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OPINION

The Termite Biological Control Continued Optimism Must Face its Ongoing Absence of Potential

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Abstract

The following piece is unusual in all respects, as it does not follow the rigorous objective nature of a scientific review. As a fair warning, it is instead a rather personal opinion piece, so as to hopefully generate much-needed discussions within a relatively small field of research, yet a remarkably persistent one. Here, I have the opportunity to express a personal perspective and share my experiences about the general field of termite biological control research, and I truly hope that the obvious frustration permeating from these lines will be processed by the reader as a friendly -yet realistic- analysis of the termite biological control endeavor. From interacting with many colleagues in the past two decades while sharing my pessimistic views on the topic, I noticed two opposite reactions: the first one was in the lines of "*we all already know, Thomas, why do you keep beating on that dead horse*?," while the second one was more like "*You can't say that Thomas, we just need more innovative research to make it work*!" Such discrepancies in reactions reflect the two mutually exclusive realities that I personally have to interact with regularly, as a solicited reviewer and as a reader keeping up with the termite literature. Consequently, the writing process of the following opinion is inherently tainted with value judgment -some would say 'incendiary'- but I find it most genuinely necessary so as to move the discussion into a non-candid realm, which remains at the core of the issue. Finally, while I may be wrong on some of these statements, as a putative research breakthrough may change the equation in a distant future, I may stay correct for a foreseeable one. Overall, I hope this piece will help any new venturer on this topic to be skeptical of the accumulated literature and to take the time to look at the larger picture before engaging in such research. In this opinion piece, 1) I review the termite biological control from an academic perspective, and 2) I then discuss what happened to this field of research since 2011 while highlighting the lingering problems surrounding termite biological control research.

Historical overview of termite biological control

Termites as pests and the search for alternative control methods. Termites represent a diverse group of eusocial cockroaches (Krishna et al., 2013) that have important ecological functions as they contribute to unlocking cellulose stored in wood and are also important actors in bioturbation functions in ecosystems (Bar-On et al., 2016; Jouquet et al., 2016; Eggleton, 2020). However, among the ~3,000 described species, a small fraction (~180 species) is known to have a significant pest status, (Krishna et al., 2013), and less than 80 species, including some highly invasive ones, are responsible for most of the annual $40B estimated cost worldwide (Rust & Su, 2012; Evans et al., 2013). As a result, despite being a relevant model system to study the evolution of eusociality in insects (Nalepa, 2015; Chouvenc et al.; 2021; Hellemans et al., 2024), termite research around the world has historically focused on pest control solutions (Chouvenc et al., 2018; Su & Lee, 2023). Termite control approaches and pest management technologies used commercially have therefore

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changed drastically over the past century, from arsenic dusts to organophosphates and fumigants, then to neonicotinoids and phenylpyrazoles, to ultimately reach the establishment of benzoylurea Chitin Synthesis Inhibitor (CSI) bait formulations (Randall & Doody, 1934; Su & Scheffrahn, 1998; Su, 2019; Su & Lee, 2023).

Termite biological control initial research field and its ongoing limitations. Although soil termiticides were used for termite control for decades, the withdrawal of chlordane and heptachlor, among many other chemicals, in the late 1970s-early 1980s, precipitated the research to look for alternative or novel approaches for termite management solutions (Chouvenc et al., 2011). In addition, with the rise of chemophobia at that time (Ropeik, 2015), a strong push to find "green alternatives" emerged, from the general public, which grew aversions to chemicals, from environmentally sensitive scientists and stakeholders, and from various business ventures. Also, the perceived and actual success of the use of natural predators, competitors or pathogens against some insect pest species in crop systems provided an incentive to support the idea of the application of biological control solutions against termite pests (Georgis et al., 1982; Hussey, 1985; La Fage, 1986; Logan et al., 1990; Grace, 1997; Gelertner & Lomer, 2000). Such motivation has led to an extensive field of research investigating potential entomopathogens as biological control agents for termite pest management applications.

However, while the motivations behind the push for research efforts on termite biological control were initially genuine, the rationale for the use of such an approach was based on a fundamentally flawed assumption that has long been shared among scientists in this field of research (Chouvenc et al., 2011). It was generally assumed that the environmental conditions within a densely populated termite nest, with relatively high temperatures and humidity, would favor the emergence of epizootics by a self-replicating pathogenic agent, as the social interactions would facilitate the spread of the disease among all individuals (Toumanoff & Rombaut, 1965; Kramm et al., 1982; Lai et al., 1982; Hanel & Watson, 1983; Grace, 1994; Wells et al., 1995; Zoberi, 1995; Delate et al., 1995; Boucias et al., 1996; Jones et al., 1996; Rosengaus & Traniello, 1997; Milner et al., 1998; Culliney & Grace, 2000; Wright et al., 2002; Lax & Osbrink, 2003; Myles, 2002; Sun et al., 2003; Siderhurst et al., 2005; Chouvenc et al., 2008; Sindhu et al., 2011). In addition to this core issue, it was not initially considered how to translate technology and positive experience from a biological control approach successfully applied to crop systems against solitary pest insects, to an urban setup against eusocial pest insects (Staple & Milner, 2000; Langewald & Cherry, 2000; Chouvenc et al, 2011).

Such a lack of consideration remains, to date, deeply problematic as it continues to set numerous research projects toward promising, yet unrealistic endeavors. Following the initial incentive to find alternative termite control methods, an extensive body of termite biological control research had accumulated by the late 2000s. However, expectations fell short, as no commercial application was ever successfully implemented (Culliney & Grace, 2000; Staple & Milner, 2000; Chouvenc et al., 2011). In addition, evidence accumulated against the assumption that termite nests were favorable environments for epizootics (Schmid-Hempel, 1998; Rosengaus et al., 1998; Traniello & Rosengaus, 2002; Grace, 2003; Milner & Pereira, 2007; Chouvenc et al., 2008; Chouvenc & Su, 2010; Rosengaus et al., 2011; Chouvenc et al., 2013). It became apparent for involved researchers that hopes for the implementation of biological control solutions for termites were essentially wishful thinking or "*just a dream*" as stated in a concluding report (Staple & Milner, 2000). Indeed, critical aspects of termite biology making the implementation of termite classic biological control challenging were systematically disregarded by the bulk of the research in this field, despite the ongoing absence of successful field assays for practical commercial use (Staple and Milner 2000, Grace 2003, Chouvenc et al. 2011).

In order to bring a much-needed reality check on the feasibility of termite biological control, Chouvenc et al. (2011) provided a comprehensive overview of the entire body of research performed on termite biological control between 1959 and 2010. I here provide a brief summary of it so as to establish the context for a 'more-than-a-decade-later' update. In the five decades prior to 2010, ~230 publications on termite biological control were published, describing experimental assays using various strains of entomopathogenic, nematodes, bacteria, and fungi against ~60 different termite pest species around the world. The majority of the studies focused on the impact of entomopathogenic fungi such as *Metarhizium* and *Beauveria* against *Coptotermes* and *Reticulitermes*, two subterranean termite genera with a major pest status. The bulk of the published studies often reported straight-forward laboratory screenings for virulence, where the mortality of small groups of termites was assessed over time, when held in containers and exposed to a pathogenic agent suspension. More critically, as the authors often interpreted their results from mostly biologically irrelevant bioassays (Lenz, 2009), oversimplistic optimism prevailed, instead of placing the study in a realistic context. As a result, Chouvenc et al. (2011) synthesized the reasons why termite biological control had essentially failed. Here, these reasons are reformulated and updated for effective communication purposes to future termite biological control prospectors.

Overoptimism and publication bias. The push for a "green alternative" is often rooted in a genuine desire to minimize the negative human footprint on ecosystems, by reducing the use of harmful chemical pesticides. However, many have used this single argument to justify the need for research on termite biological control, leading to overoptimism when interpreting datasets despite an absence of evidence that would support realistic implementations against termite pest species at the commercial level. Interestingly, despite that is much easier to kill termites in the laboratory than keeping them alive (Becker, 1969; Lenz, 2009; Chouvenc, 2023), simple screening tests have led to the conclusion that a pathogenic agent was highly promising, while in all reality, the authors just showed that they could kill a few insects in a Petri dish in a biologically irrelevant context. This overoptimism had dramatic consequences on the perception that a reader may have while investigating the potential for termite biological control, which may motivate one to give it a try. After all, if all of these studies say it's very promising, then maybe "*I*" can make it work?

Such perception has resulted in a unilateral positive publication bias, leading the steady flow of newcomers to the field to be trapped in a vicious circle of reinforced misguided optimism. Such problematic publication bias has continued to give the inflated impression of a general consensus about the bright future of termite biological control, solely from laboratory preliminary screenings. Ironically, among the few field assays ever performed, most failed or were inconclusive, indirectly demonstrating the lack of reliability and cost-efficiency of the approach. The vast majority of these studies were not published in the peer-review literature but in internal reports instead, if ever, leading to the absence of accessible information documenting the lack of efficacy for termite biological control (Chouvenc et al., 2011). Such observations are in line with the paramount importance of the publication of negative results so as to prevent future research from repeating mistakes (Stern & Simes, 1997; Dickersin, 2005; Boulesteix, 2010; Milnaric et al., 2017).

Lack of consideration for complex termite biology. Most of the studies on termite biological control were simple laboratory screening tests. Often, authors isolated various strains of a pathogen from soil samples in their locality, cultured them on artificial media, and exposed a suspension of the potential pathogenic agents to a small group of field-collected termites in Petri-dishes or vials, and in rearing conditions far removed from the nesting habit and social structure of the test species in field conditions. While such an approach is initially necessary for the selection of promising microbial strains for any pest insects, the experiments often stopped at the screening level and were never investigated further or evaluated in the field. More problematic, pathogenic strains were often tested against termites in biologically irrelevant conditions, as termites were not able to display their suite of inherent behaviors and were invariably exposed to unrealistically high-density pathogens. The lack of biological relevance in empirical studies is a recurrent observation in any laboratory assays of pathogens against termite pest species, where group size, termite density, experimental environment, behavioral avoidance, and foraging distance were not taken into account, as discussed at length by Lenz (2009) and Chouvenc (2023). Consequently, a series of confounding factors from oversimplistic bioassays often prevent proper interpretation of the results at the termite colony level (DeSouza et al., 2001; Chouvenc et al., 2011)

By 2010, termite biological control had been investigated by more than one hundred laboratories around the world, which included thousands of authors and co-authors. However, except for a handful of individuals, the vast majority of the science generated in this field of research was produced by laboratories with no robust background knowledge in termite biology (Chouvenc, 2023), as most researchers typically came from either a general crop insect pest background, or from a microbiology background. This lack of termite biology expertise has led to the accumulation of studies that did not consider the complex social structure of termite colonies, the importance of the microbiome of the nest material, the importance of group effect and foraging distances on defense mechanisms within the context of the overall immunity of the termite colony (Liu et al., 2015; Bulmer & Stefano, 2022; Hassan et al., 2024). Termites have evolved for millions of years in microbial-rich environments, and they have therefore evolved multifaceted disease resistance mechanisms that prevent epizootics in the first place, which allow them to thrive in these environments (Cremer et al., 2007; Chouvenc & Su, 2010; Rosengaus et al., 2011). The simplistic assumption that a strategy which worked for a solitary insect pest in a crop system would readily transfer to a eusocial pest in a structural context has therefore repeatedly led to an unrealistic experimental approach and to erroneous conclusions. To date, research groups with little-to-no termite background continue to bring their own experience of classical biological control to an insect model system where such rules do not apply (Chouvenc & Su, 2012).

Lack of implementation route. A commonality across the termite control field of research is the fundamental lack of investigation about how preliminary laboratory results may find their way to a tangible application for the remediation of termite pest problems at the commercial level. Few authors have provided considerations on the technical and logistical difficulties for scaling the experiment to a field assay. Most studies inevitably indicated that "future research" would include a plan for a field trial, but invariably failed to follow through, with few exceptions. Authors also failed to address how such a pathogen agent could be mass-produced as a stable commercial formulation (with a shelf life), and how it would be implemented and applied by pest control providers, which would be the end users of such technology. The few that tried to address such issues often quickly terminated their endeavor, realizing the impracticability of such an approach, and eventually provided a compelling economic argument: termite biological control is not cost-effective, and it therefore has no chance to become a commercial success (Culliney & Grace, 2000; Rath, 2000; Staple & Milner, 2000; Grace, 2003). However, the universal lack of effectiveness upon implementation is here the important point to keep in mind, regardless of the potential associated cost (Grace, 2003). Noteworthily, a few studies partially successfully used pathogens to eliminate field colonies of one-piece termite species. Such termite species have limited, self-contained nests where most of the individuals are accessible within a single piece of wood or mound. However, it was too labor intensive, too costly, and arguably, the injection of a soapy foam formulation in the limited gallery system would have resulted in an equivalent outcome (Chouvenc et al., 2011), and the approach would be difficult to scale up (Logan et al., 1990).

Initial conclusions about termite biological control prospects in 2011. Ultimately, Chouvenc et al. (2011) painted an unflattering portrait of the field of termite biological control research, by providing a cautionary tale that any attempt to implement classic biological control for termites would be a potential lost cause and that ongoing screening for new pathogens would be futile. By pointing out all the dead ends explored by research groups over the past half century, and by providing compelling evidence of the termites' ability to prevent epizootics, Chouvenc et al. (2011) hypothesized that the ongoing effort in this field of research would eventually dim out, so as to refocus efforts and resources to other research projects. As evidenced by the existence of the current review, this prediction was inaccurate.

Revisiting termite biological control once again. To date, there are still ongoing efforts to pursue the termite biological control endeavor. Unfortunately, the research in this field accumulated in the past decade has continued to mostly ignore the initial shortfalls, problems and limitations which were previously raised by predecessors. As warnings and concerns initially raised by many authors were still mostly being ignored, it has led to an ever-growing number of research publications in this field. In addition, an uncitable long list of careless reviews on the general use of entomopathogens against pest insects have listed termites as valid targets for biological control approaches, perpetuating the inaccurate consensus that termite biological control is at reach.

As I now write this current update more than a decade later, this is obviously not a *postmortem* analysis, and it reveals that history has continued to repeat itself. I here pass on the opportunity to indulge in the sterile exercise of listing each of the 240+ papers published since 2011 (Figure 1), which would have no other function than pointing the finger at unsuspecting authors. I here instead recontextualize the recent termite biological control research within the changing nature of academic publication.

Termite biological control research: what has happened since 2011?

Publication bias perpetuating erroneous concepts. While a handful of review articles focusing on termite biological control were published by the late 2000s, more than thirty "reviews" were published since (Stewart et al., 2010; Remadevi et al., 2010; Chouvenc et al., 2011; Sindhu et al., 2011; Xu et al., 2013; Dutta et al., 2013; Roy & Muraleedharan, 2014; Qasim et al., 2015; Kuswanto et al., 2015; Sindhu et al., 2017; Baïmey et al., 2017; Nakai & Lacey, 2017; Verma et al., 2018a, 2018b; Sahayaraj, 2018; Kumar, 2018; Ahmad et al., 2018; Devi, 2019; Hernández-Rosas et al., 2019; Ahmad et al., 2019; Mubeen et al., 2019; Mohammed Ali et al., 2019; Mishra et al., 2021; Rana et al., 2021; Chellappan & Ranjith, 2021; Pandey et al., 2021; Askary et al., 2021; Chen et al., 2022, 2023; Coêlho et al., 2023; Awasthi et al., 2024; Hassan et al., 2024; Bayen et al., 2024; Ghode et al., 2025; Barthi et al., 2025). This major uptick of reviews about termite biological control may at first appear to reflect a renewed desire to "finally make it work." Unfortunately, a rapid survey of the authors reveals that with very few exceptions, few have ever published on any aspects of termite biology. This recent bloat of reviews about termite biological control reflects the general ongoing inflation of scientific publication (Chouvenc & Su, 2015; Nissen et al., 2016; Hanson et al., 2024; Anta, 2025), with the ever-increasing push to publish (or perish), and the overall push from some publishers to aggressively invite scientists to write contributions for "special issues" often involving guest editors (Koerber et al., 2023). In addition, the 2020 Pandemic lockdown may also have led many academics to refocus their time toward writing review papers on topics that seemed promising research avenues, despite potentially minimal experience in termite research, inherently making their review susceptible to the preexisting publication bias. Finaly, more than a hundred additional reviews and book chapters about general biological control against various insect pest species (primarily from crop systems) have continued to consistently cite termites as good target pests for biological control implementation. Invariably, opinions were shaped from the overoptimistic literature, citing precedent work and reviews that considered termite biocontrol as very promising, or sometimes worse, claiming erroneously that termite biological control was successfully implemented as a commercial product (Sandhu et al., 2012, among many other non-termite centric references). In more recent developments, there is an increased use of Large Language Models (LLM), also known as "AI chatbot" in the scientific literature, allowing for the automatic summarization of scientific content, so as to rapidly generate "review papers" (Resnik & Hosseini, 2025). Such ethically dubious process will not only repeat the already existing content with no added value as a pseudo-plagiarism process, but it will also ineluctably perpetuate the existing overoptimism and publication bias in termite biological control research.

Unfortunately, the ongoing reinforcement of the publication bias remains deeply problematic as it continues to perpetuate the false idea that successful implementation is at reach. I would here argue that it has become an unavoidable literature pollution, representing a large background noise to the rest of the termite research literature. One other pernicious aspect of pushing for the publication of positive results may also come from the funding and promotion opportunities in academic research, where there is an incentive to oversale one's research outcome despite its potential irrelevance. The peer-pressure to produce positive stories within the publication bias context has a deep cultural impact on how we produce science as entomologists (Chouvenc & Su, 2015). I here cannot help but to anecdotally illustrate this issue from my own personal first interaction with the peer-review process. Chouvenc et al. (2008) showed that when relatively large groups of subterranean termites were provided with an environment simulating their nest habitat and foraging distances, termites not only prevented the spread of a fungal pathogen among nestmates, they were also able to inhibit the survival of the pathogen within their nest structure, resulting in the termite killing the fungus, not the other way around. It demonstrated the importance of taking into account the complexity of termite biology when testing for the potential for implementation. To my surprise, one of the anonymous reviewers commented: "*it is unfortunate that the authors were not able to make it work*." Such comment came to me as a most needed wakeup call about the general bias of the scientific community's expectation toward positive results. Unfortunately, the overwhelming absence of negative data within the literature directly highlights that the inertia of overoptimism and its legacy continue to carry over into today's research.

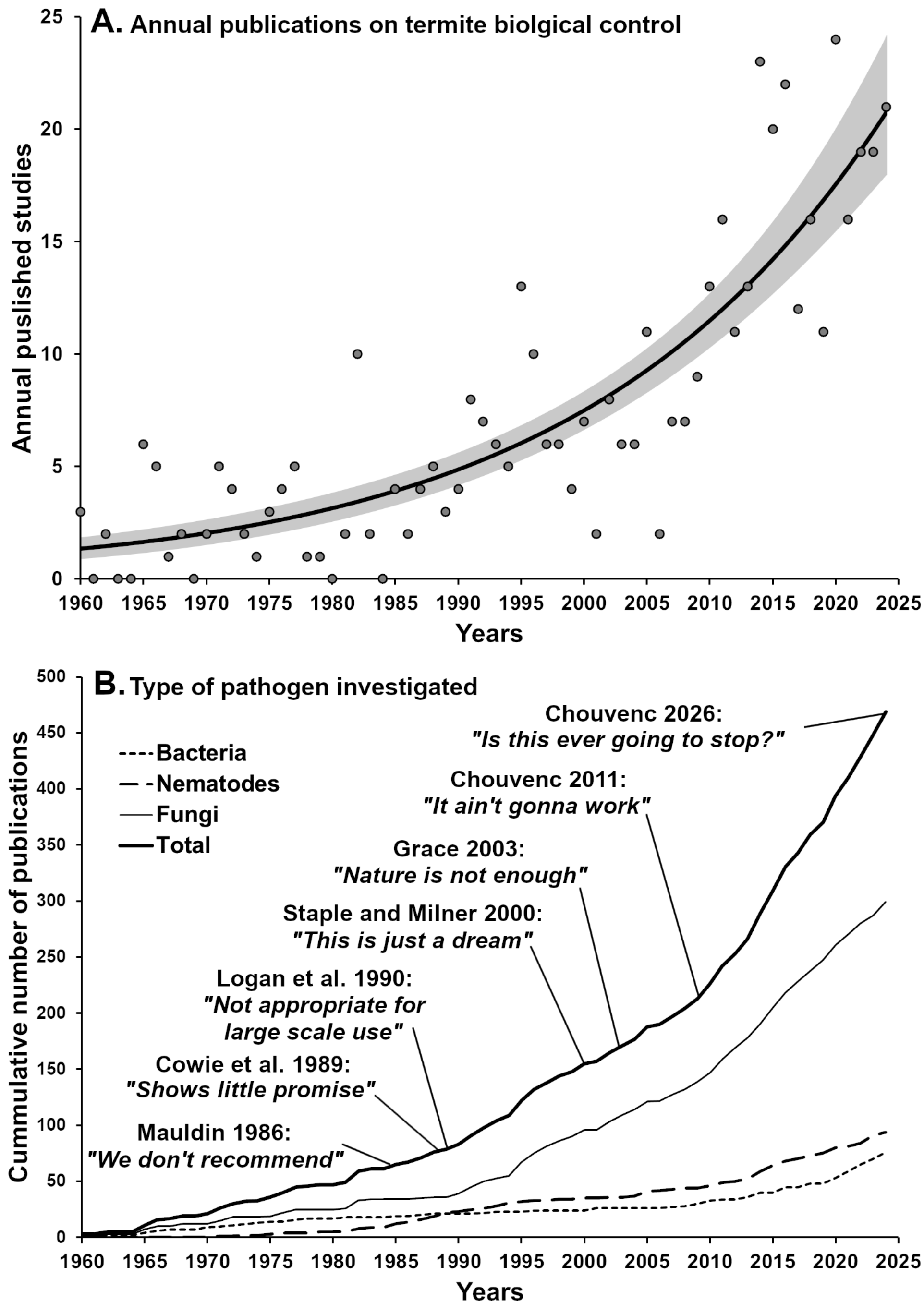

Fig 1. Publication patterns of experimental studies focusing on termite biological control from 1960 to date (review papers not included). **A**. Annual publication rate, following an exponential growth over time (95% interval). **B.** Type of pathogen investigated over time, updated from Chouvenc et al. (2011) with quotation from past researchers (Mauldin, 1986; Cowie et al., 1989; Logan et al., 1990; Staple & Milner, 2000; Grace, 2003; Chouvenc, 2011, current paper).

Increasing publication rate of experimental studies with no implementation in sight. Contrary to the expectation from Chouvenc et al. (2011) that the research for termite biocontrol would silently die down, the publication rate of experimental assays testing a pathogen against a termite pest species instead grew exponentially in the following decade (Figure 1A). Such an observation reveals that new-comers *de facto* ignored the accumulation of evidence against its feasibility and the warnings from predecessors (Figure 1B). Remarkably, while it took ~50 years to publish a little over 230 studies on the topic, it took less than an additional 15 years to publish an equivalent number of studies repeating the same approach (~480 studies as of 2025). On brand, such studies primarily focused on the same fungi, nematodes and bacteria genera that were investigated the half-century prior, but with strains isolated in the researchers' locality.

While I could attempt to duplicate the effort initially performed by Chouvenc et al. (2011) to list and re-interpret all the literature published since, it would be pointless. The narrative of the majority of these studies is rather scripted and could be (absurdly) summarized as: 1) Termites are bad. 2) Pesticides are bad. 3) We "need" green technology. 4) We have a great novel idea: how about biological control? and 5) Let's see what is in my backyard as natural enemies.

As a result, insects in Petri-dishes were killed in non-biologically relevant conditions with absurdly high densities of pathogens, *ad nauseam*. Conclusions inevitably highlighted how promising such preliminary assays were toward successful biological control, yet always failed to discuss any viable pathway toward a cost-effective implementation, and overwhelmingly failed to later proceed toward field assays. In all reality, the positive publication bias has doubled in the past decade or so, with still no possibility in sight toward cost-effective large-scale implementation. All this renewed unjustified effort just gave further credibility to a point I jokingly made in conclusion of the second international Formosan subterranean termite symposium in New Orleans (LA): "*it ain't gonna work*" (Chouvenc, 2011). It is therefore with much more seriousness and concern that I now wonder: "*is this ever going to stop*?"

The continued termite biological control research comes at a cost and a loss. A rapid survey of the most recent 30 publications (to date) shows that there are in average 5.6 ± 1.6 authors per paper, which again reflects a trend in entomology of an increase of number of authors per publication over time (Chouvenc & Su, 2015). Taking into account the salaries for all researchers involved in a project, the cost of equipment, consumables, overhead, and publication fee, I here conservatively speculate that more than US $25 million were spent globally on termite biological control research since 2010. In addition to the time and efforts provided by the research teams, each manuscript must be peer-reviewed, imposing an additional burden to reviewers and editors who volunteer their time to evaluate all of these studies. It is also therefore possible that many additional submitted manuscripts were never published, leading to a significant underestimate of the actual cost of global termite biological control research since 2010. Finally, for any experienced termite biologists, such a high publication rate is an unnecessary constant background noise, a nuisance that detracts and sometimes frustrates (admittedly, one of the motivations for me to write this current piece is to not have to be asked to review one of these manuscripts ever again).

Research beyond traditional termite biological control. In addition to the traditional biological control approaches discussed so far, some authors reframed termite biological control prospects within an integrated pest management context (IPM) under the erroneous idea that complementing different tools to eliminate termites is "integration". The principal tenants of urban IPM for termite control are that implementation of any solution(s) must be cost-effective for the provider of the service and the recipient of the service (Su & Scheffrahn, 1998). As termite biological control remains inherently cost-ineffective, integrating it with other approaches would not qualify as termite IPM. Regardless, the idea of combining multiple tools is also not new. The presumably abandoned interest in using pesticides combined with pathogens (Zeck, 1992; Boucias et al., 1996; Ramakrishna et al., 1999) was all but abandoned. Sindhu et al. (2011) supported the idea of continuing the study of termite biological control while using synergists (pesticides) to favor naturally occurrent pathogenic agents to take over colonies weakened immunity (Wright & Lax, 2013; Wang et al., 2013; Yii et al., 2016; Meena et al., 2017; Riaz & Raza, 2018; Afzal et al., 2018; Raza et al., 2019; Shoaib et al., 2025). Such an approach not only negates the initial argument for "green alternatives," its implementation remains impractical, unreliable, and not cost-effective.

Similarly, the idea of using bacterial toxic metabolites extract or botanical extracts as biopesticides has received intense interest under the argument that it would fall under "biological control," so that it could fit within the "green" or "natural product" approach. It would not only negate the need for a self-replicating agent, but it would also directly emulate termiticide application protocols, while failing to discuss how it would be formulated, applied, if it would be repellent, how it would be stable pre- and post-application, and how it would interact (= decompose) when exposed to the environmental microbial community, heat, sun, and humidity. Ultimately, any "novel" approach from the recent literature is, at most, a proof of concept that can be demonstrated in the laboratory (*i.e.,* killed insects in Petri dishes) but has little-to-no potential to be scaled cost-effectively for commercial purposes. On that matter, the large wave of studies specifically investigating botanical extracts against termites can mostly be attributed to Verma et al. (2009), which is to date, the most cited review paper on "termite control" (or lack-there-of, I would argue). This review, among others, misled thousands of researchers to buy into the erroneous idea that botanical extracts were the future of termite control, with no practical implementation in reach. The field of botanical extract research against termite pests deserves its own critical (= brutally honest) review, as the publication rate of botanical extract studies against termites over the past decade outpaced manyfold the one from microbial control. I here pass on the opportunity to quantify the publication bias in this field. It would receive the exact same counter argumentation as the one presented here against termite microbial control, for its identical lack of potential cost-effectiveness in protecting structures and crops against termites. In addition, such studies usually claim that such botanical extracts are “safe” because they are “natural.” Such claims are sometimes dubious and actually rarely (ever?) investigated accordingly (Sindle & Martin, 2021).

Finally, some studies have investigated ways to bypass termite immune barriers that have prevented pathogens from self-replicating within the termite nest (Scharf, 2015; Mogilicherla et al., 2023; Hassan et al., 2024; Chen & Li, 2025; Zhao et al., 2025). While the argument for such approaches appears promising, all experimental attempts were also limited to small groups of termites isolated in a Petri dish in the laboratory, with little-to-no possibility to scale it up to field conditions or colony-wide approaches. Thus, while such studies acknowledged the limitations of termite biological control, they still fall short of acknowledging the limitations and complexity of the endeavor, which will, again, likely not result in cost-effective implementations.

Conclusion

The field of termite biological control research has failed to produce any tangible solutions for decades. It is now paramount that any future prospective scientists looking at termite biological control take note of the continuous failure of this approach by taking into account the limitations imposed from experiments with limited biological relevancy. Implementation would be labor-intensive, unreliable, expensive, not effective, and unlikely to be scaled up toward commercial applications. This *impossible dream* (Staple & Milner, 2000) is continuously fueled by a systemic publication bias. As a result, while biological control research efforts against termite pests may no longer be justified, it is likely that such research will continue to grow exponentially in the years to come despite repeated warnings.

I also want to emphasize that the argument for the “need” for a green technology, especially against primary subterranean termite pest species, is in part disingenuous. Chitin Synthesis Inhibitor (CSI) baits are arguably the most environmentally friendly approach available for termite management and are cost-effective when implemented properly, which has led to a significant reduction of the use of pesticides and of their toxic environmental effects (Su 2019, Su and Lee 2023). Any future research direction dealing with termite control must be rooted in realism and avoid idealistic biases so that one must avoid the continuation of the mistakes of the past (Chouvenc, 2023). Readers must sort through literature polluted with mostly irrelevant and unusable overoptimistic studies about the potential for termite biological control. As a result, putting an end to the vicious circle of publication bias will be most difficult in the decades to come. Therefore, if you are a reviewer for such termite biological control manuscript and you stumbled upon the current piece as part of your reviewing effort, you may evaluate such manuscript with a grain of salt. Ultimately, it will be the task of the scientific peer-review community to understand the nature of this failed endeavor so as to redirect unaware researchers toward more constructive scientific pursuits. I would thus cautiously hope that 15 years from now, I will not have to repeat my own failed endeavor to move the discussion elsewhere.

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Many thanks to all the colleagues who have shared their perspectives with me on this topic, as supporters and detractors of the current piece will recognize. While the strong views as written in this article are mine, the sentiment is shared by many within our field. I also would like to acknowledge you as the reader, who may be considering submitting manuscripts toward termite biological control. If this piece was sent to you as justification for a manuscript rejection, I appreciate your open mind and willingness to consider its message. Please do not take anything personally: while the language in this article is intentionally direct, it is not the goal, mine or anyone else’s, to upset you or dissuade you as a scientist. But rather, to provide comprehensive context for anyone approaching the topic with a healthy dose of skepticism, so as to hopefully move the termite control research discussion onto realistic grounds.

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