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RESEARCH ARTICLE

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Progress in partial least squares structural equation modeling use in marketing research in the last decade

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Abstract

Partial least squares structural equation modeling (PLS-SEM) is an essential element of marketing researchers' methodological toolbox. During the last decade, the PLS-SEM field has undergone massive developments, raising the question of whether the method's users are following the most recent best practice guidelines. Extending prior research in the field, this paper presents the results of a new analysis of PLS-SEM use in marketing research, focusing on articles published between 2011 and 2020 in the top 30 marketing journals. While researchers were more aware of the when's and how's of PLS-SEM use during the period studied, we find that there continues to be some delay in the adoption of model evaluation's best practices. Based on our review results, we provide recommendations for future PLS-SEM use, offer guidelines for the method's application, and identify areas of further research interest.

KEYWORDS

marketing models, partial least squares, PLS-SEM, research methodology, SEM, structural equation modeling

1 | INTRODUCTION

For many years, estimating models with complex inter-relationships between observed concepts and their latent variables was equivalent to executing factor-based structural equation modeling (SEM). Recent research, however, demonstrates the rise of partial least squares (PLS) as a composite-based alternative (Jöreskog & Wold, 1982). PLS-SEM applications have grown exponentially in the past decade (Hair et al., 2022), especially in the social sciences (e.g., Ali et al., 2018; Ringle et al., 2020; Willaby et al., 2015), but also in other fields of scientific inquiry, such as agricultural science, engineering, environmental science, and medicine (e.g., Durdyev et al., 2018; Menni et al., 2018; Svensson et al., 2018). The availability of comprehensive software programs with an intuitive graphical user interface (Sarstedt & Cheah, 2019), application guideline articles (e.g., Chin, 1998; Hair et al., 2011; Henseler et al., 2009), and textbooks (e.g., Hair et al., 2022; Ramayah et al., 2018; Wong, 2019), all of which have made the method available for nontechnical use, have shaped the field significantly and contributed to PLS-SEM's dissemination.

An article by Hair et al. (2012) has had a lasting impact on the marketing and consumer behavior disciplines—as evidenced by its massive citation count. In this paper, the authors review more than 200 studies using PLS-SEM and published in top 30 ranked marketing journals between 1981 and 2010. Based on their evaluation of PLS-SEM applications according to a wide range of criteria pertaining to, for example, model characteristics and assessment practices, Hair et al. (2012, p. 428) identify "misapplications of the technique, even in top-tier marketing journals," noting that "researchers do not fully capitalize on the criteria

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available for model assessment and sometimes even misapply measures." The authors also derive comprehensive guidelines for algorithmic settings, measurement and structural model evaluation criteria, as well as complementary analyses, which have become an anchor for the method's follow-up extensions and applications.

It has been a decade since the publication of Hair et al.'s (2012) article. During these years, the PLS-SEM field has undergone extensive methodological developments (Hwang et al., 2020; Khan et al., 2019). Research has shaped the method's understanding (e.g., Rigdon, 2012), introduced new metrics (e.g., Liengaard et al., 2021), and clarified aspects related to model specification and data considerations (e.g., Rigdon, 2012; Sarstedt et al., 2016), all of which are relevant for PLS-SEM users. Some of these extensions emerged from controversies about PLS-SEM's general efficacy (Evermann & Rönkkö, 2021), such as guidelines for identifying and treating endogeneity (Hult et al., 2018), methods for estimating common factor models (Bentler & Huang, 2014; Dijkstra & Henseler, 2015; Kock, 2019), and novel means of assessing discriminant validity (Henseler et al., 2015). In addition, best practices, for example, with regard to measurement and structural model assessment (Hair et al., 2020b), have solidified in recent years as part of the PLS-SEM field's further maturation. But has PLS-SEM use truly been understood and accepted in the marketing research field? Have researchers taken the most recent best practice guidelines on board? Are there still cases of misapplications as previously pointed out by Hair et al. (2012).

This paper addresses these questions by presenting the results of a new review of PLS-SEM use in marketing research, focusing on articles published between 2011 and 2020 in the top 30 marketing journals. By applying the same coding scheme as Hair et al. (2012), our analysis facilitates drawing conclusions about developments in PLS-SEM use over time and identifying potential points of concern. In addition, our review covers recent developments in the field, such as improved metrics for assessing internal consistency reliability (Dijkstra & Henseler, 2015), discriminant validity (Henseler et al., 2015), and predictive power (Shmueli et al., 2016) to explore whether researchers follow the latest advances in the field. Based on our review results, we spell out recommendations for future PLS-SEM use, offer guidelines for the method's application, and identify areas of further research interest. Our overarching aim is to improve the rigor of the PLS-SEM method's application.

2 | REVIEW OF PLS-SEM RESEARCH PUBLISHED BETWEEN 2011 AND 2020

To ensure comparability with Hair et al.'s (2012) article, we reviewed PLS-SEM applications in the top 30 marketing journals according to Hult et al. (2009) journal rankings. Since Hair et al. (2012) examined the period from 1981 to 2010, we focused our review on the following 10 years (i.e., 2011–2020) to identify possible developments and provide an overview of PLS-SEM use in recent marketing studies. We also used the keywords "partial least squares" and "PLS" to conduct a full-text search in the *Clarivate Analytics' Web of Science*, *EBSCO Information Services*, and *Elsevier's Scopus* databases. We ensured that we would retrieve the complete set of relevant articles by using the same search terms to search the journals' websites. The search was completed on January 12, 2021.

We excluded all articles matching our keyword list, but not related to PLS-SEM (e.g., private labels, PLs). We also excluded articles applying PLS regression or only mentioning PLS-SEM, but not applying the method. Following these adjustments, we conducted a detailed review of all the articles published in journals with interdisciplinary content (*Journal of Business Research*, *Journal of International Business Studies*, *Journal of Product Innovation Management*, and *Management Science*) to identify those in marketing. Finally, we excluded all articles introducing methodological advancements of the method (e.g., Shmueli et al., 2016) or including simulation studies comparing PLS-SEM with other methods (e.g., Hair et al., 2017b).

The search produced a total of 239 articles applying PLS-SEM in the last 10 years (Table 1), which is a steep increase compared to the previous period's 204 articles (Hair et al., 2012). A breakdown of the number of articles by year (Figure 1) shows that PLS-SEM use surged substantially in 2019 and 2020, with 37 and 36 articles, respectively—only 2010 saw a higher number of PLS-SEM articles. These results clearly indicate an enduring upward trend in PLS-SEM use since the early 2000s.

The 239 articles were published in 20 journals, with *Journal of Business Research* (58 articles; 24.27%), *Journal of Marketing Management* (35 articles, 14.64%), and *Industrial Marketing Management* (31 articles, 12.97%) being the top three most used publishing outlets. Compared to the previous period, PLS-SEM was applied far more frequently in *Journal of Product Innovation Management*, *Journal of the Academy of Marketing Science*, and *International Marketing Review*. In contrast, *European Journal of Marketing* and *Journal of Marketing* published fewer articles using PLS-SEM. Table 2 documents these results with respect to all journals that published more than one PLS-SEM article between 2011 and 2020, also showing the previous period's corresponding frequencies.

Of the 239 articles, 93 (38.91%) report two or more alternative models or different datasets (e.g., collected in different years, countries, target groups, or resulting from a segmentation), yielding a total number of 486 analyzed PLS path models. In the following, we use the term "studies" to refer to the 239 journal articles and "models" to refer to the 486 PLS path models analyzed in these articles. Compared to the previous period, the average number of PLS path models analyzed per article increased from approximately 1.5 to 2 in more recent years, showing a shift to multistudy designs that have become the norm in marketing and consumer research (McShane & Böckenholt, 2017).

3 | REVIEW OF PLS-SEM RESEARCH: 2011–2020

We evaluated the 239 articles and the models included therein in terms of the following criteria, which Hair et al. (2012), follow-up reviews, and conceptual articles (e.g., Hair et al., 2020b, 2019b, 2019c) identified as

TABLE 1 PLS-SEM studies in the top 30 marketing journals between 2011 and 2020

Advances in Consumer Research[*]	Wauters, Brengman, and Janssens 2011
European Journal of Marketing	Akman, Plewa, and Conduit 2019
	Aspara and Tikkanen 2011
	Carlson, Gudergan, Gelhard, and Rahman 2019
	Chen, Peng, and Hung 2015
	Coelho and Henseler 2012
	Davari, Iyer, and Guzmán 2017
	Dessart, Aldás-Manzano, and Veloutsou 2019
	Françoise and Andrews 2015
	Gaston-Breton, and Duque 2015
	Huang and Tsai 2013
	Krishen, Leenders, Muthaly, Ziółkowska, and LaTour 2019
	Mo, Yu, and Ruyter 2020
	Nath 2020
	Olbrich and Schultz 2014
	Ormrod and Henneberg 2011
	Piehler, King, Burmann, and Xiong 2016
	Šerić 2017
	Singh and Söderlund 2020
	Vidal 2014
	Wijayaratne, Reid, Westberg, Worsley, and Mavondo 2018
	Willems, Brengman, and Kerrebroeck 2019
	Yu, Ruyter, Patterson, and Chen 2018
Industrial Marketing Management	Ali, Ali, Salam, Bhatti, Arain, and Burhan 2020
	Berghman, Matthyssens, and Vandenbempt 2012
	Camisón and Villar-López 2011
	Faroughian, Kalafatis, Ledden, Samouel, and Tsogas 2012
	Ferreras-Méndez, Newell, Fernández-Mesa, and Alegre 2015
	Genc, Dayan, and Genc 2019
	Gupta, Drave, Dwivedi, Baabdullah, and Ismagilova 2020
	Harmancioglu, Sääksjärvi, and Hultink 2020
	Hazen, Overstreet, Hall, Huscroft, and Hanna 2015
	Heirati, O'Cass, Schoefer, and Siahtiri 2016
	Hossain, Akter, Kattiyapornpong, and Dwivedi 2020
	Inigo, Ritala, and Albareda 2020
	Jain, Khalil, Johnston, and Cheng 2014
	Joachim, Spieth, and Heidenreich 2018
	Lopes de Sousa Jabbour, Vazquez-Brust, Chiappetta Jabbour, and Latan 2017
	Mahlamäki, Rintamäki, and Rajah 2019
	Mahlamäki, Storbacka, Pylkkönen, and Ojala 2020

(Continues)

	Nagati and Rebolledo 2013
	Nenonen, Storbacka, and Frethey-Bentham 2019
	Ng, Ding, and Yip 2013
	Niu, Deng, and Hao 2020
	Poucke, Matthyssens, Weele, and Bockhaven 2019
	Pulles, Schiele, Veldman, and Hüttinger 2016
	Ritter and Geersbro 2011
	Rollins, Bellenger, and Johnston 2012
	Shahzad, Ali, Takala, Helo, and Zaefarian 2018
	Sluyts, Matthyssens, Martens, and Streukens 2011
	Stekelorum, Laguir, and Elbaz 2020
	Teller, Alexander, and Floh 2016
	Vries, Schepers, Weele, and Valk 2014
	Yeniaras, Kaya, and Dayan 2020
International Journal of Research in Marketing	Miao and Evans 2012
International Marketing Review	Andéhn and L'Espoir Decosta 2016
	Freeman and Styles 2014
	Griffith, Lee, Yeo, and Calantone 2014
	Jean, Wang, Zhao, and Sinkovics 2016
	Kumar, Singh, Pereira, and Leonidou 2020
	Moon and Oh 2017
	Oliveira Duarte and Silva 2020
	Pinho and Thompson 2017
	Rahman, Uddin, and Lodorfos 2017
	Rippé, Weisfeld-Spolter, Yurova, and Sussan 2015
	Singh and Duque 2020
	Sinkovics, Sinkovics, and Jean 2013
Journal of Advertising	Coleman, Royne, and Pounders 2020
	José-Cabezudo and Camarero-Izquierdo 2012
Journal of Advertising Research	Archer-Brown, Kampani, Marder, Bal, and Kietzmann 2017
	Dennis and Gray 2013
	Miltgen, Cases, and Russell 2019
	Robinson and Kalafatis 2020
	Singh, Crisafulli, and La Quamina 2020
Journal of Business Research	Ahrholdt, Gudergan, and Ringle 2019
	Albert, Merunka, and Valette-Florence 2013
	Ali, Ali, Grigore, Molesworth, and Jin 2020
	Ballestar, Grau-Carles, and Sainz 2016
	Banik, Gao, and Rabbanee 2019
	Barhorst, Wilson, and Brooks 2020
	Blocker 2011
	Borges-Tiago, Tiago, and Cosme 2019

Caputo, Mazzoleni, Pellicelli, and Muller 2020

Cenamor, Parida, and Wincent 2019

Cervera-Taulet, Pérez-Cabañero, and Schlesinger 2019

Chang, Shen, and Liu 2016

Del Sánchez de Pablo González Campo, Peña García Pardo, and Hernández-Perlines 2014

Ferrell, Harrison, Ferrell, and Hair 2019

Flecha-Ortíz, Santos-Corrada, Dones-González, López-González, and Vega 2019

Galindo-Martín, Castaño-Martínez, and Méndez-Picazo 2019

Gelhard and Delft 2016

Gudergan, Devinney, and Ellis 2016

Hernández-Perlines 2016

Hsieh 2020

Iglesias, Markovic, and Rialp 2019

Japutra and Molinillo 2019

Japutra, Ekinci, and Simkin 2019

Kapferer and Valette-Florence 2019

Kühn, Lichters, and Krey 2020

Leischnig, Henneberg, and Thornton 2016

Leong, Hew, Ooi, and Chong 2020

Martins, Costa, Oliveira, Gonçalves, and Branco 2019

McColl-Kennedy, Hogan, Witell, and Snyder 2017

Méndez-Suárez and Monfort 2020

Merz, Zarantonello, and Grappi 2018

Mourad and Valette-Florence 2016

Navarro-García, Arenas-Gaitán, Javier Rondán-Cataluña, and Rey- Moreno 2016

Navarro-García, Sánchez-Franco, and Rey-Moreno 2016

Ohiomah, Andreev, Benyoucef, and Hood 2019

Oliveira Duarte and Pinho 2019

Padgett, Hopkins, and Williams 2020

Palos-Sanchez, Saura, and Martin-Velicia 2019

Peterson 2020

Picón, Castro, and Roldán 2014

Reguera-Alvarado, Blanco-Oliver, and Martín-Ruiz 2016

Rippé, Smith, and Dubinsky 2018

Roy, Balaji, Soutar, Lassar, and Roy 2018

Saleh Al-Omoush, Orero-Blat, and Ribeiro-Soriano 2020

Schubring, Lorscheid, Meyer, and Ringle 2016

Segarra-Moliner and Moliner-Tena 2016

Sener, Barut, Oztekin, Avcilar, and Yildirim 2019

Sharma and Jha 2017

(Continues)

	Skarmeas, Saridakis, and Leonidou 2018
	Suhartanto, Dean, Nansuri, and Triyuni 2018
	Tajvidi, Richard, Wang, and Hajli 2020
	Takata 2016
	Thakur and Hale 2013
	Tran, Lin, Baalbaki, and Guzmán 2020
	Valette-Florence, Guizani, and Merunka 2011
	Wu, Raab, Chang, and Krishen 2016
	Zhang, He, Zhou, and Gorp 2019
	Zollo, Filieri, Rialti, and Yoon 2020
Journal of Interactive Marketing	Buzeta, Pelsmacker, and Dens 2020
	Divakaran, Palmer, Søndergaard, and Matkovskyy 2017
Journal of International Business Studies	Lam, Ahearne, and Schillewaert 2012
	Lew, Sinkovics, Yamin, and Khan 2016
Journal of International Marketing	Johnston, Khalil, Jain, and Cheng 2012
Journal of Marketing	Köhler, Rohm, Ruyter, and Wetzels 2011
Journal of Marketing Management	Ashill and Jobber 2014
	Balaji and Roy 2017
	Barnes and Mattsson 2011
	Bennett 2011
	Bennett 2018
	Bennett and Kottasz 2011
	Brettel, Engelen, and Müller 2011
	Brill, Munoz, and Miller 2019
	Carlson, Rahman, Rosenberger, and Holzmüller 2016
	Carlson, Rosenberger, and Rahman 2015
	Chiang, Wei, Parker, and Davey 2017
	Dall'Olmo Riley, Pina, and Bravo 2015
	Falkenreck and Wagner 2011
	Fernandes and Castro 2020
	Finch, Hillenbrand, O'Reilly, and Varella 2015
	Hankinson 2012
	Helme-Guizon and Magnoni 2019
	Iriana, Buttle, and Ang 2013
	Jack and Powers 2013
	King, Grace, and Weaven 2013
	Ledden, Kalafatis, and Mathioudakis 2011
	Mouri, Bindroo, and Ganesh 2015
	Ngo and O'Cass 2012
	Papagiannidis, Pantano, See-To, and Bourlakis 2013
	Richard and Zhang 2012
	Ross and Grace 2012

	Roy, Balaji, and Nguyen 2020
	Roy, Singh, Hope, Nguyen, and Harrigan 2019
	Stocchi, Michaelidou, Pourazad, and Micevski 2018
	Tabeau, Gemser, Hultink, and Wijnberg 2017
	Tafesse and Wien 2018
	Taheri, Gori, O'Gorman, Hogg, and Farrington 2016
	Teller, Gittenberger, and Schnedlitz 2013
	Wu, Jayawardhena, and Hamilton 2014
	Wyllie, Carlson, and Rosenberger 2014
Journal of Product Innovation Management	Beuk, Malter, Spanjol, and Cocco 2014
	Borgh and Schepers 2014
	Brettel, Heinemann, Engelen, and Neubauer 2011
	Calantone and Rubera 2012
	Carbonell and Rodríguez-Escudero 2016
	Carbonell and Rodríguez-Escudero 2019
	Dubiel, Durmuşoğlu, and Gloeckner 2016
	Ernst, Kahle, Dubiel, Prabhu, and Subramaniam 2015
	Feurer, Schuhmacher, and Kuester 2019
	Hammedi, Riel, and Sasovova 2011
	Heidenreich and Handrich 2015
	Heidenreich, Spieth, and Petschnig 2017
	Jean, Sinkovics, and Hiebaum 2014
	Kock, Gemünden, Salomo, and Schultz 2011
	Kuester, Homburg, and Hess 2012
	Langley, Bijmolt, Ortt, and Pals 2012
	Lee and Tang 2018
	Mahr, Lievens, and Blazevic 2014
	Matsuno, Zhu, and Rice 2014
	Mauerhoefer, Strese, and Brettel 2017
	McNally, Akdeniz, and Calantone 2011
	McNally, Durmuşoğlu, and Calantone 2013
	Ngo and O'Cass 2012
	Nijssen, Hillebrand, Jong, and Kemp 2012
	Pitkänen, Parvinen, and Töytäri 2014
	Schuster and Holtbrügge 2014
	Siahtiri 2018
	Spanjol, Mühlmeier, and Tomczak 2012
	Spanjol, Qualls, and Rosa 2011
	Zobel 2017
Journal of Public Policy and Marketing	Hasan, Lowe, and Petrovici 2019
Journal of Retailing	Pelser, Ruyter, Wetzels, Grewal, Cox, and Beuningen 2015

(Continues)

Journal of Service Research	Boisvert 2012
	Mullins, Agnihotri, and Hall 2020
Journal of the Academy of Marketing Science	DeLeon and Chatterjee 2017
	Ernst, Hoyer, Krafft, and Krieger 2011
	Fombelle, Bone, and Lemon 2016
	Hansen, McDonald, and Mitchell 2013
	Heidenreich, Wittkowski, Handrich, and Falk 2015
	Heijden, Schepers, Nijssen, and Ordanini 2013
	Hillebrand, Nijholt, and Nijssen 2011
	Houston, Kupfer, Hennig-Thurau, and Spann 2018
	Hult, Morgeson, Morgan, Mithas, and Fornell 2017
	Leroi-Werelds, Streukens, Brady, and Swinnen 2014
	Martin, Johnson, and French 2011
	Miao and Evans 2013
	Nakata, Zhu, and Izberk-Bilgin 2011
	Ranjan and Read 2016
	Santos-Vijande, López-Sánchez, and Rudd 2016
	Steinhoff and Palmatier 2016
	Weerawardena, Mort, Salunke, Knight, and Liesch 2015
	Wilden and Gudergan 2015
	Wolter and Cronin 2016
Marketing Letters	Dugan, Rouziou, and Hochstein 2019
Psychology and Marketing	Barnes and Pressey 2012
	Borges-Tiago, Tiago, Silva, Guaita Martínez, and Botella-Carrubi 2020
	Devece, Llopis-Albert, and Palacios-Marqués 2017
	Evers, Gruner, Sneddon, and Lee 2018
	Fatima, Mascio, and Johns 2018
	Gong and Yi 2018
	Hernández-Perlines, Moreno-García, and Yáñez-Araque 2017
	Jain, Malhotra, and Guan 2012
	Revilla-Camacho, Vega-Vázquez, and Cossío-Silva 2017
	Sheng, Simpson, and Siguaw 2019
	Verhagen, Dolen, and Merikivi 2019
	Zhang and Zhang 2014

Note: California Management Review, Harvard Business Review, Journal of Consumer Psychology, Journal of Consumer Research, Journal of Marketing Research, Management Science, Marketing Science, Quantitative Marketing and Economics, and Sloan Management Review did not produce any relevant articles.

Journal of Business ceased publication of the journal at the end of 2006.

Abbreviation: PLS-SEM, partial least squares structural equation modeling.

*We excluded five studies by Caemmerer and Mogos Descotes (2011), Chen et al. (2011), Kidwell et al. (2012), Luo et al. (2015), and Rippé et al. (2019) published in *Advances in Consumer Research* as these were only published as extended abstracts.

relevant for PLS-SEM use: (1) reasons for using PLS-SEM, (2) data characteristics, (3) model characteristics, (4) measurement model evaluation, (5) structural model evaluation, (6) advanced modeling and analysis techniques, and (7) reporting. Addressing these seven critical issues, we provide an update of PLS-SEM applications in marketing and compare our findings with those of Hair et al. (2012). Where applicable, we discuss major shifts in PLS-SEM use between the two periods under consideration (i.e., 1981–2010 and 2011–2020).

3.1 | Reasons for using PLS-SEM

Our review shows that 196 (82.01%) of the 239 studies provide a rationale for using PLS-SEM instead of factor-based SEM or sum scores regression (Table 3). The five most frequently mentioned reasons are: small sample size (114 studies, 47.70%), nonnormal data (76 studies, 31.80%), theory development and exploratory research (73 studies, 30.54%), high model complexity (70 studies, 29.29%), predictive study focus (61 studies, 25.52%), and formative measures (56 studies, 23.43%).

The two main reasons mentioned in our study are similar to those reported by Hair et al. (2012) a decade ago. Given the increasing calls to move beyond data characteristics to motivate the use of PLS-SEM and, instead, emphasize the research objective (e.g., Hair et al., 2019b; 2019c; Sarstedt et al., 2021), this finding raises concerns. While PLS-SEM performs well in terms of statistical power (Sarstedt et al., 2016) and convergence compared to other methods when the sample size is small (Henseler et al., 2014; Reinartz et al., 2009), it is *undeniable* and *axiomatic* that small samples adversely affect *all* statistical techniques, including PLS-SEM (Benitez et al., 2020). In fact, more than two decades ago, Chin (1998, p. 305) warned researchers that "the stability of the estimates can be affected contingent on the sample size." Cassel et al. (1999) also noted that PLS-SEM analyses should draw on sufficiently large sample sizes to warrant small standard errors. Numerous others, such as Marcoulides and Saunders (2006), Hair et al. (2013), and

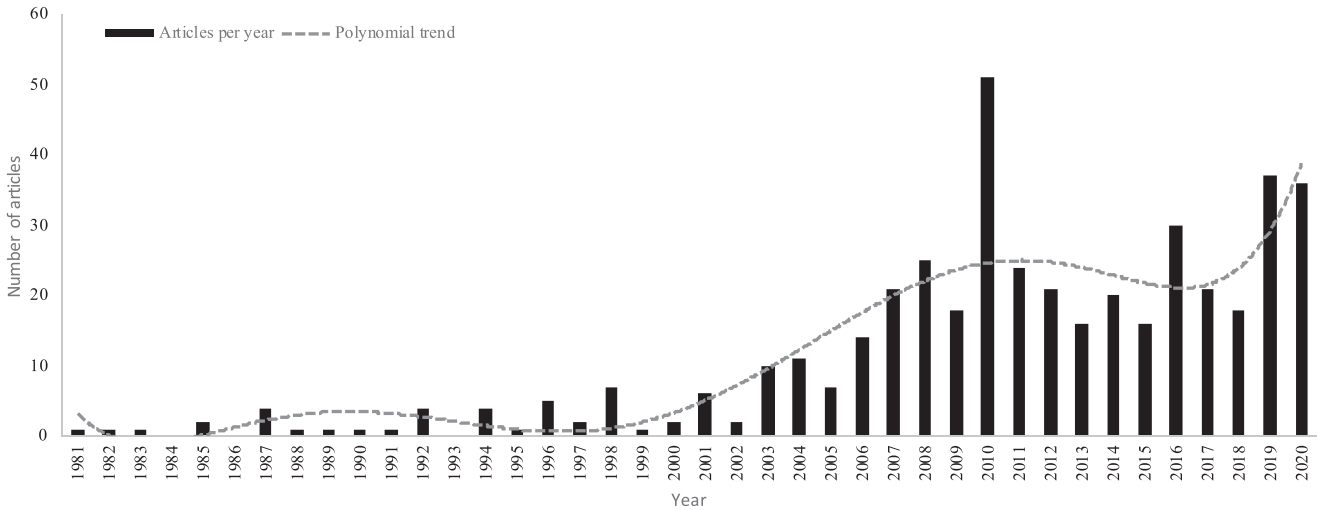

FIGURE 1 Articles per year

TABLE 2 Journals with more than one publication between 2011 and 2020 compared to 1981–2010

Journals	Publications 1981–2010 (*n* = 204)	Proportion (%)	2011–2020 (*n* = 239)	Proportion (%)
Journal of Business Research	15	7.35	58	24.27
Journal of Marketing Management	6	2.94	35	14.64
Industrial Marketing Management	23	11.27	31	12.97
Journal of Product Innovation Management	11	5.39	30	12.55
European Journal of Marketing	30	14.71	22	9.21
Journal of the Academy of Marketing Science	13	6.37	19	7.95
International Marketing Review	3	1.47	12	5.02
Psychology and Marketing	9	4.41	12	5.02
Journal of Advertising Research	4	1.96	5	2.09
Journal of Advertising	3	1.47	2	0.84
Journal of Interactive Marketing	2	0.98	2	0.84
Journal of International Business Studies	2	0.98	2	0.84
Journal of Service Research	7	3.43	2	0.84

Rigdon (2016), have clearly advised against relying on the logic of using PLS-SEM to derive results from small samples—unless the nature of the population can justify such a step (e.g., a small population size as commonly encountered in B2B research; Benitez et al., 2020; Hair et al., 2019c). In fact, critics such as Evermann and Rönkkö (2021), Rönkkö and Evermann (2013), and Rönkkö et al. (2016) have repeatedly used PLS-SEM's misapplication as a "straw-man" argument to criticize the technique itself, rather than the weak research designs (Petter & Hadavi, 2021; Petter, 2018). Similarly, simulation studies have shown that PLS-SEM does not offer substantial advantages over other SEM methods in terms of parameter accuracy when the data depart somewhat from normality (Goodhue et al., 2012; Reinartz et al., 2009). Reflecting this evidence, guideline articles have emphasized that it is not sufficient to motivate the choice of PLS-SEM over alternative methods primarily on the basis of nonnormality alone (e.g., Hair et al., 2019b, 2019c, 2020b).

Despite their strong emphasis on the data characteristics, our results also attest to marketing researchers' higher maturity in terms of motivating their method choice. For example, compared to the previous period, an increasing number of researchers emphasize model complexity and their study's predictive focus. Both are valid arguments, as PLS-SEM can handle highly complex models with many indicators, constructs, and model relations without identification concerns (Akter et al., 2017). When estimating these models, PLS-SEM follows a causal-predictive paradigm aimed at testing the predictive power of a model developed carefully on the basis of theory and logic (Liengaard et al., 2021). The underlying theoretical rationale, also referred to as explanation and prediction-oriented (EP) theory, "corresponds to commonly held views of theory in both the natural and social sciences" (Gregor, 2006, p. 626). Numerous standard models, such as the various customer satisfaction index (e.g., Eklöf & Westlund, 2002; Fornell et al., 1996) or technology acceptance models (e.g., Davis, 1989; Venkatesh et al., 2003), follow an EP-theoretic approach by aiming to explain the cause-effect mechanisms the model postulates, while also generating predictions that underline its practical usefulness (Sarstedt & Danks, 2021). PLS-SEM's ability to strike a balance between machine learning methods, which are fully predictive but atheoretical by nature (Hair & Sarstedt, 2021), and factor-based SEM, which is purely concerned with theory confirmation (Hair, et al., 2021b), makes PLS-SEM a particularly valuable method for applied research disciplines like marketing.

Researchers' increased focus on predictive purposes is no coincidence. Seminal articles by Evermann and Tate (2016) as well as Shmueli et al. (2016) on the method's predictive performance have opened a new chapter in PLS-SEM-based methodological research. For example, Sharma et al. (2021c) introduced prediction-oriented model comparison to the field by identifying metrics that excel at selecting the model with the highest predictive power and adequate fit from a set of competing

TABLE 3 Reasons for using PLS-SEM

	1981–2010		2011–2020	
	Number of studies (n = 204)	Proportion (%)	Number of studies (n = 239)	Proportion (%)
Nonnormal data	102	50.00	76	31.80
Small sample size	94	46.08	114	47.70
Formative measures	67	32.84	56	23.43
Explain variance in the endogenous constructs	57	27.94	–	–
Theory development and exploratory research	35	17.16	73	30.54
High model complexity	27	13.24	70	29.29
Categorical variables	26	12.75	5	2.09
Theory testing	25	12.25	11	4.60
Predictive study focus	–	–	61	25.52
PLS-SEM's popularity and standard use in the field	–	–	14	5.86
Moderation effects	–	–	12	5.02
Higher-order constructs	–	–	8	3.35
Simultaneous use of multi- and single-items constructs	–	–	6	2.51
Latent variable scores availability	–	–	5	2.09
Mediation effects	–	–	3	1.26
Other reasons (e.g., small number of indicators per construct, model comparison assessment, higher statistical power than factor-based SEM)	–	–	19	7.95

Abbreviation: PLS-SEM, partial least squares structural equation modeling.

models. Liengaard et al.'s (2021) cross-validated predictive ability test and its recent extension (Sharma et al., 2021a) enable researchers to test a model's predictive power relative to that of competing models and on a standalone basis. Other studies have extended prior research by comparing PLS-SEM's predictive power with that of Hwang and Takane's (2004) generalized structured component analysis, identifying situations that favor each method's use from a prediction point of view (Cho et al., 2021). Guideline articles that make these techniques accessible to a broader audience have accompanied these methodological extensions (Chin et al., 2020; Hair, 2021; Shmueli et al., 2019). Our review of the reasons given for the choice of PLS-SEM reflects some of these developments, but we expect a further shift toward emphasizing the method's causal-predictive focus in the future.

Finally, and unlike in the previous period, researchers also suggest additional reasons, such as PLS-SEM's popularity and standard use in the field (14 studies, 5.86%), the estimation of moderation effects (12 studies, 5.02%), mediation effects (3 studies, 1.26%) and higher-order constructs (8 studies, 3.35%). Indeed, recent research emphasizes the method's efficacy regarding estimating conditional process models combining mediating and moderating effects in a single analysis (Cheah et al., 2021; Sarstedt et al., 2020a), and clarifies the specification, estimating, and evaluation of higher-order constructs by means of PLS-SEM (Sarstedt et al., 2019). At the same time, fewer studies motivate their choice of PLS-SEM on the grounds of formative measurement—a finding that the studies' model characteristics, which we discuss later, also mirror.

3.2 | Data characteristics

Of the 486 models, 476 report the sample size (97.94%). The average sample size (5% trimmed mean = 279.19) and the median (199) of the models in our review are higher than those that Hair et al. (2012) reported for the 1981–2010 period (5% trimmed mean = 211.29; median = 159). Four studies in our review were conspicuous due to their very large sample sizes of between $n = 8876$ and $n = 26{,}576$. At the same time, 85 of the 476 models (17.86%) rely on sample sizes of less than 100 (i.e., the smallest sample size was $n = 29$). Only 17 of the 476 models (3.57%) fail to meet the ten times rule (Barclay et al., 1995), which is a very rough guideline for determining the minimum sample size required to achieve an adequate level of statistical power (for details and alternatives, see Hair et al., 2022, Chap. 1). Those models that do not meet the ten times rule draw on sample sizes that are, on average, only 10.98% below the recommended level (Table 4).

These results are encouraging compared to those in Hair et al. (2012), who reported a greater share of models estimated with less than 100 observations (24.44%), more models that failed the ten times rule (9.00%), and a higher relative deviation from the recommended sample size (45.18%). Marketing researchers—despite their reporting of PLS-SEM's efficacy regarding estimating models with small sample sizes as motivating their method choice—seem to be more concerned about sample size issues than previously. Critical accounts of the method (Goodhue

TABLE 4 Data characteristics

	1981–2010		2011–2020	
	Models ($n = 311$)	Proportion (%)	Models ($n = 486$)	Proportion (%)
Sample size: no. of models reporting	311	100%	476	97.94
5% trimmed mean	211.29	–	279.19	–
Median sample size	159	–	199	–
Sample size below 100	76 of 311	24.44	85 of 476	17.86
Ten times rule of thumb not met	28 of 311	9.00	17 of 476	3.57
Average deviation from the recommended sample size	45.18%		10.98%	
	Number of studies ($n = 204$)	Proportion (%)	Number of studies ($n = 239$)	Proportion (%)
Missing values: No. of studies reporting	–	–	41	17.15
Casewise deletion	–	–	39 of 41	95.12
Mean replacement	–	–	2 of 41	4.88
Outliers: No. of studies reporting	–	–	7	2.93
Nonnormal data: No. of studies reporting	19	9.31	24	10.04
Degree of nonnormality reported	–	–	5 of 24	20.83
Use of discrete variables: No. of studies reporting	57	27.94	24	10.04
Binary variables	43 of 57	75.44	15 of 24	62.50
Categorical variables	14 of 57	12.28	9 of 24	37.50

et al., 2012; Rönkkö & Evermann, 2013) and conceptual discussions (Hair et al., 2019c; Rigdon, 2016) have certainly contributed to this increased awareness. Nevertheless, empirical research almost never discusses the statistical power arising from a concrete analysis, despite the available tutorials on power analyses in a PLS-SEM context (Aguirre-Urreta & Rönkkö, 2015). For sample size determination, researchers can also draw on Kock and Hadaya's (2018) inverse square root method, which is relatively easy to apply and offers a more accurate picture of minimum sample size requirements than the ten times rule does.

Missing values are a primary concern in the data collection and processing phases. Nevertheless, only 41 studies (17.15%) acknowledge missing values in their data, while almost all of them (39 of 41 studies, 95.12%) used casewise deletion to treat them (Table 4). Grimm and Wagner (2020) have recently shown that PLS-SEM estimates are very stable when using casewise deletion on datasets with up to 9% missing values. However, when doing so researchers need to ensure the missing value patterns are not systematic, and that the final model estimation yields sufficient levels of statistical power. None of the relevant studies in our review offer such information. While casewise deletion seems to be the default setting for researchers using PLS-SEM, extant guidelines recommend using mean replacement when less than 5% values are missing per indicator or applying more complex procedures, such as maximum likelihood and multiple imputation (Hair et al., 2022). However, while the efficacy of more complex imputation procedures has been tested in a factor-based SEM context (Graham & Coffman, 2012; Lee & Shi, 2021), their impact on PLS-SEM's parameter accuracy and predictive performance remains a blind spot in methodological research.

Even though outliers influence the ordinary least squares regressions in PLS-SEM, only seven studies (2.93%) explicitly considered them. In one of these studies, the authors did not try to identify outliers but used an interval smoothing approach to mitigate the effect of corresponding observations. Of the remaining six studies that considered outliers, three did not mention the specific method used to detect outliers, whereas the remaining three studies included brief descriptions of the approaches: univariate and multivariate outlier detection, inconsistent or overly consistent response patterns, and univariate outlier detection using boxplots followed by detecting incongruence in response patterns. Most of these studies (5 out of 6) removed outliers from their dataset; the other study did not identify any outliers. While the presence of outliers might influence results substantially (Sarstedt & Mooi, 2019), our findings indicate that applied PLS-SEM research generally disregards outlier detection and treatment.

Nonnormal data is the second most frequently mentioned reason for using PLS-SEM. Nevertheless, only 24 studies (10.04%) acknowledge the nonnormal distribution of their data with few studies quantifying the degree of nonnormality by means of, for example, kurtosis and skewness (5 studies; Table 4). This is generally unproblematic, since PLS-SEM is a nonparametric method and, as such, robust in terms of processing nonnormal data (Hair et al., 2017b; Reinartz et al., 2009). While Hair et al. (2022, Chap. 2) note that highly nonnormal data may inflate standard errors derived from bootstrapping, Hair et al.'s (2017b) simulation study results suggest that such data do not impact Type I or II errors negatively.

Using discrete variables (i.e., binary and categorical variables) when measuring models is a final area of concern in PLS-SEM applications. Of the 239 studies, only 24 (10.04%) use binary or categorical variables as elements in measurement models (i.e., not as a grouping variable to split the dataset as part of a multigroup analysis), which is considerably less than what Hair et al. (2012) reported (27.94%). In line with Hair et al.'s (2012) recommendations, which raised concerns regarding their general applicability, especially when used as binary single-item indicators of endogenous constructs (e.g., to represent a choice situation), recent studies primarily use binary and categorical variables to perform multigroup analyses. However, research on the handling of discrete variables in PLS-SEM has progressed since 2012. For example, Hair et al. (2019a) document how to process data from discrete choice experiments where binary indicators representing a choice situation (e.g., buy/not buy) measure the constructs. Similarly, Cantaluppi and Boari (2016) extended the original PLS-SEM algorithm to accommodate ordinal variables when researchers cannot assume equidistance between the scale categories (see also Schuberth et al., 2018b). Given these developments, we expect the use of discrete variables in PLS path models, such as when estimating data from choice experiments, to gain traction in the future.

To summarize, while our results point to improvements in researchers' awareness of sample size issues in PLS-SEM use, they devote too little effort toward quantifying the degree of statistical power associated with the model estimation. Missing data are not routinely discussed, and their treatment in PLS-SEM is still an area of potential concern, which methodological research should address. Researchers should also be aware of recent extensions that facilitate the inclusion of discrete variables in PLS path models.

3.3 | Model characteristics

The number of constructs and indicator variables that define model complexity and the use of formative measurement models are the key reasons for PLS-SEM's attractiveness (Hair et al., 2022, Chap. 1). Table 5 offers an overview of these and other model characteristics, contrasting our results with those of Hair et al.'s (2012) review of the 1981–2010 period.

The average number of latent variables in the path models is 7.39, which is only slightly lower than in the previous period (7.94). Similarly, the average number of indicators per model is similar in both periods (1981–2010: 29.39; 2011–2020: 29.55), suggesting there has been no change in the model complexity over the years. The average number of structural model relationships, which increased only slightly from 10.56 to 11.90 from the initial period to the most recent, also supports this finding. In addition, we do not observe any substantial changes in the model types considered in the studies; that is, whether the models are focused versus unfocused by comprising a higher versus lower share of exogenous constructs than endogenous constructs. Specifically, the share of focused, unfocused, and balanced models is almost the same and very similar to those

TABLE 5 Model characteristics

Criterion	1981–2010 Results (n = 311)	Proportion (%)	2011–2020 Results (n = 486)	Proportion (%)
Number of latent variables				
Mean	7.94	–	7.39	–
Median	7.00		7.00	
Range	(2; 29)		(2; 24)	
Number of structural model relations				
Mean	10.56	–	11.90	–
Median	8.00		10.00	
Range	(1; 38)		(1; 70)	
Model type[a]				
Focused	109	35.05	161	33.13
Unfocused	85	27.33	149	30.66
Balanced	117	37.62	176	36.21
Measurement model[b]				
Only reflective	131	42.12	374	76.95
Only formative	20	6.43	7	1.44
Reflective and formative	123	39.55	105	21.60
Not specified	37	11.90	233	47.94
Number of indicators per reflective construct				
Mean	3.99	–	3.85	–
Median	3.50		3.00	
Range	(1; 27)		(2; 30)	
Number of indicators per formative construct				
Mean	4.62	–	4.28	–
Median	4.00		3.00	
Range	(1; 20)		(2; 38)	
Total number of indicators in models				
Mean	29.55	–	29.39	–
Median	24.00		24.00	
Range	(4; 131)		(3; 222)	
Number of models with single-item constructs	144	46.30	177	36.42

[a]Focused model: number of exogenous constructs at least twice as high as the number of endogenous constructs in the model; unfocused model: number of endogenous constructs at least twice as high as the number of exogenous constructs in the model; balanced model: all remaining models.

[b]This paper's authors classified models with missing information about the measurement mode ex post.

reported in Hair et al. (2012). We therefore repeat Hair et al.'s (2012) call to consider focused and balanced models, also because these model types should meet PLS-SEM's prediction goal better.

PLS path models, depending on their measurement model specifications, can be classified as either only reflective, only formative, or a combination of both.[1] We find the majority of the 486 models' constructs (374 models, 76.95%) are only reflectively measured, followed by models with both reflective and formative measures (105 models, 21.60%)—a much higher share of reflective measures compared to Hair et al. (2012). Only 7 of the 486 models' constructs (1.44%) are purely formatively specified. Somewhat surprisingly, a large percentage of the models (233 models; 47.94%) lack a description of the constructs' measurement modes, which is much higher than in Hair et al.'s (2012) review (11.90%). Classifying these models ex post by means of Jarvis et al. (2003) guidelines suggests the overwhelming majority (227 models; 97.42%) should be considered as only comprising reflectively specified constructs. While this result suggests that researchers consider reflective measures the default option, the consequences of measurement model misspecification should not be taken lightly. The impact of misspecifications on downstream estimates are generally not pronounced (Aguirre-Urreta et al., 2016), but the primary problem of such misspecifications arises from content validity concerns, since both measurement types rely on fundamentally different sets of indicators (Diamantopoulos & Siguaw, 2006). In line with Hair et al. (2012), researchers should ensure the measurement model specification is explicit to preempt criticism arising from potential misspecification claims. The confirmatory tetrad analysis for PLS-SEM (Gudergan et al., 2008; CTA-PLS; Hair et al., 2018, Chap. 3) provides a statistical test to confirm the choice of measurement model. However, only six studies (2.51%) used this approach to assess the adequacy of their measurement model specification.

The average number of indicators for reflectively measured constructs is 3.85 and 4.28 for formatively measured ones, which is similar to those in the previous period (Table 5). Contrary to their reflective counterparts, formative measurement models need to cover a much broader content bandwidth, as they are not conceived as interchangeable (i.e., highly correlated) measures of the same theoretical concept (Diamantopoulos & Winklhofer, 2001). Consequently, formative measurement models should have more indicators than reflective models do. Our review supports this notion, but the comparably small, albeit significant ($t = 2.037$, $p = 0.044$) difference in the average number of indicators is surprising, suggesting that future studies should devote more attention to content validity concerns in formative measurement.

We also find the share of single-item constructs decreased from 46.30% to 36.42% from one period to the next. On the one hand, this finding is encouraging, as it suggests researchers have become aware of single-item measures' limitations in terms of explanatory power. Specifically, Diamantopoulos et al. (2012) show that, due to the absence of multiple indicators of the same concept, the lack of measurement error correction deflates the model estimates, potentially triggering Type II errors in the model estimates (Sarstedt et al., 2016). On the other hand, single items are still too prevalent in PLS path models, which is particularly problematic when measuring endogenous target constructs. Researchers should generally refrain from using single items unless they measure observable characteristics, such as is commonly done in the form of control variables (e.g., income, sales, and number of employees).

To summarize, we observe a clear shift toward reflective measurement in PLS-SEM studies. The various controversies about formative measures' use (Howell et al., 2007; Rhemtulla et al., 2015; Wilcox et al., 2008) probably triggered this development, prompting researchers to "take the safe route" and rely on standard reflective measures. By following this mantra, researchers forego the opportunity to offer marketing practice differentiated recommendations. Contrary to their reflective counterparts, formative measures offer practitioners concrete guidance on how to "improve" certain target constructs—an important consideration in light of the growing concerns about marketing research's relevance for business practice (Homburg et al., 2015; Jaworski, 2011; Kohli & Haenlein, 2021; Kumar, 2017). At the same time, we see some improvement in the model specification practices, with researchers relying less on single-item measures. Nevertheless, the percentage of studies using single items is still high, providing an opportunity for improvement.

3.4 | Model evaluation

Since Hair et al.'s (2012) review, research has proposed various improvements in the model evaluation metrics for both measurement and structural models. In the following, we assess whether researchers consider best practices, while acknowledging that some of these improvements have only recently been introduced. We distinguish between reflective and formative measurement models, since their validation—as extensively documented in the literature—relies on totally different sets of criteria (Hair et al., 2021a, 2022; Ramayah et al., 2018; Wong, 2019).

3.4.1 | Reflective measurement models

A total of 479 of the 486 models (98.56%) included at least one reflectively measured construct (Table 5). Of these 479 models, 390 models (81.42%) reported the indicator loadings and 386 models (80.59%) reported at least one measure of the internal consistency reliability (Table 6). The majority of the models reported composite reliability ρ_C in conjunction with Cronbach's alpha (190 models, 39.67%). Composite reliability ρ_C was the only reliability metric reported in 132 models (27.56%), and Cronbach's alpha was the only reliability metric in 49 of the 479 models (10.23%). While these reporting practices reflect previous recommendations in the literature

[1]Note that this distinction refers to the measurement-theoretic perspective. To estimate formative measurement models, PLS-SEM uses composite indicators that define the construct in full. The method does not allow for estimating a formative construct's error term using causal indicators (Cho et al., 2021; Sarstedt et al., 2016).

TABLE 6 Measurement model evaluation

Panel A: Reflective measurement models					
		1981–2010		2011–2020	
	Empirical test criterion in PLS-SEM	Number of models reporting (n = 254)	Proportion reporting (%)	Number of models reporting (n = 479)	Proportion reporting (%)
Indicator reliability	Indicator loadings[a]	157	61.81	390	81.42
Internal consistency reliability	Only Cronbach's alpha	35	13.78	49	10.23
	Only ρ_c	73	28.74	132	27.56
	Only ρ_A	–	–	3	0.63
	Cronbach's alpha & ρ_c	69	27.17	190	39.67
	Cronbach's alpha & ρ_A	–	–	1	0.21
	ρ_c & ρ_A	–	–	2	0.42
	All three	–	–	9	1.88
Convergent validity	AVE	146	57.48	371	77.45
	Other	7	2.76	16	3.34
Discriminant validity[b]	Only Fornell-Larcker (FL) criterion	111	43.70	176	36.74
	Only cross-loadings	12	4.72	8	1.67
	Only HTMT	–	–	25	5.22
	FL criterion & cross-loadings	31	12.20	81	16.91
	FL criterion & HTMT	–	–	38	7.93
	Cross-loadings & HTMT	–	–	3	0.63
	All three	–	–	12	2.51
Panel B: Formative measurement models					
		1981–2010		2011–2020	
	Empirical test criterion in PLS-SEM	Number of models reporting (n = 143)	Proportion reporting (%)	Number of models reporting (n = 112)	Proportion reporting (%)
	Reflective criteria used to evaluate formative constructs	33	23.08	10	8.93
Collinearity	Only VIF/tolerance	17	11.89	43	38.39
	Only condition index	1	0.70	1	0.89
	Both	4	2.80	9	8.04
Convergent validity	Redundancy analysis	–	–	6	5.36
Indicator's relative contribution to the construct	Indicator weights	33	23.08	74	66.07
Significance of weights	Standard errors, significance levels, t values/p values	25	17.48	36	32.14
	Only confidence intervals	–	–	0	0.00
	Both	–	–	2	1.79

Abbreviation: PLS-SEM, partial least squares structural equation modeling.

[a]Single item constructs were excluded in 1981–2010 and 2011–2020.

[b]Proportion reporting (%) for 2011–2020 uses all 479 models as the base even though the HTMT criterion has only been proposed in 2015.

(Hair et al., 2017a), more recent guidelines call for using ρ_A (Dijkstra & Henseler, 2015) as an additional appropriate measure of internal consistency reliability. However, only 15 models rely on this criterion, either exclusively (3 models; 0.63%) or in conjunction with one or more other metrics (12 models, 2.51%). Compared to the previous period, a larger percentage of researchers consider both the indicator reliability and internal consistency reliability (Table 6). The internal consistency reliability assessment more frequently relies on Cronbach's alpha, whose use in the PLS-SEM context is criticized due to the metric assuming a common factor model (Rönkkö et al., 2021). When used in composite models, however, Cronbach's alpha underestimates internal consistency reliability, making it a conservative reliability measure. Consequently, when Cronbach's alpha does not raise concerns in a PLS-SEM analysis, the construct measurement can a fortiori be expected to exhibit sufficient levels of internal consistency reliability.

Of the 479 models, 371 (77.45%) assessed the convergent validity correctly by using the average variance extracted. The remaining models either established convergent validity incorrectly by using criteria such as cross-loadings and composite reliability (16 models, 3.34%) or did not comment on this aspect of model evaluation (92 models, 19.21%).

Discriminant validity is arguably the most important aspect of validity assessment in reflective measurement models (Farrell, 2010), because it ensures each construct is empirically unique and captures a phenomenon that other constructs in the PLS path model do not represent (Franke & Sarstedt, 2019). Against this background, it is surprising that only 343 of the 479 models (71.61%) assessed their discriminant validity. The most frequently used metric is the well-known Fornell-Larcker criterion (Fornell & Larcker, 1981), either as a standalone metric (176 models, 36.74%), or in conjunction with other criteria (131 models, 27.35%). The second most used criterion is an analysis of the indicator cross-loadings, which 104 of the 479 models (21.71%) report. Recent research has shown, however, that both criteria are largely unsuitable on conceptual and empirical grounds to assess discriminant validity. Henseler et al. (2015) propose the heterotrait-monotrait (HTMT) ratio of correlations as an alternative metric to assess discriminant validity, and a series of follow-up studies have confirmed its robustness (Franke & Sarstedt, 2019; Radomir & Moisescu, 2020; Voorhees et al., 2016). We find that 78 models of the 326 (23.93%) models that have been published since the publication of Henseler et al. (2015) draw on this criterion, either exclusively (25 models, 7.67%) or jointly with at least one other criterion (53 models, 16.26%). Of the 78 models applying the HTMT, 47 (60.26%) compare its values with a fixed cutoff value, two models (2.56%) rely on inference testing based on bootstrapping confidence intervals, and 23 models (29.49%) apply both approaches.

While our findings demonstrate some improvements in the reflective measurement model assessment compared to the previous period, concerns about the discriminant validity assessment remain. Given methodological innovations' diffusion latency in applied research, the strong reliance on the Fornell-Larcker criterion and cross-loadings seems understandable. However, future studies should only draw on the HTMT criterion and use bootstrapping to assess whether its values deviate significantly from a predetermined threshold. This threshold depends, however, on the conceptual similarity of the constructs under consideration. For example, a higher HTMT threshold, such as 0.9, can be assumed for conceptually similar constructs, whereas the analysis of conceptually distinct constructs should rely on a lower threshold, such as 0.85 (Franke & Sarstedt, 2019; Hair et al., 2022). Roemer et al. (2021) recently proposed the HTMT2, which relaxes the original criterion's assumption of equal population indicator loadings. Their simulation study shows that the HTMT2 produces a marginally smaller bias in the construct correlation estimates when the indicator loading patterns are very heterogeneous; that is, when some loadings are 0.55 or lower while others are close to 1. The small differences between the values of HTMT and HTMT2 are, however, extremely unlikely to translate into different conclusions when using the criterion for inference-based discriminant validity testing—as recommended in the extant literature (Franke & Sarstedt, 2019). For common loading patterns, where loadings vary between 0.6 and 0.8, the regular HTMT actually performs better than the HTMT2, unless one assumes extremely high construct correlations that would violate discriminant validity, regardless which criterion is being used. Hence, while the notion of an "improved criterion for assessing discriminant validity" appears appealing, the HTMT2 does not offer any improvement over the original metric.

3.4.2 | Formative measurement models

The assessment of formative measurement models draws on different criteria than those used in the context of reflective measurement due to the conceptual differences between the two approaches. Specifically, while reflective measurement treats the indicators as error-prone manifestations of the underlying construct, formative measurement assumes that indicators represent different aspects of a construct that jointly define its meaning (Diamantopoulos et al., 2008).

Overall, 112 of the 486 models (23.05%) include at least one formatively measured construct (Table 5). In 10 of these 112 models (8.93%), the researchers use reflective measurement model criteria to evaluate formative measures (Table 6). This practice is very problematic, however, because formatively measured constructs' indicators do not necessarily correlate highly, which renders standard convergent and internal consistency reliability metrics meaningless (Diamantopoulos & Winklhofer, 2001)—even though corresponding metrics may occasionally reach satisfactory values, since strong indicator correlations may also occur in formative measurement models (Nitzl & Chin, 2017). Instead, researchers need to (1) run a redundancy analysis to assess the formative construct's convergent validity, (2) ensure the formative measurement model is not subject to collinearity, and (3) evaluate the formative indicator weights. Table 6 shows the results of this aspect of our review.

In only 6 of the 112 models (5.36%) with formative measures, the authors reported the results of a redundancy analysis. This analysis establishes a formative measure's convergent validity by showing that it correlates highly with a reflective multi-item or single-item measure of the same concept (Cheah et al., 2018). Chin (1998) introduced the redundancy analysis in a PLS-SEM context more than 20 years ago, but extant guidelines only adopted this method more recently (Hair et al., 2022; Ramayah et al., 2018), which could explain its limited use.

Conversely, collinearity assessment is much more prevalent in formative measurement model evaluation, with about half of the models (53 models, 47.32%) assessed in this regard. The variance inflation factor is the primary criterion for collinearity assessment and applied in most studies (Table 6).

Formative measurement model assessment's main focus is interpreting the indicator weights, which represent each indicator's relative contribution to forming the construct's (Cenfetelli & Bassellier, 2009). Slightly less than two-thirds of the models in our review (74 models, 66.07%) report the formative indicator weights, and researchers also assess the indicators' statistical significance in 44 of the 74 models (59.46%). In six of these 44 models (13.63%), researchers only comment on the weights' significance, whereas they report inference statistics in 38 models (86.26%). A detailed analysis shows that 21 of the 44 models (47.73%) include nonsignificant weights. Rather than deleting corresponding indicators, extant guidelines suggest analyzing the formative indicators' loadings, which represent their absolute contribution to the construct (Hair et al., 2019b, 2022). Only five of the 21 models (23.81%) follow this recommendation.

Overall, our results regarding formative measurement model evaluation are encouraging, as they suggest researchers have become more concerned about applying criteria dedicated to this measurement mode. In comparison, in the previous period, less than one-third of all models analyzed the formative indicator weights and far fewer tested for potential collinearity issues. Nevertheless, in absolute terms our results offer room for improvement. There is virtually no redundancy analysis in PLS-SEM research, which is problematic, since it offers an empirical validation that the formative measure is similar to established measures of the same concept. In addition, many of the models still do not report collinearity statistics. This finding is surprising, since collinearity assessment has long been acknowledged as an integral element of formative measurement model assessment (Cenfetelli & Bassellier, 2009; Diamantopoulos & Winklhofer, 2001; Petter et al., 2007). Researchers should consider these aspects carefully before focusing on indicator weight assessment.

3.4.3 | Structural model

Once the measures' reliability and validity have been established, the next step is to assess the structural model's explanatory and predictive power, as well as the path coefficients' significance and relevance (Hair et al., 2020b). Table 7 documents our review's corresponding results.

In a first step, researchers need to ensure that potential collinearity between sets of predictor constructs in the model does not negatively impact the structural model estimates. Our review results show that marketing researchers generally omit this step, as only 100 of the 486 models (20.58%) mention inspecting collinearity in the structural model.

Not surprisingly, almost all the models (480 models, 98.77%) report path coefficients and their significance (473 models, 97.33%). Inference testing relies primarily on *t* tests with standard errors derived from bootstrapping (419 models, 86.12%). In keeping with recommendations in the more recent literature (Streukens & Leroi-Werelds, 2016), 54 models (11.11%) report bootstrapping confidence intervals either alone (11 models, 2.26%) or together with *t* values (43 models, 8.85%). Aguierre-Urreta and Rönkkö's (2018) simulation study suggests that researchers should primarily draw on percentile confidence intervals or, alternatively, on bias-corrected and accelerated confidence intervals. Both approaches perform very similarly, but the bias-corrected confidence intervals achieve slightly better results when the bootstrap distributions are highly skewed. Researchers seem to have internalized these recommendations, since 39 of 54 models (72.22%) rely on the percentile method, while 15 models (27.78%) apply the bias-corrected and accelerated confidence intervals. Supplementing the path coefficient reporting, 86 of 486 models (17.70%) also express the structural model effects in terms of the f^2 effect size, which provides evidence of an exogenous construct's relative impact on an endogenous construct in terms of R^2 changes. While earlier guidelines called for the routine reporting of the f^2 (Chin, 1998), more recent research emphasizes that this statistic is redundant in terms of path coefficients, and concluding that reporting it should be optional (Hair et al., 2022).

The majority of the models report the R^2 (430 models, 88.48%) to support the structural model's quality, which is almost identical to what occurred in the previous period (88.42%). During our review, we noticed that some authors view the R^2 as a measure of their models' predictive power. However, the R^2's computation draws on the entire dataset and, as such, indicates a model's explanatory power (Shmueli, 2010). Assessing the PLS path model's predictive power requires estimating the parameters by means of a subset of observations, and using these estimates to predict the omitted observations' case values (Sarstedt & Danks, 2021; Shmueli & Koppius, 2011). Our review shows that only 11 of the 239 studies (4.60%) use holdout samples to validate their results, possibly because research has only recently offered clear guidelines on how to run a holdout sample validation in a PLS-SEM context (Cepeda Carrión et al., 2016). Alternatively, researchers could draw on Shmueli et al.'s (2016) $PLS_{predict}$ procedure, which implements *k*-fold cross-validation to generate case-level predictions on an indicator level. The procedure partitions the data into *k* subsets and uses *k*-1 subsets to predict the indicator values of a specific target constructs in the remaining data subset (i.e., the holdout sample). This process is repeated *k* times such that each subset once serves as a holdout sample.

TABLE 7 Structural model evaluation

Criterion	Empirical test criterion in PLS-SEM	1981–2010 Number of models reporting (n = 311)	1981–2010 Proportion reporting (%)	2011–2020 Number of models reporting (n = 486)	2011–2020 Proportion reporting (%)
Path coefficients	Values	298	95.82	480	98.77
Significance of path coefficients	Standard errors, significance levels, t values/p values	287	92.28	419	86.21
	Confidence intervals	0	0.00	11	2.26
	Both	0	0.00	43	8.85
Effect size	f^2	16	5.14	86	17.70
Explanatory power	R^2	275	88.42	430	88.48
Predictive power	$PLS_{predict}$[a]	–	–	23	4.73
Predictive relevance (blindfolding)	Q^2	51	16.40	162	33.33
	q^2	0	0.00	15	3.09
Model fit	GoF	16	5.14	60	12.35
	SRMR	–	–	40	8.23
	d_{ULS}	–	–	1	0.21
	d_G	–	–	1	0.21
	RMS_{theta}	–	–	1	0.21
Criterion	**Empirical test criterion in PLS-SEM**	**1981–2010 Number of studies reporting (n = 204)**	**1981–2010 Proportion reporting (%)**	**2011–2020 Number of studies reporting (n = 239)**	**2011–2020 Proportion reporting (%)**
Observed heterogeneity	Categorical moderator	47	23.04	57	23.85
	Continuous moderator	15	7.35	58	24.27
Unobserved heterogeneity	Latent class techniques (e.g., FIMIX-PLS)	0	0.00	10	4.18

[a]Proportion reporting (%) for 2011–2020 uses all 486 models as the base even though $PLS_{predict}$ has only been proposed in 2016.

$PLS_{predict}$ is a relatively new procedure and research has only recently offered guidelines for its use (Shmueli et al., 2019). It is therefore not surprising that only 23 of the 295 models (7.80%) that have been published *after* Shmueli et al.'s (2016) introduction of $PLS_{predict}$ apply the procedure. Instead, many researchers (162 of the 486 models, 33.33%) rely on the blindfolding-based Q^2 statistic, which scholars had long considered a suitable means of assessing predictive power (Sarstedt et al., 2014)—a far greater percentage than in the previous period, when only 16.40% of all models included this assessment. However, recent research casts doubt on this interpretation, noting that this statistic confuses explanatory and predictive power assessment (Shmueli et al., 2016). We, therefore, advise against its use and that of the q^2, which is a Q^2-based equivalent of the f^2 effect size that 15 of the 486 models (3.09%) report.

A controversial and repeatedly raised issue in research is whether model fit—as understood in a factor-based SEM context—is a meaningful evaluation dimension in PLS path models. While some researchers strongly advocate the use of model fit metrics (e.g., Schuberth & Henseler, 2021; Schuberth et al., 2018a), others point to the available metrics' conceptual deficiencies and question their performance in the context of PLS-SEM (e.g., Hair et al., 2019c; Hair et al., 2022; Lohmöller, 1989). Our review shows that model fit assessment does not feature prominently in recent PLS-SEM applications as researchers report model fit metrics for only 103 of the 486 models (21.19%). Most researchers rely on Tenenhaus et al. (2004) GoF metric (60 models, 12.35%), which Henseler and Sarstedt (2013) debunked as ineffective for separating well-fitting models from misspecified ones. Some researchers (additionally) report SRMR (40 models, 8.23%), relying on thresholds proposed by Henseler et al. (2014). The reporting of distance-based measures d_{ULS} and d_G as well as the RMS_{theta} is practically nonexistent in PLS-SEM research (1 model each, 0.21%). Scholars have conceptually discussed (Schuberth & Henseler, 2021) and empirically tested (Schuberth et al., 2018a) the SRMR, which lend this metric some authority. However, given SRMR's performance in research settings commonly encountered in

applied research, Hair et al. (2022, Chap. 6) comment critically on its practical utility. Specifically, in PLS path models with three constructs, the SRMR requires sample sizes of approximately 500 to detect misspecifications reliably. Our review shows that the vast majority of PLS path models are far more complex (Table 5) and rely on much smaller sample sizes, therefore supporting the raised concerns. The same holds for the bootstrap-based test for exact model fit, whose use recent research advocates (Benitez et al., 2020; Henseler et al., 2016a; Schuberth & Henseler, 2021), but which none of the studies in our review applies.

To summarize, we observe some improvements in PLS-SEM-based structural model assessment, with more researchers seeming to be concerned about predictive power evaluations. In this respect, researchers have made increasing use of the Q^2 statistic, which is, however, not a reliable indicator of a model's predictive power. The latter practice is not surprising, since concerns regarding Q^2's suitability have only been raised in rather recent research (e.g., Shmueli et al., 2016). Researchers should therefore follow the most recent recommendations and apply *k*-fold cross-validation by using $PLS_{predict}$ or drawing on holdout samples. Researchers should also be aware of the controversies surrounding the efficacy of extant model fit metrics in PLS-SEM, and carefully check whether their model and data constellations favor their use. Judging by Schuberth et al.'s (2018a) findings, the SRMR and exact fit test will exhibit very weak performance on the average model identified in our review with seven constructs and 280 observations.

3.5 | Advanced modeling and analysis techniques

With the PLS-SEM field's increasing maturation (Hwang et al., 2020; Khan et al., 2019), researchers can draw on a greater repertoire of advanced modeling and analysis techniques to support their conclusions' validity and to identify more complex relationships patterns (Hair et al., 2020a).

For example, many researchers use PLS-SEM because of its ability to accommodate higher-order constructs without identification concerns. Analyzing the prevalence and application of higher-order constructs, we find that 71 of the 239 studies (29.71%) included at least one such construct. The majority of these studies considered second-order constructs (65 studies), while the remaining studies included third-order constructs (5 studies) or both (1 study). Analyzing the measurement model specification of the second-order constructs (Wetzels et al., 2009), we find that most of the studies employ Type I (reflective-reflective; 30 studies), Type II (reflective-formative; 26 studies), or both (4 studies). Only five studies employ Type IV (formative-formative), while no study draws on a Type III (formative-reflective) measurement specification. Analyzing the specification, estimation, and evaluation of the higher-order constructs in greater detail, gives rise to concern in two respects. First, 47 of the studies that employed higher-order constructs do not make the specification and estimation transparent.[2] Those that do, either draw on the repeated indicators approach (15 studies) or the two-stage approach (9 studies). This practice is problematic as endogenous Type II and IV higher-order constructs cannot be reliably estimated using the standard repeated indicators approach (Becker et al., 2012). Second and more importantly, only eight studies correctly evaluate the higher-order constructs using criteria documented in the extant literature (e.g., Sarstedt et al., 2019). The majority of the 71 studies do not evaluate the higher-order constructs at all (30 studies) or incompletely apply the relevant criteria (21 studies), typically disregarding discriminant validity assessment in reflective (i.e., Type I) and the redundancy analysis in formative higher-order constructs (i.e., Types II and IV). Finally, 12 studies misapply the evaluation criteria by erroneously interpreting the relationships between lower- and higher-order components as structural model relationships. Overall, our findings suggest that researchers need to provide much more care in their handling of higher-order constructs by considering the most recent guidelines (Sarstedt et al., 2019).

The identification and treatment of observed and unobserved heterogeneity is another aspect that has attracted considerable attention in methodological research (Memon et al., 2019; Rigdon et al., 2010; Sarstedt et al., 2022). Our review shows that researchers' use of PLS-SEM also mirrors this development. A total of 115 studies (48.12%) in our review took observed heterogeneity into account by either considering continuous (58 studies 24.27%) or categorical (57 studies 23.85%) moderating variables. The strong increase in the percentage of studies using continuous moderating variables compared to the previous period, when only 7.35% undertook corresponding moderation analyses, demonstrates the growing interest in more complex model constellations. Although a total of 57 studies investigated the categorical moderators' impact by means of multigroup analysis (Sarstedt et al., 2011), only 12 address the issue of measurement invariance by using the MICOM procedure (Henseler et al., 2016b). This practice is problematic as establishing measurement invariance is a requirement for any multigroup analysis. By establishing (partial) measurement invariance, researchers are assured that the group differences in the model estimates are not the result of, for example, group-specific response styles (Hult et al., 2008).

While the consideration of moderators facilitates the identification and treatment of observed heterogeneity, group-specific effects can also result from unobserved heterogeneity. Since failure to consider such unobserved heterogeneity can be a severe threat to PLS-SEM results' validity (Becker et al., 2013), researchers call for the routine use of latent class procedures. Recent guidelines recommend the tandem use of finite mixture PLS (FIMIX-PLS; Hahn et al., 2002) and more powerful latent class techniques, such as PLS prediction-oriented segmentation (PLS-POS; Becker et al., 2013), PLS genetic algorithm segmentation (PLS-GAS; Ringle et al., 2014), and PLS iterative reweighted regressions segmentation (PLS-IRRS; Schlittgen

[2]In most cases, the descriptions suggest that the authors averaged the items of the lower-order components to generate indicators of the higher-order construct. This practice is, however, problematic as it ignores the biasing effect of measurement error that PLS-SEM's differential indicator weighting accounts for.

et al., 2016). While FIMIX-PLS allows researchers to reliably determine the segments to extract from data (Sarstedt et al., 2011), techniques such as PLS-POS reproduce the actual segment structure more effectively. Recent research calls for the routine application of latent class techniques to ensure that heterogeneity does not impact the aggregate level results adversely (Sarstedt et al., 2020b). Despite these calls, only 10 of the 239 studies in our review (4.18%) conduct a latent class analysis. All these studies apply FIMIX-PLS, which, while detecting heterogeneity issues well, clearly lags behind in terms of treating the heterogeneity.

Researchers have long noted the need to explore alternative explanations for the phenomena under investigation by considering different model configurations. Such explanations will allow researchers to compare the emerging models by using well-known regression literature metrics (Burnham & Anderson, 2002) to select the best-fitting model in the set. Only 10 of our review studies (4.18%) compare alternative models by using the same data set. By disregarding alternative theoretically plausible model configurations, researchers could fall prey to confirmation biases, since they only look for configurations that support "their" model (Nuzzo, 2015), and miss out on opportunities to fully understand the mechanisms at work (Sharma et al., 2019). Further, very few studies compare multiple models by drawing on a large set of criteria, therefore, applying between one and nine different criteria. However, none of the studies applies the Bayesian Information Criterion (Schwarz, 1978) or the Geweke and Meese (1981) criterion, which prior research identified as superior in PLS-SEM-based model comparisons (Danks et al., 2020; Sharma et al., 2019; Sharma et al., 2021c). With further recent developments in these directions (e.g., based on the cross-validated predictive ability test, CVPAT), we expect the popularity and relevance of the predictive model assessment (Sharma et al., 2021a) and model comparison (Liengaard et al., 2021) to increase.

While our review results point to some progress in terms of applying advanced analysis techniques, there is still ample room for improvement. First, researchers need pay greater attention to the specification, estimation and particularly the validation of higher-order constructs. Second, measurement invariance assessment should precede any multigroup analysis (Henseler et al., 2016b), since failure to establish at least partial measurement invariance renders any multigroup analysis meaningless. Third, researchers should routinely apply latent class analyses to check their aggregate level results' robustness. Failure to do so may result in misleading interpretations when unobserved heterogeneity affects the model estimates. Finally, researchers should more often compare theoretically plausible alternatives to the model under consideration. Considering alternative explanations is a crucial step before making any attempt to falsify a theory (Popper, 1959).

3.6 | Reporting

Hair et al.'s (2012) review emphasized that the algorithm settings used in the analysis needed to be transparent to facilitate its replicability. Our review shows some improvements compared to the previous period, most notably with regard to documenting the software used for the analysis. Whereas less than half of all the studies in the previous period report the software (49.02%), the percentage is much higher in recent PLS-SEM research (193 studies, 80.75%). Most PLS-SEM studies (166 studies, 69.46%) apply SmartPLS, with PLS-Graph following far behind (18 studies, 7.53%). Other rarely used software programs include XLSTAT (4 studies, 1.67%), WarpPLS (3 studies, 1.26%) and ADANCO (2 studies, 0.84%). In addition, two studies (0.82%) use the plspm package of the R software.

Only five studies (2.09%) mention the parameter or algorithm settings, seven studies (2.93%) the computational options (e.g., weighting schemes), and 12 studies (5.02%) the maximum number of iterations. Although making these algorithm settings transparent is certainly useful, different algorithm settings do not normally induce significant deviations in the estimation results. This, however, differs in bootstrapping settings; Rönkkö et al. (2015), for example, show that the use of sign change options can trigger considerable Type I errors, whereas Streukens and Leroi-Werelds (2016) emphasize the need to draw many bootstrap samples (i.e., at least 10,000) from the original data. In this regard, it is encouraging that 175 studies (73.22%) mention the use of bootstrapping, 141 of which provided additional details of the algorithmic settings, such as the number of bootstrap samples used. None of the studies use jackknifing as an alternative means of deriving standard errors, which is likely due to this method only being implemented in the early PLS-Graph software that only a few researchers still use.

Research has produced various variants of the original PLS-SEM algorithm (Becker & Ismail, 2016). One prominent research stream deals with the original PLS-SEM's extensions, to estimate common factor models. While researchers have introduced various such extensions (Bentler & Huang, 2014; Kock, 2019), consistent PLS-SEM (PLSc-SEM) has become the primary technique in this stream (Dijkstra & Henseler, 2015). The use of PLSc-SEM has triggered discussions with certain researchers, who call for its routine application (Benitez et al., 2020; Evermann & Rönkkö, 2021; Henseler et al., 2016a), while others conclude the method "adds very little to existing knowledge of SEM" (Hair et al., 2019c, p. 570). PLS-SEM users seem to share this skepticism as only five studies (2.09%) apply PLSc-SEM.

Finally, we see no increase in the share of studies reporting indicator covariance or correlation matrices. This practice is problematic, because it hinders the results' replicability and checking whether model evaluation metrics, such as the HTMT, are adequate. On the contrary, studies routinely report construct-level correlation matrices (180 studies, 75.31%), largely due to their relevance for Fornell-Larcker-based discriminant validity assessment.

To summarize, reporting practices regarding the software and algorithm settings used have improved since Hair et al.'s (2012) review. Nevertheless, more studies need to make the software and, particularly, the bootstrap settings transparent. Furthermore, SmartPLS (Ringle et al., 2015) is currently the most widely applied software, likely due to its user friendliness (Sarstedt & Cheah, 2019), features, and functionalities (Memon et al., 2021). However, with new software packages, such as

TABLE 8 Checklist for the application of PLS-SEM

Aspect	Recommendation/rules of thumb	Suggested references
General		
Motivate the use of PLS-EM	Motivating the use of PLS-SEM on the grounds of "small sample size" should be done with caution, as small samples affect the method adversely. Do not motivate PLS-SEM use on the grounds of nonnormal data alone.	Hair et al. (2019b); Rigdon (2016)
	Emphasize the causal-predictive nature of the analysis, model complexity, the estimation of conditional process models, and the use of formative measures.	Hair et al. (2019b); Sarstedt et al. (2020a); Sarstedt et al. (2016)
Data characteristics		
Sample size	Run power analyses to determine the required sample size prior to the analysis or, alternatively, apply the inverse square root method. The ten times rule offers only very rough guidance regarding the minimum sample size requirements and should not be used as a justification.	Aguirre-Urreta and Rönkkö (2015); Kock and Hadaya (2018)
Missing values	Report missing values and the use of imputation procedures.	Hair et al. (2022)
Outliers	Detect outliers using univariate or multivariate methods. Delete outliers if necessary or treat them as a separate segment.	Sarstedt and Mooi (2019)
Use of discrete variables	Discrete variables may be used as grouping variables in multigroup analyses or be included as dummy variables. PLS-SEM can process discrete choice modeling data.	Hair et al. (2019a); Hair et al. (2019b)
Model characteristics		
Model type	Specify funnel-like models where the number of predictor constructs in the model's partial regressions are larger than the endogenous constructs. Avoid chain-like models. Use CB-SEM for unfocused models.	Hair et al. (2012); Hair et al. (2021b)
Description of the measurement models	Provide a detailed list of indicators in the appendix; distinguish between reflective and formative measurement.	Hair et al. (2022)
Single items	Generally, refrain from using single items unless measuring observable characteristics, which is commonly done in the form of control variables (e.g., income, sales, number of employees, etc.).	Cheah et al. (2018); Diamantopoulos et al. (2012)
Measurement model evaluation: reflective		
Internal consistency reliability Cronbach's alpha, composite reliability ρ_c and ρ_A	Consider Cronbach's alpha as the lower and ρ_C as the upper boundary of internal consistency reliability. ρ_A should be considered the best point estimate of internal consistency reliability.	Dijkstra and Henseler (2015); Hair et al. (2022)
	Thresholds of all reliability measures: 0.70-0.90 (0.60 in exploratory research, max. 0.95 to avoid indicator redundancy).	
Convergent validity	Interpret the average variance extracted (AVE).	Sarstedt et al. (2021)
	Threshold:	
	AVE ≥0.50	
Discriminant validity	Do not use the Fornell-Laker criterion or cross-loadings, but the HTMT.	Franke and Sarstedt (2019); Henseler et al. (2015)
	Thresholds:	
	HTMT <0.90 for conceptually similar constructs	
	HTMT <0.85 for conceptually different constructs	
	In addition, test whether the HTMT is significantly lower than the threshold value.	

(Continues)

TABLE 8 (Continued)

Aspect	Recommendation/rules of thumb	Suggested references
Measurement model evaluation: formative		
Convergent validity (redundancy analysis)	Use the redundancy analysis to assess the convergent validity; a global single- item or a reflectively measured multi-item scale can be used as an alternative measurement. The construct correlation should be ≥0.70	Cheah et al. (2018); Chin (1998)
Collinearity	Use the variance inflation factor (VIF) to assess collinearity in all sets of formative indicators.	Hair et al. (2019b)
	Thresholds:	
	VIF≤3: no collinearity issues	
	VIF 3–5: possible collinearity issues	
	VIF ≥5: critical collinearity issues	
Indicator weights	Report the indicator weight estimates and information regarding their significance.	Aguirre-Urreta and Rönkkö (2018); Hair et al. (2022)
	For inference testing, report the *t* values, *p* values with standard errors or, preferably, the bootstrapping confidence intervals. Use the percentile method to construct confidence intervals; use the bias-corrected and accelerated bootstrap method for highly asymmetric bootstrap distributions.	
	Consider the indicator loadings of nonsignificant weights. Indicators with nonsignificant and low loadings (<0.5) should be removed from the measurement model.	
Structural model evaluation		
Collinearity	Use the variance inflation factor (VIF) for the collinearity assessment of all sets of predictor constructs in the structural model.	
	Thresholds:	
	VIF≤3: no collinearity issues	
	VIF 3–5: possible collinearity issues	
	VIF ≥5: critical collinearity issues	
Path coefficients	Report the path coefficient estimates and information on their significance.	Aguirre-Urreta and Rönkkö (2018); Hair et al. (2022)
	For inference testing, report *t* values, *p* values with standard errors or, preferably, the bootstrapping confidence intervals. Use the percentile method to construct confidence intervals; if the bootstrap distributions are highly asymmetric, use the bias-corrected and accelerated (BCa) bootstrap method.	
Explanatory power	Report the R^2, but do not consider the metric as indicative of predictive power.	Hair (2021); Hair et al. (2019b); Shmueli and Koppius (2011)
	The R^2 values depend on the model complexity and the phenomena under research. Note that very high R^2 values such as 0.90 are usually indicative of model overfit when measuring theoretical concepts.	Hair et al. (2019b)
Predictive power	Use $PLS_{predict}$ to compare the predictions generated by the PLS path model with those of a naïve linear benchmark model.	Shmueli et al. (2016); Shmueli et al. (2019)

TABLE 8 (Continued)

Aspect	Recommendation/rules of thumb	Suggested references
Model fit	Do not use the GoF index. Apply the SRMR or the bootstrap-based test for an exact model fit, but only if the model complexity and sample size support this step.	Hair et al. (2022); Schuberth and Henseler (2021); Schuberth et al. (2018b)
Advanced modeling and analysis techniques		
Higher-order constructs	The specification, estimation, and evaluation of higher-order constructs require particular scrutiny. Follow the most recent guidelines.	Sarstedt et al. (2019)
Observed heterogeneity	Consider recent guidelines on moderator, multigroup, and conditional process model analyses.	Klesel et al. (2019); Memon et al. (2019); Sarstedt et al. (2020a)
Unobserved heterogeneity	Use latent class analyses as a robustness check to ascertain that unobserved heterogeneity does not adversely impact the aggregate level results.	Becker et al. (2013); Sarstedt et al. (2022); Sarstedt et al. (2020b),
	Use finite mixture PLS in combination with more advanced latent class procedures, such as PLS prediction-oriented segmentation.	
Measurement Invariance	Use the MICOM procedure to at least establish partial measurement invariance as a necessary condition for any multigroup analysis.	Henseler et al. (2016b)
Model comparisons	Consider different model configurations as representations of alternative explanations of the phenomena under consideration.	Liengaard et al. (2021); Sharma et al. (2019); Sharma et al. (2021b)
	Use the Bayesian information criterion (BIC) to select the best model.	
	Consider using BIC-based Akaike weights to quantify each model's relative suitability. Alternatively use the cross-validated predictive ability test (CVPAT).	
Reporting		
Software	Mention the software applied and correctly cite its use, especially if required by the license agreement.	Hair et al. (2012)
Algorithm settings	Report, at least, the following bootstrapping settings: the number of bootstrap samples used (recommendation: 10,000) and the type of bootstrapping confidence interval.	
	Do not use sign change options.	
Correlation matrix	Report the indicator and construct correlation matrices.	Hair et al. (2012)

Abbreviation: PLS-SEM, partial least squares structural equation modeling.

cSEM (Rademaker & Schuberth, 2021) and SEMinR (Ray et al., 2021), as well as the detailed documentation on them (Hair et al., 2021a; Henseler, 2021), we expect the use of R packages to gain traction. Finally, in light of the growing concerns about social science research's replicability (Rigdon et al., 2020), reporting indicator and construct-level correlation matrices should become mandatory.

4 | CONCLUSION

The substantial increase in the number of articles applying PLS-SEM during the last decade shows that PLS-SEM has become an essential element in marketing researchers' methodological toolbox. Our review covers 239 articles published in the top 30 marketing journals between 2011 and 2020—a significant increase compared to those in Hair et al.'s (2012) study, which covered 204 articles published in the same journals between 1981 and 2010.

We find that researchers are currently more aware of the stumbling blocks of PLS-SEM use. Recent research uses PLS-SEM to estimate models with similar average complexity as in the previous period, but drawing on larger datasets, which favors the analyses' statistical power. Our results also suggest that researchers have become more aware of single-item measures' limitations (Diamantopoulos et al., 2012) and of more complex modeling options, like higher-order constructs (Sarstedt et al., 2019) and moderation (Memon et al., 2019). Similarly, researchers have started

accommodating recently introduced model evaluation metrics, like the ρ_A, to assess internal consistency reliability (Dijkstra & Henseler, 2015), the HTMT to assess discriminant validity (Henseler et al., 2015), and Shmueli et al.'s (2016) $PLS_{predict}$ procedure to evaluate a model's predictive power.

At the same time, we also observe a certain degree of latency in other areas of PLS-SEM use. For example, researchers still rely strongly on the Fornell-Larcker criterion (Fornell & Larcker, 1981) and on cross-loadings to assess discriminant validity; they also hardly use a redundancy analysis to establish formative measures' convergent validity. The use of more advanced analyses, such as latent class techniques, is still lacking in applied PLS-SEM research despite persistent calls for their routine use to ensure the results' validity (Becker et al., 2013; Sarstedt & Danks, 2021a; Sarstedt et al., 2017, 2022). Reporting practices in terms of, for example, the software used and the bootstrapping algorithm settings, are still inadequate. The latency with which methodological innovations diffuse in applied research might explain some of these findings, but certainly not all of them. Researchers, reviewers, and editors should pay greater attention to current developments and the latest best practices in PLS-SEM use (e.g., Hair et al., 2022). Table 8 summarizes these best practices in keeping with the aspects covered in our review and serves as a checklist for future applications of PLS-SEM. We also offer common rules of thumb, which offer initial guidance for estimating and assessing models, and provide supporting references for further reading. Figure 2 visualizes the specific aspects that researchers need to

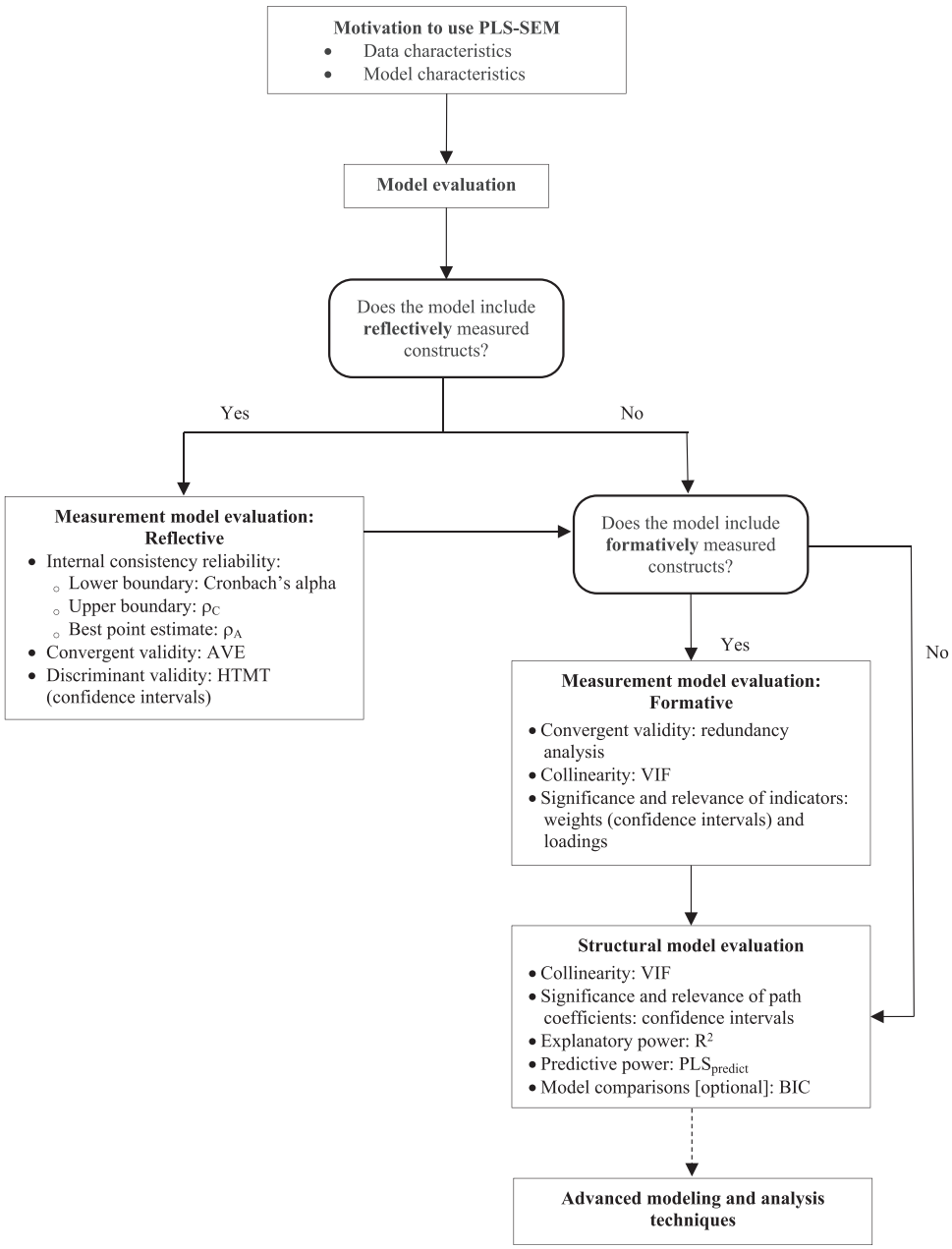

FIGURE 2 A systematic procedure for reporting PLS-SEM results. PLS-SEM, partial least squares structural equation modeling

TABLE 9 Advanced analysis techniques

Procedure	Description	Suggested references
Agent-based simulation	The combination with agent-based simulation (ABS) makes PLS-SEM results dynamic and extends their predictive range. The PLS-SEM agent uses a static path model and PLS-SEM results to determine the ABS settings at the agent level. Then, the dynamic ABS modeling method extends PLS-SEM's predictive capabilities from the individual level to the population level by modeling the diffusion process in a network (e.g., consumers).	Schubring et al. (2016)
Endogeneity	Methods for identifying and treating endogeneity, which occurs when a predictor construct is correlated with the error term of the dependent construct to which it is related.	Hult et al. (2018)
Fuzzy-set qualitative comparative analysis (fsQCA) and necessary condition analysis (NCA) and	In fsQCA, both the independent and dependent variables are calibrated into set membership scores in order to identify (calibrated) independent variables that are sufficient but not necessary for an outcome of the dependent variable. This approach has been transferred to PLS-SEM to analyze necessary conditions in the structural model. Different from the fsQCA, the NCA does not rely on binary necessity statements and therefore considers that an outcome or a certain level of an outcome can only be achieved if the necessary cause is in place or is at a certain level.	Leischnig et al. (2016), Rasoolimanesh et al. (2021), Richter et al. (2020)
Importance-performance map analysis	Allows researchers to gain more insights from the PLS-SEM analysis by contrasting constructs' total effects with their (rescaled) average scores.	Ringle and Sarstedt (2016)
Latent class analysis techniques to identify segments with distinct construct scores	Partial least squares k-means facilitates identifying groups of data that maximize score differences while at the same time accounting for structural and measurement model heterogeneity.	Fordellone and Vichi (2020)
Latent class techniques to identify segments with distinct model relations	Response-based segmentation techniques that identify distinct segments that differ in terms of structural or measurement model relations (e.g., FIMIX-PLS, PLS-IRRS, PLS-POS).	Becker et al. (2013), Sarstedt et al. (2021a), Schlittgen et al. (2016)
Model comparisons	Empirically compare a set of theoretically plausible models.	Danks et al. (2020), Liengaard et al. (2021), Sharma et al. (2019)
Moderated mediation and conditional process analysis	This approach combines the moderator analysis and mediator analysis into a moderated mediation and conditional process analysis in PLS-SEM.	Cheah et al. (2021), Sarstedt et al. (2020a; 2020b)
Nonlinear effects estimation	PLS-SEM assumes linear relationships. In some instances, however, this assumption does not hold in that relationships may be nonlinear and require nonlinear estimations of coefficients in PLS-SEM.	Basco et al. (2021), Sarstedt et al. (2020b)
Weighted PLS-SEM (WPLS)	The WPLS algorithm is a modified version of the original PLS-SEM algorithm that incorporates sampling weights.	Becker and Ismail (2016), Cheah et al. (2021)

Abbreviation: PLS-SEM, partial least squares structural equation modeling.

consider in their application of PLS-SEM, focusing on the illustration of the model evaluation and all relevant metrics.

As evidenced by PLS-SEM's numerous extensions (e.g., Liengaard et al., 2021; Rasoolimanesh et al., 2021; Richter et al., 2020) and by controversial debates (Hair et al., 2021b; Rönkkö et al., 2021), research in the field is highly dynamic (Hwang et al., 2020; Khan et al., 2019). This rapid progress makes it difficult for researchers to keep up with the latest developments and to make up their minds regarding which research stream to follow. For example, our review points to some confusion in terms of model fit assessment. Some researchers seem to be tempted to report a model fit metric, which has become the sine qua non in factor-based SEM research, adopting the GoF index prematurely which, however, cannot separate correctly specified models from misspecified ones (Henseler & Sarstedt, 2013). Very few researchers apply metrics, like the SRMR, which have been tested in simulation studies (Schuberth et al., 2018a), but whose users seem to disregard their quite weak performance in typical model and data constellations. Future research should, therefore, develop better model fit metrics that detect model

misspecifications reliably in standard data and model settings, which reflect those of the various customer satisfaction indices (e.g., Fornell et al., 2020) and technology acceptance models (e.g., Venkatesh et al., 2012).

Recent research also presents various developments that could soon play very important roles in the PLS-SEM field. Hwang and Cho (2020) recently addressed a criticism of the PLS-SEM algorithm by introducing a variant that draws on a single optimization criterion. This extension has the potential to solve various criticisms of the original PLS-SEM method in terms of its modeling options (e.g., introducing model constraints) and model fit assessment. Future studies should further explore this global PLS-SEM method's efficacy for applied research.

Researchers should also advance recently proposed methodological extensions, which we document in Table 9. For example, Richter et al. (2020) subject the PLS-SEM-produced construct scores to a necessary condition analysis (NCA). By following a necessity logic, NCA implies that an outcome—or a certain level of an outcome—can only be achieved if the necessary cause is in place, or is at a certain level (see also Rasoolimanesh et al., 2021). PLS-SEM-based construct scores have also been used in other contexts, such as in the identification and treatment of endogeneity (Hult et al., 2018). Other advances in PLS-SEM address nonlinear effects estimation (Basco et al., 2021) and ensuring the validity of results by uncovering unobserved heterogeneity (Becker et al., 2013; Sarstedt & Danks, 2021). Future research should build on these initial efforts to develop more comprehensive methods that consider the necessity logic and endogeneity before initially computing the construct scores. Such efforts would further enhance PLS-SEM's capabilities and lead to its even more accelerated use in the decade to come.

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CONFLICT OF INTERESTS

The authors declare that there are no conflict of interests.

DATA AVAILABILITY STATEMENT

The data that support the findings of this study are available from the corresponding author upon reasonable request.

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