

The Identity Crisis in Language Motivation Research

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Abstract

The 40th anniversary of the *Journal of Language and Social Psychology* occurs around the corner of another anniversary, the language motivation field reaching 60 years. At this occasion, we pause to reflect on the contribution of language motivation research to language teaching practice. We argue that this contribution has been negligible and put forward two main reasons. The first is related to an identity crisis in the language motivation field, falling at the intersection of applied linguistics, education, and psychology; the second is the marginalization of the role of context. To address these issues, we first present insights from two perspectives—sociocultural theory and complex dynamic systems theory—and then propose three solutions to incorporate these insights: (1) moving from the abstract notion of “motivation” to the more tangible construct of “engagement”, (2) encouraging rigorous transdisciplinary research, and (3) taking advantage of the potential of artificial intelligence to translate research findings into practice.

Keywords

motivation, language teaching, sociocultural theory, complex dynamic systems theory, *perezhivanie*, artificial intelligence, student engagement

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As the language motivation (LM) field has reached 60 years since it was founded (Al-Hoorie, 2017; Al-Hoorie & MacIntyre, 2020; Gardner & Lambert, 1959), it seems apt to stop and reflect on its achievements to date. Notably, this is not the first time in the history of the field that LM researchers have stopped to question the direction the field was taking, as a similar self-reflection took place in the early 1990s. Problematizing the then-dominant framework pioneered by Robert Gardner (1979, 1985, 2010), Crookes and Schmidt (1991) called for a “reopening” of the research agenda in order to accommodate more classroom-friendly LM research. That challenge to the status quo culminated in what came to be known as the *Modern Language Journal* debate on LM (Dörnyei, 1994a, 1994b; Gardner & Tremblay, 1994a, 1994b; Oxford, 1994; Oxford & Shearin, 1994), where leading scholars exchanged views on what aspects of motivation had been unduly overlooked. A general take-home lesson from this debate was that the field had overemphasized the social macro-perspective of motivation—represented in integrative motivation in particular—at the expense of its cognitive underpinnings and contextual factors. The field subsequently entered a new phase described by some as the cognitive-situated period (Dörnyei & Ryan, 2015) and the educational period (Al-Hoorie, 2017).

In this article, we reflect on the extent to which LM research has achieved the goal of informing language teaching practice. We argue that its success has been limited. We attribute this to an “identity crisis” afflicting LM researchers. LM falls at the intersection of applied linguistics, education, and psychology (among others), which requires proportionately wide-ranging graduate training and later rigorous transdisciplinary investigations. We then propose solutions stemming from Vygotskian socio-cultural theory and complexity theory in the hope of addressing this gap.

Has Research Informed Teaching Practice?

Today, there is skepticism about whether applied linguistics research has indeed transformed into the classroom-friendly enterprise that can enlighten teaching. As Larsen-Freeman (2015, p. 271) observed, “research has been less consequential in affecting practice widely” than one would have expected it to be. Countless MA, PhD, and peer-reviewed articles are produced every year, but are then left unread by classroom practitioners. This led Maley (2016) to wonder whether the oft-repeated phrase “further research is needed” is a mantra taken too far, as academic research has consistently failed to address the daily concerns of language teaching professionals. The sobering reality, as reflected in one fairly large-scale multi-national study (Borg, 2009), is that hardly more than 15% of the surveyed teachers reported reading the literature regularly, citing, among other things, the irrelevance of academic research findings to their local classroom contexts. Another more recent survey by Marsden and Kasprowitz (2017) reached comparable conclusions, a result the researchers described as “bleak” (p. 613).¹

While it is hard to dispute that research has, occasionally, come up with some non-intuitive ideas that have the potential to enhance classroom practice (see Hattie, 2009; Paran, 2017), few would disagree that “[r]esearch is *not* the primary basis of [language teaching] knowledge for the practitioner” (Ur, 2012, emphasis added). The sheer

volume of academic research published almost daily dwarfs the modest contribution this research has made to date, raising the question of whether research into language teaching should now be considered overrated. Not surprisingly, some have gone as far as to describe academic researchers as “mere extras in the language-teaching operation” (Medgyes, 2017, p. 494) and their role as “parasitical” (p. 496), and thus mostly failing to meet the ethical obligations that researchers ought to uphold when working in collaboration with their teacher partners (De Costa, 2015; De Costa et al., 2020). Some progressive language teaching approaches, like Dogme (Meddings & Thornbury, 2009), hardly make an attempt to forge connections with language learning research.

In the LM field more specifically, the situation is not very different. Considering that attitudes and motivation are perceived as an easy target, numerous novice researchers have jumped on its bandwagon (Ushioda, 2016). Perhaps motivated by journal editorial policies, modern-day LM papers usually end with some variation of “pedagogical applications” that more often than not do not logically and unambiguously follow from the results of the research—a classic case of misapplied linguistics (see Han, 2007; Swan, 2009). Book-length treatments on motivational “strategies” and “techniques” have also started to proliferate. These strategies are sold to teachers as recipes for application rather than being proposed to fellow researchers as hypotheses for investigation, even though they have mostly emerged from superficial observational and self-report sources of data. Such *pseudo-applications*, as we would refer to them, can do more harm than good to the credibility of the LM field (Al-Hoorie, 2018).

While we do not discount the expertise practitioners build over years of experience, having all spent decades in language classrooms ourselves, as educational researchers we must also concede that the evidentiary basis for what passes as effective, high-leverage language pedagogy must come from an empirical understanding of how people learn languages in instructed settings (Han & Nassaji, 2019; What Works Clearinghouse, n.d.). In order to help the reader evaluate the contribution the LM field has made to language teaching, we suggest a simple three-pronged approach. We invite readers to consider how many motivational applications satisfy the following criteria:

1. *Originality*: The application is not a common-sensical practice that the average teacher (Sato & Loewen, 2018), or even an uninformed observer (Maley, 2016), can figure out on their own and without the labor-intensive academic research conducted by teams of PhD holders.
2. *Source*: The application originates from or is significantly reshaped by the LM field, rather than being borrowed and repackaged from existing educational and psychological theories. This is to establish whether our field is a mere subsidiary consumer of these “mainstream” fields, and whether our field actually contributes to their dialog in meaningful ways.
3. *Methodology*: The effectiveness of this application to classroom practice has been demonstrated through independently replicated (preferably pre-registered) experimental interventions whose effect sizes are considered substantial enough to warrant educational implementation—or through a sufficiently evidence-based transferability argument in other research paradigms.

We suspect that the list of LM applications satisfying these criteria would be very short. Indeed, it is no secret that “[g]ood teachers know far more about motivating students than the sum of knowledge that can be gained from research” (Henry et al., 2019, p. 15). A counterargument might argue that the LM field is still in its infancy, aged just 60 years as noted earlier—or even 30 years since the shift to classroom-oriented research. However, first, motivation is one of the most popular research topics, showing an exponential increase in quantity year after year (Boo et al., 2015). At the same time, the quality of much of this work, we would argue, has not seen a corresponding upward trend (e.g., Al-Hoorie & Al Shlowiy, 2020; Hiver & Al-Hoorie, 2020a). Second, as mentioned above, many language researchers *do* list such pseudo-applications routinely, and superficially, in their publications. It would indeed be a solemn cause for self-reflection if an avalanche of research over several decades has not produced some motivational applications satisfying the three criteria we proposed above.

We acknowledge that, by nature, progress in the human and social sciences is slower than in the hard sciences (cf. Meehl, 1978). Consider for example advances in the medical and technological arenas. Dewaele (2019) additionally argued that the advent of complexity theory with its rethinking of generalizability might have slowed down contribution to practice (though see Hiver & Al-Hoorie, 2020b, for an in depth treatment of this topic). Along these lines, one veteran motivation scholar openly declared:

We are not doing science, we are doing the difficult stuff. Science was developed for the physical world. We deal with the symbolic world of abstract conceptualizations such as motivation, intention, goals, rewards, wishes, imagined futures. So we don't do science; we only explore phenomena of interest. (John Schumann, cited in Dörnyei & Ryan, 2015, p. 198)

Obviously, exploring phenomena of interest is one thing, and claiming to inform teachers about how to do their job more effectively is quite another. Giving advice to teachers has become *de rigueur* of late, which strikes us as antithetical to engaging in necessary critical reflection on theory and the limits of available empirical evidence.

In this paper, we probe why LM research has been hampered from making genuine and direct contributions to teaching. We first argue that this status quo has resulted to a large extent from an identity crisis in our field. We then make the case that a second major factor is the marginalization of the role of context. We present insights from Vygotsky's sociocultural theory and complexity theory to address the vital role of context, and then contemplate ways to translate these insights into practice.

Identity Crisis: Three Factors

Graduate (Mis)Training

Although scholars with a social agenda may see it as having a sociological dimension (Clément & Norton, submitted; Norton, 2020), motivation is generally seen by LM researchers as a psychological construct. Indeed, “the psychological dimension will not go away but is likely to take up an increasingly central position” (Dörnyei, 2019a, p. 33).

Despite this heavy flavor of psychology in motivation, LM researchers typically graduate from (applied) linguistics and language departments, where they study a mixture of courses on theoretical linguistics and on practical aspects of language teaching—the latter having “virtually no linguistic foundation” (Dörnyei, 2019a, p. 28). Some of these departments may additionally offer one or two courses on the psychology of language, but this is inadequate to equip the new generation of researchers with the tools, skills, and methods necessary for systematic research on psychological phenomena. LM researchers therefore spend years training as applied linguists, but end up doing psychology research. Many would probably agree that these years of training would be relatively squandered if the linguistics knowledge gained is not actually utilized in and applied to subsequent LM research. (Indeed, one does not need a degree in linguistics to read and understand the latest and most cutting-edge LM literature.) Thus, LM researchers, in effect, graduate from the “wrong” departments.

In recognition of the psychological nature of LM, a growing number of LM researchers identify as language learning *psychologists*. They now have their own International Association for the Psychology of Language Learning, which oversees a biannual Psychology of Language Learning (PLL) conference and a dedicated psychology journal (*Journal for the Psychology of Language Learning*). There is also a growing interest in positive psychology in LM (Al-Hoorie, 2017; Dewaele et al., 2019; Pitts, 2019); it would be rather unusual to do positive psychology research but then claim not be a psychologist. At the same time, our engagement with the latest in psychological research is still not at an optimal level. And, as psychologists, we “do not have the option of ignoring the new psychological approaches” (Dörnyei, 2019a, p. 33). As is often seen in reference lists, our field seems to make little attempt to keep up with the latest findings and methods in either psychology or psycholinguistics (Oga-Baldwin et al., 2019). Beyond citing a few classics, mostly published in the 1980s and 1990s, there is generally little indication of an interest to enter into a conversation with mainstream psychologists—let alone publish in their flagship journals.² As a sign of this identity crisis, language and linguistics departments have produced a generation of motivation researchers with “minimal formal training in the other core discipline, psychology” (Mercer & Ryan, 2016, p. 3).³

The Fundamental Difference Curse

Another factor that contributes to this identity crisis is the longstanding belief that language learning is substantively different from learning other school subjects. Since language has a social element related to an out-group, learning a foreign language is presumed to have deep identity and cultural implications (e.g., Dörnyei, 2003, 2009; Gardner, 1985, 2010; Williams, 1994), somehow making it “a special case” (Ushioda, 2012). In fact, this idea “has been accepted by researchers all over the world, regardless of the actual learning situation they were working in” (Dörnyei, 2005, p. 67) and is considered a “breakthrough” that has “rightfully influenced the motivation research [for] decades” (Dörnyei, 1994b, p. 519). This view has been described as the *fundamental difference hypothesis* (Al-Hoorie & Hiver, 2020).

While there is hardly any direct empirical evidence to support this hypothesis, there is actually some evidence to the contrary. Some research shows that motivational processes of language learning and of learning other school subjects are comparable (see Al-Hoorie & Hiver, 2020; Lalonde & Gardner, 1993; MacIntyre et al., 2012). Furthermore, researchers interested in other school subjects have expressed similar sentiments to those found in the LM field. For example, there is a decade of evidence that identity, emotions, social and political factors, both inside and outside of school, can have an impact on learning mathematics (e.g., Boaler, 2002; Darragh, 2016; Gutiérrez, 2013; Nasir & de Royston, 2013). Indeed, if LM researchers believe that their area of study is somehow unique, then it would be logical to also believe that psychology and education are the wrong places to look for insight. This fundamental difference myth has apparently contributed to an egocentric isolation of our field, further impeding its development. Crucially, this isolation stems not only from a rigid paradigmatic perspective. As noted by De Costa et al. (2017; see also De Costa et al., 2019), theory, paradigm, and methodology are inextricably linked. Isolation leads to methodological limitations, a point to which we turn next.

The Questionnaire Curse

LM research has its roots in individual differences (see, for example, Dörnyei, 2006). Individual differences research (e.g., on personality and intelligence) typically involves asking participants a set of questions and then using their responses to predict important outcomes. When LM researchers turned their attention to the classroom, the methodology of this paradigm—represented in questionnaire-heavy research designs—has persisted. Regrettably, the most popular outcome variable in contemporary LM literature is self-reported intended effort. This convenient outcome variable is sometimes euphemistically called *motivated behavior* and at other times simply the *criterion measure* “sometimes with capital C and M” (Al-Hoorie, 2018, p. 740). Combined with the accessibility of user-friendly software crunching such self-report data at will, motivation research has become obsessed with designs that “relate one measure based on verbal report to another measure based on verbal report” (Gardner, 2010, p. 73). The most popular research design has become administering self-report questionnaire scales to measure both independent and dependent variables, and then presenting tables of correlations showing significance asterisks (Al-Hoorie, 2018). This widespread methodological practice is why Ushioda (2016), based on Meara’s (2009) observation, describes a large proportion of LM research as “boring and predictable” and “rather dull” (p. 565).

As a natural consequence of this flood of boring motivational research (pardon the oxymoron), a growing number of major applied linguistics journals have implemented blanket desk-reject policies to submissions of such exclusively observational research. Just as Gardner and Tremblay (1994a) cautioned from the very beginning during the *Modern Language Journal* debate, claims based on observational findings are problematic and can sometimes be “of no value” (p. 366). Instead, we need more empirical evidence that is interventional in nature (although we, of course, do acknowledge the

value of sound descriptive research explaining phenomena that would have otherwise gone unnoticed). Mere correlations, even if camouflaged with sophisticated procedures like structural equation modeling, remain observational and usually cannot shed direct light on whether a motivational or pedagogical application is effective (Hiver & Al-Hoorie, 2020a, 2020b). Unlike other language areas where the number of instructional interventions can reach double the amount of observation research (Plonsky, 2013), LM interventions remain a rare commodity.

The Role of Context

To be clear, we are not advocating an anti-research position. We do acknowledge the contribution of academic research, especially to “macro,” policy-level aspects. In general education, for example, there have been important insights about issues such as lesson duration, spaced repetition over days and weeks, and flipped learning. In language research more specifically, we have obtained rich insights from areas such as language testing, materials development, and curriculum design. However, when it comes to actual classroom teaching, teachers are required to make constant, split-second decisions to adapt to changing and evolving contexts (Hiver et al., 2019). Asking teachers to learn how to teach from research findings is akin to asking an individual to learn how to drive or swim through reading books sans actual practice. Books might help in some respects (e.g., explaining rules, giving general tips), but in the end drivers and swimmers have to refine their skills through sustained practice and by trial and error due to the complex and unpredictable nature of context. This is precisely why understanding the role of context is so essential. In the following sections, we present two perspectives on context and then try to synthesize insights from them into classroom practice.

Vygotskian Sociocultural Theory

Veresov and Mok (2018) emphasize that in any developing system, it is essential to approach the theme of investigation as a unit, not as separate elements. A dialectical approach using superordinate concepts can provide clues to understanding the complexity of motivation in each language learner. One such superordinate concept is *perezhivanie*, which refers to “emotional experience” or “lived experience” (Veresov & Mok, 2018, p. 89; see also Vygotsky, 1994). According to *perezhivanie*, an individual learner and their environment are two crucial considerations. Individual learners with different previous learning histories, backgrounds, and genetic makeup will perceive and place value on the environment differently. In this sense, *perezhivanie* can be a useful epistemological tool to illuminate the differential effects of the environment among language learners. Lantolf and Swain (2020) argue that this conceptualization helps explain why the effect of the environment on individual development is not deterministic, “a charge that was, and continues to be, leveled by those who are uncomfortable with a theory claiming that the source of psychological development resides in the social environment” (p. 84). In other words, though we may find very general trends, every learner

creates still a unique *perezhivanie*. Veresov and Mok (2018) therefore argue: “it is only through examination of *perezhivanie* that we can come to understand the specific personal and situational characteristics that determined these aspects of the children’s social situations of development at a particular moment” (p. 92).

The proposal to prioritize the subjective, differential impact of the environment on the language learner can also be found in the concept of affordances (van Lier, 2004). Developed initially by Gibson (1979), an ecological understanding of affordances can be distinguished from the environment. While the environment may be full of potential, it becomes meaningful to the learner only if they realize this potential. Affordances are therefore “what is available to the person to do something with” (van Lier, 2004, p. 90). To synthesize arguments from Vygotsky (1994) and van Lier (2004), when a language learner perceives the importance of the environment at the personal level, the environment is transformed into an affordance that enables a learner to experience *perezhivanie*. The experience of *perezhivanie*, in turn, significantly affects the learner’s more discrete motivational characteristics. For example, reconstruction of self-efficacy creates the potential for the learner to further perceive the environment differently when a new dialectical motivational cycle commences.

Similarly, language learning motivation, as it is incubated, maintained, and possibly terminated in a subjectively perceived environment, requires long-term investigation to identify its ontogenetic influence on the learner’s language development over their lifespan. Ontogenesis is distinguished from microgenesis, or “the momentary instances of concrete, practical activity in which subjects engage with the world around them” (Cross, 2010, p. 439). In contrast, ontogenetic investigation refers to the accumulated accounts of personal, microgenetic “aha” moments, which require substantial longitudinal analysis of individual language learners as unique agentic beings. A “personal” timeline encapsulating the past, present, and future is essential to account for a learner’s motivation. This point is affirmed by some language identity researchers such as Norton (2013), who views identity as “the way a person understands his or her relationship to the world, how that relationship is constructed across time and space, and how the person understands possibilities for the future” (p. 4). Perceived experiences of success or failure in language learning are connected to the learner’s current level of motivation, which, in turn, creates a fertile ground for the learner’s future motivational outlook. In this regard, Ushioda’s (2009) person-in-context relational view may need to expand to and encompass a *history-in-person ontogenetic view* (Donato & Davin, 2018). Donato and Davin (2018) state that ontogenetic development is

the history of a single human being and is situated in relation to one’s sociocultural and historical circumstances and the tools that have been derived from one’s involvement in situated social practices that are currently used to mediate and regulate mental functioning. (p. 458)

Complexity Theory

One of the main ways complex dynamic systems theory can help language researchers to rethink the essence of motivation is to conceptualize LM as a complex system

situated in context. Complex systems consist of a number of components embedded in context, interacting with each other interdependently, and changing over time in system-wide patterns of behavior (Hiver & Al-Hoorie, 2016). This relational unit of analysis has encouraged scholars to think about how parts of the whole relate to each other and, in contrast to much of the existing work, to adopt a view of motivation as more organic and constrained more by contextual affordances (Nolen et al., 2015). Once researchers are able to view motivation as relational and context-dependent, a companion principle to this shift in thinking is that context shapes complex system behavior and its outcomes (Ushioda, 2009). This notion of interdependence within a language learning context and the phenomena under investigation is not new in the study of language development and use (Ushioda, 2016), but it has thus far been peripheral to mainstream LM. It is only recently that the notion of LM as a complex system situated in context has come to be discussed more explicitly in LM research (e.g., Hiver & Papi, 2019), and we anticipate that a more explicit consideration of this notion will yield future enhancements in to research designs.

The openness of complex systems to the environment gives rise to context-dependent behaviors, and this means that LM development and outcomes cannot be fully understood by breaking them into discrete parts (Hiver & Larsen-Freeman, 2020). This person–context interdependence (see also Ushioda, 2009) encourages a conceptual shift to view context as an intrinsic, core part of all motivated thought and action. The main research implication of this viewpoint is that LM is always situated and contextually constrained (e.g., Joe et al., 2017; Papi & Hiver, 2020). This assumption is also grounded in the idea that adaptation and development are not based on pre-existing or hard-wired motivational mechanisms separate from the immediate context of which a system is part (Larsen-Freeman, 2017). Instead, in particular contexts, soft-assembled mechanisms that involve a particular adaptation of the system in its environment are viewed as a major mechanism for motivational change. Such soft-assembled aspects of motivation are only realized within the immediate context of a situation or task (Larsen-Freeman & Cameron, 2008), and involve only the tools and structures that are currently available and necessary to the language learner. Simply put, complex systems in LM are embedded within an environment and are an integral constitutive part of that context. For this reason, contextual factors will now need to be understood as key dimensions of the system itself (Overton & Lerner, 2014; Rauthmann et al., 2015). Thus, LM can no longer be conceived of exclusively as a conventional, modular independent variable. Consequently, the broad contribution of complex dynamic systems theory to future advances in LM is its potential ability to “explain the dynamic development of real people in actual contexts” (Dörnyei, 2017, p. 87).

Translating These Insights into Practice

The complex and multifaceted nature of LM described above makes us wonder whether it is possible for our understanding to even approximate the abstract notion of motivation, let alone provide teachers with workable pedagogical recommendations. A more realistic endeavor may be to shift attention to the more tangible and actionable construct

of engagement (Hiver et al., 2021). Studying learner engagement allows us to take into account motivation but in specific tasks, in certain environments, and under certain conditions. As Dörnyei (2019b) explains, engagement offers “a natural way of mapping the most important facets of the learning experience, which in turn allows us to capture the key aspects in measurable terms” (p. 25). In the LM field, such contextual-level analysis has been relegated to the role of an elusive shadow. Dörnyei (2019b) posits two possible reasons for this situation: the historical roots of context and its under-theorized nature, and the over-emphasis on the “self” in recent literature (Al-Hoorie, 2018).

To better conceptualize the dynamic, diachronic transformational nature of the language learning experience in context, we also need to cast a wider net that recognizes and accommodates various crucial environmental factors (i.e., school contexts, syllabus, the teaching materials, learning tasks, peers, teachers). It is hard to fathom how this task can be accomplished competently without transdisciplinary research (De Costa & Norton, 2017; Hiver et al., in press; The Douglas Fir Group, 2016). As mentioned above, many LM researchers seem to “self-train” in psychology after completing formal education as (applied) linguists. This leads to a situation akin to eclecticism, and even though it “has assumed a meaning of almost the highest praise. . . eclectic positions have never yet led to success” (Leont’ev, 1978, p. 46). Self-training in other disciplines is not the most efficient approach to transdisciplinary research. Transdisciplinary research is less about individual researchers gaining (shallow) expertise in multiple disciplines, and more about a group of researchers with (deep) expertise in different disciplines teaming up to address a phenomenon of common interest (Hiver et al., in press). From this perspective, LM becomes the arena where researchers meet and collaborate *after* they obtain training and expertise in their respective fields (e.g., linguistics, psychology, sociology, economics, neuroscience, genetics, etc.). When linguists, trained formally as linguists, bring in their linguistics knowledge and team up with psychologists, they would most likely produce more quality research than would linguists-turned-psychologists.

The fruit of such transdisciplinary collaboration, while certainly illuminating and informative, will eventually reach the same roadblock when it comes to translating LM research findings to actual classroom practices. Research findings are invariably generic and need to be localized and adapted to a large extent—in turn making research contribution secondary and peripheral. At the same time, the best teaching is personalized and adapted to accommodate the individual learner’s motivation, needs, and history. The teacher, in the traditional sense, would find it overwhelming to process all this data for each individual learner considering the large number of students that teachers have to deal with, per class, usually for just one semester before a new batch of students comes in.

One way out of this vicious cycle draws from artificial intelligence (Dodigovic, 2005). Artificial intelligence has the potential to efficiently automate the teaching process, taking into account the individual learner’s history, current skill, and anticipated challenges, more than a human teacher ever could. For example, researchers have started exploring the potential of chatbots in education (e.g., Pham et al., 2018; Yang & Evans, 2019). In theory, such chatbots can be programmed to provide an adaptive

learning experience (i.e., identifying the learner's current proficiency level and presenting appropriate tasks), to recognize student linguistic input despite their developmental and typing errors, to detect subtle improvement and deterioration in performance and adjust tasks accordingly, to apply evidence-based motivational (even game-like) principles, to account for each student's individual learning history ontogenically, to integrate data from physiological markers to determine real-time mood, fatigue, anxiety, and other emotions—which can even be unconscious to the learner themselves—and to do all that in a human-like fashion so that one cannot tell whether they are interacting with another human or a machine. This promises to create a personalized learning experience with the “virtual agent” being available for 24 hr a day on the learner's smart device. Considering that youngsters nowadays seem to enjoy spending time more on their phones than talking with other people, artificial intelligence would bring about a revolution in language learning.

The rise of artificial intelligence, machine learning, and virtual reality would lead to a drastic transformation in the traditional educational process. In fact, these developments will push the field to rethink the role of teachers and instructional designers when technology becomes—rather autonomously—able to provide language input, process student output, and provide meaningful feedback to enhance language learning. In a similar vein, it is anyone's guess what teacher preparation will look like in an era of artificial intelligence. As an illustration, it is not unimaginable that, as artificial intelligence takes a stronger hold and accumulates massive amounts of personal data on students, privacy laws might constrain teachers' access to students' private data. Will this once more reduce the role of the teacher to that of a “technician”? Whether we take the ominous outlook that artificial intelligence foreshadows the death of the language teacher, or more optimistically believe that a human touch will always remain indispensable, few would question the fact that teacher education will need to anticipate and adapt to a redefinition of the teacher's role.

Most existing LM literature would consequently be at risk of becoming obsolete after the artificial intelligence revolution. Indeed, the “T” in ELT and TESOL might come to stand for Technology at some point. If that is the case, it is likely that LM research will continue being disengaged from everyday classroom reality without a means like artificial intelligence to translate its findings to practice.

Conclusion

In this article, we have presented what some might consider a sharp self-critique of the body of LM research. We believe that it is important for a healthy discipline to engage in critical reflection over its achievements. The implicit assumption in the whole LM literature is that researchers have a doctor–patient relationship with language teachers, though in reality if the whole LM literature were to suddenly disappear, this would hardly make a dent in everyday teaching practice. The impact of research on practice has generally speaking been “negligible, even zero” (Morrison & van der Werf, 2016, p. 351) and “at best disappointing; silence is a powerful condemnation” (p. 352; see also Hiver & Dörnyei, 2017, on teacher immunity).

In LM, the calls to move away from the social element of motivation, then represented in integrative motivation, has not led to the classroom-friendly research scholars had hoped for. The cognitivization of motivation has not been accompanied by a comparable and corresponding emphasis on context. To date, probably the best advice to give to a novice teacher is not to bury themselves in recently-published LM research, but to simply rely on experience and trial and error, and perhaps a good mentor. But this state of affairs is not a promising sign for the maturity or the contribution of our field to language teaching practice.

We have also argued that although most LM researchers already acknowledge that motivation is fundamentally psychological, a large proportion of LM researchers do not explicitly identify as psychologists earlier in their career and education. This acknowledgment has to reflect on the formal training of researchers as well as in journal policies, conference guidelines, and grant requirements. We have also argued that there is an urgent need for more transdisciplinary research to further our understanding of language-inflected motivation. An important part of this transdisciplinary research for the future might include the role of artificial intelligence and how it can transform teaching as we know it today.

Finally, the call for transdisciplinary research is reminiscent of the tale Gardner and Tremblay (1994b) told during the *Modern Language Journal* debate about two friends whose disagreement about the glass being half-full or half-empty led to discussions about various interesting topics long into the evening “but they parted good friends knowing that they had both profited immensely from the conversation” (p. 524).

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Notes

1. In recent years, Marsden and colleagues have set up the OASIS database (<https://oasis-database.org/about>) which is a repository of summaries (one-page descriptions of research

- articles) in order to make research into language learning and teaching openly available and easily accessible to anyone who might be interested in applied linguistics research.
2. Publishing in psychology journals might additionally be challenging for LM researchers due to, among other things, methodological expectations. This point further underscores the need to reconsider graduate training for future LM researchers. We thank Jean-Marc Dewaele for this comment.
 3. As one reviewer suggested, researchers can self-train in psychology and other disciplines after completing their formal education. It would be disconcerting to rely on this strategy broadly in a field, rather than rethinking education in the first place. Moreover, one criticism of self-training is that it can be amateurish in nature.

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