 11% hybridization rate that remained stable over a 3-year period. The similar proportions of hybridization among these studies conducted at different sites and over time is striking, suggesting stability in the system. Fischer et al. (2015) concluded that *L. nigrinus* might displace *L. rubidus* on hemlock but not on white pine, the main host plant of its native prey, a detail omitted by Leppanen et al. (2018). The discovery and careful examination of hybridization in this system has helped to alert future researchers to this potential impact during the exploratory phase of evaluating biological control agents with closely related native species. The numerous published studies cited above clearly indicate that scientists took this issue seriously, examined it extensively, and reported it multiple times. A careful reading of this literature suggests that hybridization has a low probability of adversely affecting *L. rubidus*. In contrast, Leppanen et al. (2018) exaggerates the risk associated with hybridization in this system by suggesting that hybridization between *Laricobius* species could lead to extinction or that hybrids could become pests, as has been reported in some other ecological systems, almost all involving vertebrates or plants (Todesco et al. 2016). Indeed, we are not aware of any documented examples of a native insect species suffering either of these impacts after hybridization with a non-native species. So, while we agree that hybridization of *L. nigrinus* and *L. rubidus* should continue to be monitored, there is no evidence to suggest that this is an immediate "threat to native species, ecosystems, and biodiversity."

Leppanen et al. (2018) also focuses on the discovery of *Laricobius naganoensis* (Leschen) in *L. osakensis* colonies as an "unreported newsworthy discovery." The opinion that this is "newsworthy" and the non-sequitur that the outreach publication, Havill et al. (2016), lacks "scientific rigor and discourse" *because* it does not discuss this, are baffling. *Laricobius naganoensis* was found among *L. osakensis* collected from Japan and evaluated in quarantine before any field releases were made of *L. osakensis*. This discovery and the way it was handled is actually an example of the extraordinary care and rigor that has gone into minimizing the risk of non-target impacts. *Laricobius naganoensis* is a cryptic species that can only be distinguished from *L. osakensis* by examining the male genitalia or the DNA of dead specimens. Upon discovery of the contamination during assessment of colony genetic diversity, a plan was put in place with the approval of USDA APHIS (Modified Permit P526P-02688) to laboriously remove this species from lab colonies. Attempts were made to perform non-destructive DNA identification of live beetles (Fischer et al. 2014), but these proved unreliable. Using an estimate of the proportion of the unwanted species (*L. naganoensis*) in the colony, adults were separated into rearing colonies of 20 adults to isolate the few *L. naganoensis* from finding mates, and the groups were subsequently reared through several generations (Fischer et al. 2014). Only F_2 or older generations of *L. osakensis* that were demonstrated to have no *L. naganoensis* as parents were released (Fischer et al. 2014). The *L. osakensis* colonies continued to be rigorously monitored and managed in this manner through 2018. As far as the lack of mention of this in outreach publications such as Havill et al. (2016), no *L. naganoensis* could have been released at the time of its publication, so this criticism is illogical, and certainly provides no evidence attesting to this publication's lack of "scientific rigor".

Finally, Leppanen et al. (2018) contains numerous other statements which are false, unsupported, or highly speculative. For the sake of brevity, we do not provide an exhaustive list, but examples include:

Effectiveness and specificity of purebred *L. nigrinus*…reported in media after this discovery may no longer apply owing to hybridization with *L. rubidus*. [There is no evidence to support this speculation. *Laricobius nigrinus* is still found primarily on hemlock associated with HWA and *L. rubidus* is still found primarily on pine associated with pine bark adelgid.]

Additionally, the same rationale sometimes used to justify waiting for biocontrol release outcomes, that impacts may take time to become apparent (Van Driesche and Bellows 1996), might be applied to reconsider some role in HWA control by other natural enemies already present, native (e.g., *L. rubidus, Chilocorus stigma*) and non-native (e.g., *Scymnus suturalis, Harmonia axyridis*) (Montgomery and Lyon 1995; personal observation). [After nearly seven decades since the discovery of HWA in eastern North America, we are unaware of any documented example of native natural enemies becoming more effective, or even marginally effective control agents of HWA. *Chilocorus stigma* (Say) is primarily a scale feeder (Mayer and Allen 1983). There is no peer-reviewed documentation to suggest that it does or might reduce HWA densities or impact. This "personal observation" from an unnamed source does not provide evidence for anything relevant to this discussion.]

Host-specificity testing provides information about how agents perform under controlled conditions but cannot predict how agents will perform under natural conditions in the field or when interacting with species other than those tested. [This is false. Laboratory and enclosure tests of host range do provide predictive power.]

Scientists and other experts studying management techniques may feel pressure to provide solutions to invasive species and pest problems and thus to represent their techniques as successful even when evidence for efficacy is limited or uncertainties are significant. [This statement is highly speculative and represents a serious but unsubstantiated accusation. The scientists working on this problem understand well the importance of not making claims that extend beyond the data they have collected and analyzed.]

Conclusion

We agree with Leppanen et al. (2018) that is important to not overstate the success of the management programs for HWA or any other invasive pest, and that potential non-target impacts are, and will continue to be a vital component of risk assessment. However, the flawed methodology and misrepresentation of the published literature in Leppanen et al. (2018) ultimately says nothing about whether "media representations of risks of HWA management may reflect a failure of responsible science communication", and more seriously, presents a skewed picture of both the peer-reviewed literature and how it is being applied by popular media. Certainly, continued evaluations are needed to assess HWA biocontrol efficacy. The scientists working on HWA biological control are well aware that biological control has not yet achieved control of this pest; nor have we claimed to have achieved it. Successful biological control programs often take decades to realize and, in the case of HWA, may require multiple biological control agents and integration with other management strategies; or biological control may end up not being successful. A continued focus on research and empirically driven discussion is needed to continue to advance our understanding of these complex systems, and the development of tools to manage them. Scientists also need to improve collaboration with popular media outlets, striving for accurate translation of their research. The ecological and economic costs associated with losing eastern hemlock (considered a "foundation" species in eastern North America) are enormous (Ellison et al. 2005; Holmes et al. 2010), and biological control remains one of a limited number of viable low-risk strategies in the multi-pronged effort to minimize and prevent these losses. The discussion of biological control in the context of invasion ecology has been, and can continue to be, very productive. We hope this response contributes constructively to that discussion.

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