Double Moderated Mediation Models: Problems and (Part) Remedies

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Abstract

Researchers in management regularly face modelling issues that involve double moderated mediation models. Here, we illustrate how to conceptualise, specify and empirically estimate mediation effects when having to simultaneously account for continuous (Likert type) and nominal (i.e., group) moderator variables. Researchers' estimates of the mediation effects suffer serious bias due to the effects of unaccounted confounders. This is an issue that plagues management research and this work shows how to address these valid reservations for our focus models.

In aiming to inform a wider management audience, we deliberately use the rich context of a focus case since this allows us to clarify the nuances that management researchers face applying double moderated mediation models. Specifically, our focus case is on professionals' willingness to implement a new government policy. We also combine traditional and Bayesian statistical approaches and explain the differences in estimation and interpretation that are associated with the Bayesian approach.

Explaining, and exemplifying the use of, the models we focus on can substantially increase the robustness of the methods employed in management research and can considerably improve the quality of the generated theoretical insights. We also clarify important assumptions and solutions.

Keywords: Moderated mediation, sequential ignorability, Bayesian estimation, Mplus

Introduction

Management researchers regularly face two important problems in their modelling endeavours.

The first problem: This relates to the conceptualising, specifying for, and empirically estimating of indirect (mediation) effects where one moderator is continuous (e.g., a psychological construct) and a second *simultaneous* moderator is nominal (e.g., gender). Traditionally, researchers follow Baron and Kenny (1986) and adopt the logic of an antecedent variable (*X*) influencing an outcome (*Y*) via an intervening mediator variable (*M*). A 'moderated mediation' model is one where a covariate (*Z*) moderates the mediation effect (MacKinnon *et al.*, 2007). The mediated effect varies with the level of the covariate (Valeri and WanderWeele, 2013: 142) (also see Edwards and Lambert, 2007:4). Graphically, mediation is depicted in 'model 4' in Hayes (2013) and moderated mediation is conceptualised in, for instance, models 8 or 59 in Hayes (2013). A high-profile case used by Kline (2011: 333) in explaining the problem is Lance's (1988) study which focused on the relationship between recall accuracy of a lecture script (*Y*), memory demand (*X*), complexity of social perception (*Z*) and an interaction effect (between *X* and *Z*). The model also included a mediator, namely 'recollection of behaviours mentioned in the script' (*M*).

However, testing mediation without *simultaneously* controlling for *both* a continuous and a nominal moderator (like for instance gender as in Lance, 1988) is neither easy nor without biases. Including both moderators enables investigating the complex pathways of co-influence. For instance, a continuous moderator may influence the mediation effect in group A differently/dissimilarly than in group B. This refers to the direction of effect, its shape and the lower/upper bounds. Here, we demonstrate how to conceptualise, specify and empirically test such double moderated mediation models using our context case.

The second problem: This refers to the substantive, and untenable, assumptions implicitly made when identifying direct and indirect effects while modelling mediation (Baron and Kenny, 1986). The validity of commonly used analysis critically relies on safeguarding against the so-called 'sequential ignorability' assumption (Imai *et al.*, 2010a; 2010b). Safeguarding, explained simply, has two parts (Imai *et al.*, 2010: 310): ensuring that there is no unmeasured confounder (meaning a co-influencing, but non-measured, variable) of the *M*-*Y* relationship and that any *M*-*Y* confounder is unaffected by *X* (Muthén, 2011: 8). There is consensus that the latter cannot, under any circumstances, be ensured, and this implies that causal effects cannot be identified (VanderWelle and Vansteelandt, 2009; Imai *et al.*, 2010a; 2010b; VanderWeele, 2010; Muthén, 2011). There are several reasons for this. First, study participants' attribution of scores to questions on predictors and outcomes means that counterfactual outcomes are never observed (Yamamoto, 2012: 239) and so these remain an *unobservable* quantity. Next, the selection of *X* and *M* variables is rarely random. Management researchers may simply be unable to randomise the studied variables in observational studies (Imai *et al.* 2011: 53). Theoretical frameworks in management may contain variables that do *not* vary randomly; and some may even stem from one another (Antonakis *et al.*, 2014). One also cannot preclude the possibility of multiple covariates (i.e., additional predictors) confounding the estimates (Imai *et al.*, 2011). 'Confounding' has been defined primarily as non-modelling model-relevant variables ('confounders') (VanderWeele and Shpitser, 2013) resulting to inaccurate estimates (Antonakis *et al.*, 2010). Next, even if the *X* and *M* variables are randomised, the mediation effects cannot be identified unless an additional constraint, that there is no interaction effect between *X* and *M*, is assumed (Robins, 2003; Imai *et al.*, 2010b: 56). Simply put, without testing the impact of unobserved covariates, the estimates may be distorted and produced theory may be biased. This *plagues* current management research (Antonakis *et al.*, 2010). Antonakis *et al.*, (2010; 2014) observe that many simply fail to understand the seriousness of the matter.

Using moderators, does it diminish the strength of these problems? No, on the contrary. Their existence increases the limitations. These are nicely explained by Valeri and VanderWeele (2013: 138): *"While the concept of mediation, .., is theoretically appealing, the methods traditionally used to study mediation empirically have important limitations concerning their applicability in models with interactions or nonlinearities (Pearl, 2001; Robins & Greenland, 1992)"*. In essence, if there are confounders of the *X-Y*, *M-Y* or *X-M* relationships, these should be controlled for and the sensitivity of the estimates must be tested (Valeri and VanderWeele, 2013: 142). Moreover, sensitivity is about confidence. Even when all the above issues have been addressed, and parameter estimates are adjusted, the degree of confidence in the results is *still* unknown. A sensitivity test identifies upper and lower bounds and quantifies confidence regarding the estimates. These reservations must be addressed to secure robust results. We demonstrate how to adjust –in a tripartite manner- the double moderated mediation estimates for the effect of unaccounted confounders. Specifically, we adapt the Muthén (2011) procedure for estimating the tripartite effects of unaccounted confounders. We also calculate the confidence one can place on the mediation estimates. Summarising, we therefore aim to contribute by explaining and exemplifying:

a) the use of double moderated mediation models accounting for both continuous (note that this is of Likert type in our data) and nominal moderating variables;
b) how to address reservations in such models due to sequential ignorability issues and we focus on the *M-Y* link.

A relevant new aspect is also demonstrating the tripartite manner by which to control for confounders in such models and, at the same time, calculate the confidence in the estimates.

We aim to make these developments accessible to a wide audience of management researchers and we link to graphical representations provided by Hayes (2013) and demonstrate our approach using a context case, explained next.

The context case

In 2008, as part of a wider new Health Market Organization Law, the Dutch government introduced Diagnosis Related Groups (DRGs) in mental healthcare. Implementing DRGs to improve transparency and to control costs is in line with a trend seen in various countries (such as Australia, China, US and Germany) (Kimberly *et al.*, 2009). The previous system meant that the more sessions a mental healthcare professional (such as a psychologist or psychiatrist) had with a patient, the more recompense could be claimed; and this was judged to be inefficient (Kimberly *et al.*, 2009). The DRG policy changed the situation and stipulated a standard rate for each disorder. For instance, for a mild depression, the mental healthcare organisation receives a standard rate, and can treat the patient, directly and indirectly, for between 250 and 800 minutes. This policy has been seen as a shift to more efficient resource use (Hood, 1991:5). However, rather than simply implementing this new DRG policy, psychologists and psychiatrists started to forcefully resist it: they demonstrated against it, set up negative press websites and some even quit their job (Smullen, 2013). In one large-scale survey, about 90 per cent of such professionals wanted the DRG policy to be abandoned (Palm *et al.*, 2008). The following quotation from a healthcare professional is illustrative (cited in Tummers, 2012:516): *"Within the new healthcare system, economic* *values are leading. Too little attention is being paid to the content: professionals helping patients. The result is that professionals become more aware of the costs and revenues of their behaviour. This comes at the expense of acting according to professional standards."*

Willingness to implement the policy (our dependent variable Y)

We use 'willingness to implement the policy' (*Will*) as our dependent variable (*Y*) to reflect our context case of professionals' behavioural intention towards adopting the proposed government policy. Drawing on Metselaar (1997:42), we define willingness to implement a policy as a "positive behavioural intention towards the implementation of modifications in an organization's structure, or work and administrative processes, resulting in efforts from the organization member's side to support or enhance the change process". In our context, willingness to implement the DRG policy amounts to professionals being willing to invest energy in implementing this policy, not intending to sabotage it and being willing to convince colleagues of the benefits of the policy. As a reflection of this intended behaviour, willingness to implement the policy can be assumed to lead to actual behaviour (Fishbein and Ajzen, 2009). Willingness to implement the policy is also a function of both institutional social norms and individual aspects, such as attitudes (Ajzen, 1991; Fishbein and Ajzen, 1975; 2009), which we explain below.

Institutional social norms (our variable X)

Institutionally based social norms such as colleagues' opinions (*COL*) span a continuum from negative to positive, and such opinions can capture the prevalent institutional stance towards altering institutional logics (DiMaggio and Powell, 1983; 1991). A social norm can be defined as "the perceived social pressure to perform or not to perform a behaviour" (Ajzen, 1991:188). Such a social norm is based on the beliefs of 'significant others' towards the focus behaviour. In the case of professionals implementing a policy, the relevant 'significant others' are their own professional colleagues. These colleagues constitute the institutional field in which the individual professionals work (Muzio *et al.*, 2013). Thus, in our case, when colleagues are extremely positive about the new governmental policy, other individual professionals may, due to peer pressure, be more willing to engage in implementing the new policy. Hence, relevant questions will include: do colleagues support the policy, or do they talk negatively about the change during meetings? This 'social norm' is our independent variable (*X*) and would be graphically represented by a direct pathway ($X \rightarrow Y$), where colleagues' opinions (*COL*) affect willingness (*Will*) to implement the new policy.

Attitudes (our mediator variable M)

Individuals interpret institutional social norms in deciding their own behavioural intentions towards institutional logics. Individuals may *not* be willing to implement suggested changes (Dent and Goldberg, 1999; Ford *et al.*, 2008; Higgs and Rowland, 2005; Piderit, 2000) because their personal attitudes towards the focus behaviour are contrary to the social norms. Individuals may have their own individual interpretation of aspects relevant to the proposed institutional logics on the basis of their own knowledge or beliefs. Conversely, positive personal attitudes may positively affect one's willingness to implement a proposed change. In our case, such an attitudinal element is the meaningfulness of the policy for society as perceived by the individual professionals (May *et al.*, 2004). Rewording, *societal meaningfulness* (*SM*) for the professionals is therefore the perception that the policy contributes to socially relevant goals. That is, does the DRG policy benefit society, does it really contribute to, for instance, greater efficiency or transparency? Attitudes are then formed within the framework of a self-expected personal stance towards professional matters. *SM* impacts upon their subsequent willingness or otherwise to implement the new policy.

What is a possible mediational mechanism, and working pathway for the functioning of SM?

We theorise that institutional social norms are precursors to singular views but individual attitudes filter and channel the influence of antecedent social norms through their own individual interpretations of the outcome these norms may bring (Meyers and Vorsanger, 2003; Higgs and Rowland, 2005). Such a conceptualisation can be specified in terms of a mediation effects model (Preacher *et al.*, 2007) where the positive behaviour of colleagues (*X*) results in the willingness of professionals to adopt government plans (*Y*), albeit this relationship is mediated by the degree of societal meaningfulness (our *SM*).

Moderation effects (*our moderator Z and N variables*)

We have argued that individual attitudinal processes, wholly or partially, substitute for and reconfigure the impact of logics to produce an eventual outcome. However, we cannot assume that such impact and reconfiguration takes place irrespective of the context. We would expect aspects of the context, such as professional work context and individual issues related to work, to have an impact. These, it is argued, *condition* the relationship linking social norms, attitudes and intended behaviour. For instance, Freidson (2001) and Powell and Colyvas (2008) suggest that the environment's impact on attitudes and actions is *dependent on* contexts. This introduces the notion of moderation as an influence in our mediation framework.

The first moderator: Job Satisfaction (*JS*) is our first moderator (our variable *Z*) and its interaction term with Col (*X*) is expressed as *ColxJS* (*XZ*). Job satisfaction is seen as one of the core attitudinal outcomes in the work context (Judge *et al.*, 2001) and as a prime candidate to reflect an individual person's contexts and also interpretation of such professional contexts (Griffin *et al.*, 1999). More specifically, social exchange theories (Janssen and Van Yperen, 2004) and identity theory (Ashforth and Mael, 1989; Tyler and Blader, 2001; Ashforth *et al.*, 2008) argue that satisfied employees often have stronger ties with their colleagues. As such, they are more influenced by the attitudes and behaviours of their colleagues, and this provides strong support for accepting *JS* as reflecting individual contexts within a profession and the personal interpretation of the role of that profession. It is thus expected that, particularly for satisfied employees, the behaviour of colleagues will be important for shaping their perceptions of the value of the DRG policy, in turn influencing their willingness to implement it. That is because satisfied people generally feel more attached to their environment, as evidenced in work on social exchange (Janssen and Van Yperen, 2004) and identity theory (Ashforth and Mael, 1989; Tyler and Blader, 2001; Ashforth *et al.*, 2008). Satisfied people are less isolated and care more about what others think and do, and this therefore more strongly shapes their own attitudes and actions. Our theoretical formulation indicates therefore a moderation effect upon two paths, namely: $X \rightarrow M$ and $X \rightarrow$Y denoting at the same time, due to lack of clear theoretical support, exclusion of a moderating influence of Z on the $M \rightarrow Y$ path. In doing so, our model resembles Model 8 of Hayes (2013).

The second moderator: Profession (a nominal variable) is our second moderator (our variable *N*). It has been established that, for people working in individualistic as opposed to collectivistic settings, the influence of social norms on attitudes and behavioural intention is lower (Triandis, 1989; Markus and Kitayama, 1991). In our illustrative case, there are two distinct professional groups that were expected to adopt the proposed government plan: the psychiatry and the psychology professions. These professions can be considered quite different, thereby providing a solid base to treat them as distinct professional fields (Neukrug, 2011). Psychiatrists usually undergo a medical education and are thus medical doctors, whereas psychologists are not. Psychologists have usually received a scientific education before subsequent professional training. Onyett *et al.* (1997) have shown that, of the two groups, psychiatrists work more individualistically and less intensively in teams. They score higher on depersonalisation, a quality which lessens the impact of others on one's own beliefs (Deary *et al.*, 1996; Onyett *et al.*, 1997; Guthrie *et al.*, 1999). On this basis, we would expect the relationship between the behaviour of colleagues and willingness to implement, mediated by societal meaningfulness, to be stronger for psychologists than for psychiatrists.

Answers to the two problems

Answering the first problem, namely modelling double moderated mediation: Our theoretical stance requires a mediational model that simultaneously takes account of two co-influencing conditional processes. The problem is exacerbated because one of these processes is nominal (profession) and the other is a continuous (in our case Likert type) variable. The solution we propose is to specify the above conceptual framing as a double moderated mediation model. This can be summarised using two regression equations. The first regression equation predicts the outcome *Y*, namely the willingness to implement the proposed government plan (*Will*), using the four predictors we have selected as follows:

$$Will = \beta_0^n + \beta_1^n \cdot SM + \beta_2^n \cdot COL + \beta_3^n \cdot JS + \beta_4^n \cdot COL \cdot JS + e_1^n \quad (1)$$

which can be simplified as:

$$Will = \beta_0^n + \beta_1^n \cdot SM + (\beta_2^n + \beta_4^n \cdot JS) \cdot Col + \beta_3^n \cdot JS + e_1^n \qquad (2)$$

Here, β_0^n is the intercept, *COL* is our independent (*X*); *SM* is our mediator *M*, *JS* is our moderator *Z*, *COLxJS* is the interaction term *XZ* and *n* is the group number (n = 1, 2) of our moderator *N*. This results in separate estimates for psychologists and for psychiatrists. For example, β_1^1 refers to the regression coefficient of *SM* upon *Will* for psychologists (Group A), whereas β_1^2 refers to the same coefficient for psychiatrists (Group B). The residual error variances for each group, denoted by e_1^n are assumed to be normally distributed with a mean of zero. All estimates are calculated separately for each group n. The second regression equation predicting the mediator SM is provided below:

$$SM = \gamma_0^n + \gamma_1^n \cdot COL + \gamma_2^n \cdot JS + \gamma_3^n \cdot Col \cdot JS + e_2^n \qquad (3)$$

which can be simplified as:

$$SM = \gamma_0^n + (\gamma_1^n + \gamma_3^n \cdot JS) \cdot COL + \gamma_2^n \cdot JS + e_2^n \qquad (4)$$

Thus, the direct moderation effect is then

$$\beta_2^n + \beta_4^n \cdot JS \qquad (5)$$

and the indirect moderation effect through the mediator *M* is

$$(\gamma_1^n + \gamma_3^n \cdot JS) \cdot \beta_1^n \qquad (6)$$

Adjustments to the demonstrated equations will be required if the researcher follows a different (for instance Model 59 of Hayes, 2013) conceptualisation.

[Insert Figure 1 here]

Answering the second problem: Satisfying the sequential ignorability assumption modelling issue and calculating sensitivity: The classical mediation analysis (usually based upon Baron and Kenny, 1986; and MacKinnon *et al.,* 2002; 2007), or Bollen (1989) in a SEM context, is seriously questioned. The direct and indirect effects identified through the traditional method may not actually be causal (Holland, 1988; Sobel, 2008). There are important issues at stake, and the existing assumptions are simply untenable and unfulfilled in practice (Muthén, 2011:

7). VanderWeele and Vansteelandt (2009) and Imai *et al.* (2010a; 2011) provide a detailed technical and formal background to the assumptions behind the causally defined direct and indirect effects. Focusing on research contexts involving experimental treatments (mostly binary), Valeri and VanderWeele (2011) summarise the assumptions in the modelling as:

(i) There is no unmeasured confounding factor in the treatment (independent *X*) - outcome (*Y*) relationship;

(ii) there is no unmeasured confounding within the mediator (*M*) - outcome (*Y*) relationship;

(iii) there is no unmeasured treatment (independent *X*) - mediator (*M*) confounding;

(iv) there is no mediator (*M*) - outcome (*Y*) confounder affected by treatment (independent *X*).

The last assumption is almost certainly violated, even in ‘random’ data (also see Holland, 1998; Sobel, 2008; Bullock *et al.*, 2010). In brief, it is difficult to defend that the model we investigate here is not immune to unobservable confounder effects. Antonakis *et al*. (2010: 1091) argue that such confounders may relate to group/sample selection, reverse causality, imperfect measures, common-method variance, heteroscedasticity or cluster-robust standard errors in panel data, or, simply, model misspecification.

How can this gap be addressed? Causally defined effects can only inferred more accurately by conducting additional analyses and subjecting the specified models to further constraints (see also Emsley *et al.*, 2010; Muthén, 2011: 3; Valeri and VanderWeele, 2013). Imai *et al*. (2010b) and Muthén (2011) propose different methods to account for the potential confounding effects of unobserved covariates in moderated mediation albeit their focus is on the *M-Y* link. They provide a method to calculate the extent of the impact due to the residual covariance of non-identified covariates. They also suggest an additional sensitivity analysis to test for the lower and upper statistical boundaries of the impact from violating the basic assumptions. We implement in a tripartite way, the Muthén (2011) procedure to measure the impact of unobserved covariates (the variable denoted 'u' in Figure 1). 'Tripartite' refers to estimating the effects of confounders and sensitivity for the mediation pathway $\gamma 1 * \beta 1$ while controlling for two additional pathways, namely $\gamma 2 * \beta 1$ and $\gamma 3 * \beta 1$ (see Figure 2).

[Insert Figure 2 here]

Assumptions

Like Muthen (2011) our sensitivity analysis concentrates only on the possibility of a hidden confounding in the *M-Y* relationship and by definition disallows other confounding – especially affecting the independent (*X*) or the *X-M* relationship (see Antonakis *et al*., 2010: 1091). Implicitly focusing on the M-Y relationship that goes back to logic and research traditions used in areas like clinical trials and epidemiology where experiments (seen as the gold standard) measure the effects of health interventions. Given the design of such experiments and random assignment of participants to control and treatment groups permitted *X* and *M* variables to be conceptualised and treated as exogenous. Later though, researchers suggested that corrections are also required on the effect of hidden confounding in the X–M relationship (example, Jo *et al*., 2011). In addition, it was in economics where they also realised that the assumption of exogeneity regarding the independent *X* may not hold for a variety of reasons too (issue also applicable regarding the mediator). Sample-selection bias may be an issue and Heckman (1979) provided a solution. Another assumption is that *X* is unaffected by random disturbances or measurement error. The use of instrumental variables to correct the estimates was suggested (e.g., Sargan, 1958). Exogeneity regarding the nature of the moderating variables (here *Z* and *N*) is also assumed too. Next, that there is absence of mediated moderation (i.e., no interactions in the effect on outcome) and obviously no further hidden confounding on the direct effect of X on Y. A separate, relevant in management research, source of endogeneity is the assumption of lack of common method variance (CMV) bias (Podsakoff *et al.*, 2003). CMV bias is attributed to simultaneous measurement of multiple constructs and use of single respondents.

What further steps can be taken to test our assumptions? These are explained next. To test and correct the lack of endogeneity regarding the independent (*X*) a researcher can proceed to test for sample-selection bias using Heckman's procedure (see for instance the procedure 'heckman' in Stata) (Clougherty *et al.*, 2016 provide further details). Garen (1984) has provided a remedy for continuous variables. Testing can use a 2SLS or 3SLS estimation (Antonakis, 2010) (see for instance procedure 'reg3' in Stata). Bascle (2008) explain relevant testing and comment on the problem of weak instruments. Testing and correcting for hidden confounders in the *X-M* relationship can employ methods such as propensity scores (see Li, 2013 for further details). Testing and correcting for CMV bias can be implemented via several methods some of which cater for variance which is congeneric (i.e., coming from the same sources of method bias causes) or non-congeneric (i.e., coming from different sources of method bias causes). An excellent start is Lindell and Whitney (2001) who employ the correlation marker approach, albeit the CFA Marker approach may be superior in detecting CMV biases (see Richardson *et al.*, 2009, Williams *et al.*, 2010). Antonakis (2010: 1106-Figure A) also provides a correction to the CMV bias using instrumental variables. Further testing is needed when links between the independent variable and the moderators *Z* and *N* are not orthogonal (i.e., they are correlated). Such assumption (sometimes strong and implausible) is almost certainly violated when several mediators and/or moderators are introduced in the model or if these have common causes themselves. Non-zero error covariance will then likely remain even after correction is applied. Another assumption refers to causal identification which is a different concept to statistical identification (i.e., seeking unique values for each parameter). Additional instrumental variables may be required to help establish causal identification. Every parameter should be "causally identified" (semnet, 2016). Last but not least, in causal reasoning (unlike associational reasoning mostly practiced under a SEM framework), the definition of direct and indirect effects involve quantities that are not all observable: $Y(x)$: the potential values of Y that would have occurred had X been set, possibly counter to fact, to the value x; $M(x)$: the potential values of M that would have occurred had X been set, possibly counter to fact, to the value x. Similarly for $Y(x, m)$ and $Y(x, M(x^*))$. Pearl (2009) clarifies this logic and Bollen and Pearl (2013) provide the overview and delineate the causal assumptions in current SEM practice.

In sum, our effort is a focused insight to correct for confounding in specific parts of a moderated mediation modelling effort which is however also characterized by its own assumptions. Researchers will therefore be advised to clarify the exact nature of their moderated mediation model and carefully consider the assumptions in their effort and the necessary corrections.

Data and measures

Data and measures: We draw our sample data from a population of 5,199 professionals, all members of either the Dutch Association of Psychologists (NIP) or the Dutch Association for Psychiatry (NVvP). The data collection process resulted in 1,307 questionnaires being returned; a response rate of 25%. These included 761 psychologists (our Group A) and 546 psychiatrists (Group B). All the items were measured using a five-point Likert scale, ranging from 'strongly disagree' to 'strongly agree', unless stated otherwise. The dependent variable (Y) was measured using the validated four-item scale of Metselaar (1997), which is based on

Ajzen (1991). A sample item being "I am willing to contribute to the introduction of the DRG policy". The antecedent variable (*X*) was measured using a validated eight-item scale by Metselaar (1997). Here, the respondents could answer either yes (1) or no (0). Sample items are "Colleagues talk negatively about the DRG policy during meetings" (reversed) and "Colleagues support the DRG policy". The collegial behaviour score, a formative measure, is calculated by summing the eight item scores and ranges from 0 (very negative) to 8 (very positive) (Diamantopoulos and Winklhofer, 2001). The mediation variable (*M*) was measured using a five-item validated scale (Tummers, 2012) that allows the researcher to use templates to specify the goal (here, enhancing efficiency in mental healthcare) and the policy to achieve this goal (the DRG policy). A sample item is "Overall, I think that the DRG policy leads to more efficiency in mental healthcare". Our first moderator variable (*JS*) (*Z*) was measured using a single item: 'Overall, I am satisfied with my job'. We opted for a single item measure on the basis that Nagy (2002:85) states that measuring job satisfaction with one item "is more efficient, is more cost-effective, contains more face validity, and is better able to measure changes in job satisfaction". Furthermore, we asked the professionals to indicate their profession (our second, nominal moderator *N*).

Analysis

Measures

First, we present descriptive statistics of the variables in Table 1. Psychologists were more positive than psychiatrists about the DRG policy; for instance scoring more positively (by .24, p<.01) regarding its implementation. All the bivariate correlations for the main variables were statistically significant.

We subsequently carried out a Confirmatory Factor Analysis (CFA) of the latent constructs to be able to report validity and reliability estimates of our factorial structures in line with current practice. The CFA of the latent construct of the *Y* dependent, using maximum likelihood (ML) estimation, exhibited a good fit to the data (RMSEA =.08; CFI=.99; TLI=.98) with standardised factor loadings between .58 and .86. The Average Variance Extracted (AVE) were .56 and .57 and the Composite Reliability (CR) were .83 and .84 for the two groups respectively: values that indicate the measure is valid and reliable. The loadings were also high (> .86) for our mediator *M* (*SM*), with AVE of .83 and .82 and CR of .96 and .95 respectively. Finally, a multiple group model, assuming measurement invariance (Van de Schoot *et al.*, 2012), also demonstrated a good fit to the data (RMSEA=.07; CFI=.98; TLI=.98). Figure 3 shows the loadings on the *SM* and *Y* constructs.

[Insert Table 1 here]

[Insert Figure 3 here]

The item scores for the exogenous measure involved in the interaction *XZ* and for the endogenous *M* measure were centred before the subsequent models' estimation. We centred to eliminate any impact on the statistical identification of priors regarding our variables. 'Priors' refer to what type (shape) of distribution we declare to express our initial uncertainty about our parameters. A Bayesian estimation combines prior distributions of parameters with data likelihood to form posterior distributions for the parameter estimates. Thus, the first reason to centre was to decrease the impact on the distribution of priors used in the estimations. A second reason was to minimise any effect due to multicollinearity between the independent variables, the moderator and the interaction effects. Grand Mean Centring was used as the alternative (Group Centring), would introduce group inequality bias.

Structural Equation Models: use, estimation and interpretation of Bayesian estimates

Why to use Bayesian statistics and what are the differences in interpreting? Using Mplus v7.11 (Muthén and Muthén, 1998-2014; Muthén and Asparouhov, 2012), we employed Bayesian estimation *credibility* intervals (*CI*) (Gelman *et al.*, 2004; Yuan and MacKinnon, 2009) rather than maximum-likelihood-based *confidence* intervals in all the subsequent analyses. We opted for Bayesian statistics primarily because of the usefulness of the interpretations of the Bayesian parameter estimates. Here, one should be aware of the differences in interpretation between the frequentist and the Bayesian approaches. For example, the 95% Bayesian *CI* can be interpreted as the interval that contains the population parameter with a 95% probability and this can be used to determine a significance difference from zero (i.e., the 95% *CI* does not include zero) or significant differences between groups (the 95% *CI*s do not overlap). Second, and quite importantly, we favour Bayesian statistics because when indirect effects are being estimated (for mediation), or interaction effects for moderation, the parameter estimates are never normally distributed and should therefore not be tested using the default Wald test (MacKinnon *et al.*, 2002). Frequentist estimation techniques usually produce symmetric confidence intervals and, therefore, conclusions based on these will be biased. To accommodate the non-normal distribution of indirect or interaction effects, most scholars use bootstrapping to compute asymmetric confidence intervals (Preacher and Hayes, 2008). An alternative procedure is to use a Bayesian approach. Both methods use an iterative process in which all the parameter estimates of the model (e.g., regression parameters, variances, etc.) are estimated and these can then be summarised by plotting the results obtained in each iteration and using this distribution to compute their means and *CI*s. Moreover, technically, a Bayesian approach estimates posterior distributions, whereas a frequentist approach computes only one estimate per parameter. In the Bayesian approach, conditional sampling is used where each iteration is dependent on the previous iteration. This is not the case with bootstrapping (for an in-depth discussion on the differences between Bayesian parameters and maximum likelihood parameters see Kruschke *et al*., 2012 or Van de Schoot *et al*., 2013). When we re-analysed all our models using bootstrapping, there were some numerical differences regarding the estimates but the conclusions drawn would not have been any different if bootstrapping were used. In addition, uninformative priors and large samples result in Bayesian and frequentist results being very similar numerically, but the two approaches allow very different interpretations of these results. While the numerical point estimates may be similar, interpretations of the Bayesian results allow one to draw inferences about the probability of the parameters themselves. Furthermore, there is no reason not to perform the Bayesian computation using construct measures that have been validated using traditional methods.

Decisions to take: A Bayesian estimation requires decisions on several issues explained next. The first decision is whether to use specific (i.e., informative) or non-specific (i.e., uninformative) priors. This constrains the possible range of values that the algorithm can sample from. We used the default of uninformative priors with diffuse (i.e. vague) priors (e.g., $\beta_{\iota} \sim$ N (0, 1.0 +6E); $\sigma_{\iota}^{2} \sim$ IGamma (0.001, 0,001) (Congdon, 2006; Wang and Preacher, 2015). Theoretically driven and empirically tested in previous research, informative priors can lead to the parameter estimates being more accurate and the estimation more efficient. The use of diffuse distributions is however advisable when (as in our case) past theory cannot confidently suggest the distribution shape or the numerical values of the target variables.

A second issue relates to starting values. Since iterations may perform better if one commences from a suitable starting point, we used the maximum likelihood estimates (*ML*) as starting values. To improve the situation further, we also specified that 50 random sets of starting values (all around the *ML* estimates) were to be generated in the initial stage, and 10 optimisations carried out in the final stage before the Monte Carlo Markov Chains (MCMC) chains are initiated. A Markov Chain is a mathematical system that transits from one state to another in a memory-less manner such that the next state depends only on the current state and not on the sequence of events that preceded it (Norris, 1998). MCMC are algorithms (i.e., step-by-step calculation procedures) for sampling from probability distributions in order to build a Markov chain (Fishman, 1995). For our sampling, we used the Gibbs sampling procedure (Gilks *et al*., 1996) which is a 'random-walk' procedure, i.e., one that randomly explores among all possible numerical values. However, Gibbs sampling requires it to be possible to exactly sample all parts of the target distribution. Specifically, Gibbs sampling iteratively draws samples from the assigned conditional distribution of all the parameters. When used with 'diffuse' distributions (i.e., ones that are not predetermined), as here, it ensures representation of all potential numerical values.

A third issue concerns how many of these Gibbs sampling MCMC chains will be employed. We requested as many chains as the processors of the PC we used (namely 8) since this allows faster computation. A fourth issue relates to the number of iterations to be undertaken by each MCMC chain. We have requested a minimum of 20,000 and a maximum of 100,000 iterations. Convergence (with a value of =.01) is confirmed graphically by checking the trace plots and through the use of the Gelman-Rubin test (Gelman *et al.*, 2004). This creates a proportional scale reduction (PSR) factor for each parameter. Smaller PSR values reflect smaller between-chain variations, or greater convergence (should reach <1.05).

Interpretation of the coefficients, especially with reference to the moderating effect: This is important topic. Having used centring, the meaning of the coefficients is altered. The change in the standard deviation of the dependent *Y* (*Will*) as a function of a one standard deviation change in the independent *X* (behaviour of colleagues, *COL*) can be interpreted at different values of the moderator (job satisfaction, *JS*) using the moderator function (*COL*JS*). At the zero mean of *JS*, a standard deviation increase in *COL* (*sdcol*) leads to a *bcol* standard deviation increase in *Will*. At one standard deviation above the mean value of *JS* (where *JS* = 1), an *sdcol* increase leads to a 2*sdbcol* increase in *Will*. At one standard deviation below the mean value of *JS*, a one standard deviation increase in *COL* leads to a -2*sdbcol* decrease in *Will*.

Our models: We specified and empirically estimated three models in our main analysis, and these are explained and interpreted below (see Table 2). Convergence was achieved in all three models with PSR factors <1.03 and excellent trace plot graphs (omitted due to space constraints).

Model 0 (direct effects of X→Y only) identified that professionals being in favour of the DRG policy (i.e. a high *COL*) was positively associated with a willingness to implement (*Will*), both for psychologists (*b*=.42; 95% *CI*=.35 - .48) and for psychiatrists (*b* =.37; 95% *CI*=.29 – .45). In this model, a larger proportion of the variance was explained for psychologists (R^2=17%; 95% *CI*=12 – 23%) than for psychiatrists (R^2=14%; 95% *CI*= 8 – 20%). However, since the 95% *CI*s for the direct associations of the psychologists and the psychiatrists overlap, one cannot claim that the direct effect is different for psychologists and for psychiatrists.

Model 1 specifies *SM* as a mediator together with profession as a moderator variable (see Model 1 in Table 2). The direct effects (*COL*→*Will*) had lower coefficients for both psychologists and psychiatrists than in Model 0. Specifically, in standardised form, the *b* coefficients decreased to .27 (from .42); 95% *CI*=.19 - .34 and to .30 (from .37); 95% *CI*=.22 - .37 respectively. Similarly, the unstandardised β coefficients decreased from 1.61 to 1.03

and from 1.45 to 1.24 respectively. The mediating effect of *SM* is significantly different from zero for both psychologists (β=.62; 95% *CI*=.45 – .81) and for psychiatrists (β=.28; 95% *CI*=.081 – .49) (see also Figure 3). These results indicate that the direct impact (COL→*Will*) is not dissipated, suggesting partial mediation and a two-way process, both direct and indirect, of influence. Further, the mediating effect appears to be higher for psychologists than for psychiatrists. Their *CIs* only just overlap (the upper 95% *CI* boundary for psychiatrists is .49 while the lower 95% *CI* boundary for psychologists is .45). The R^2 of the outcome (willingness) explained when *SM* is added more than doubles in the case of psychologists (from R^2= 17% to 39%; 95% *CI*=32 – 45%) and triples in the case of psychiatrists (from R^2= 14% to 45%; 95% *CI*=37 - 51%). Thus, the partially mediated relationship is strongly dependent on profession.

Model 2 specifies *SM* as a mediator together with both profession and *JS* as moderator variables (see Model 2 in Table 2). The explained variance remained largely at the same levels for both psychologists (R^2= 37%; 95% *CI*=31–44%) and psychiatrists (R^2= 44%; 95% *CI*=38-51%) as in Model 1 (also see Figure 4). However, Model 2 *per se* does not unveil the exact way that the moderator *JS* operates to produce these results. One cannot assume that the moderation effects are in the same direction, of similar shape or have similar lower and upper bounds across the range of values of the moderator. To assess this, we generated a loop using the respondents' moderator scores to test the direction, and the shape of the effects for the two groups. A loop is a sequence of repeated instructions, and the appendix provides the syntax used to estimate the loop (see Model 2 –under the heading '`MODEL CONSTRAINT`'). We used this loop to see how the effect evolves over a range of possible values. Our interest here was on the Likert-type moderator (*JS*) as we wanted to see its effect on the mediated relationship. We could not use the range of the original Likert scale that measured the construct as possible values because the moderator *JS* is centred (= *cJS*) with a mean of zero.

Instead, we set upper and lower bounds of ±2 standard deviations from the mean (e.g., -2 to +2) since this will avoid outlier observations. We also used small steps (0.1) giving 40 steps from -2 to +2 to ensure sufficient cover between the upper and lower bounds (see Figure 5). As can be seen in Figure 4, *JS* apparently has a small but negative effect on *SM* for both psychologists and psychiatrists. Its influence on *Will* is only evident, and again small, for Group B. Here however, the loop results (see Figure 5) reveal that the role of the moderator *JS* upon the *Col*→*Will* link, mediated by *SM*, varies considerably in terms of the direction, shape and the *CI* bounds of the influence. In more detail, with the psychologists (Group A), the influence of *JS* decreases, but never becomes negative. For the psychiatrists (Group B), the influence increases. Yet, this is initially negative and the lower 95% confidence interval bound is only positive for respondents' raw scores of 4 (satisfied) and 5 (very satisfied).

Loop generation: Generating the loops helps to develop and refine theory. In our example case, *JS* attenuates the effect of colleagues' behaviour indicating that the more satisfied psychologists are with their job, the less interested they will be in agreeing to action. For psychiatrists, *JS* has its own direct positive influence on *Will* and only for those who are satisfied or very satisfied, a simultaneous accentuating effect in shaping their perceptions of the value of the policy.

[Insert Table 2 here]

[Insert Figure 4 here]

[Insert Figure 5 here]

Model 3 tests (see Table 2) tests whether the above findings can be sustained under the important condition of ignorability and whether there are any (unaccounted for) confounders.

Muthén (2011) argues that to be able to claim that effects are causal, it is not sufficient to use causally defined effects - rather their identification requires stringent, unverifiable, assumptions. We have adopted a procedure developed by Muthén (2011) to simultaneously test, in a tripartite manner, for the confounding impact of ignored covariates as well as assess the sensitivity of the estimates. The basis of the procedure is as follows. Based upon Pearl's (2009; 2011) mediation formula, the direct effect of *X* (for ease put in a binary form here) (see Muthén, 2011 regarding how this is expressed) is calculated, given the covariate, of the difference between the outcomes when X =1 and X=0 when the mediator is held constant at the value it would obtain for the control group. The total indirect effects are defined following Robins (2003) as (Muthén, 2011), given the covariate, of the difference between the outcomes with X =1 when the mediator changes from the value it would obtain in the X=1 group to the value it would obtain in the X=0 group.

Conducting the sensitivity analysis: A sensitivity analysis (Imai *et al.*, 2010b) is subsequently carried out where the effects are computed for different fixed values of the residual covariance. The estimation commences from a residual correlation of zero (Muthén, 2011). We are interested in the indirect effect of *COL* ($\gamma 1 * \beta 1$) (labelled *g1Acol*b1Acol* for Group A and *g1Bcol*b1Bcol* for Group B (see Figure 2) and so there is a need to control for any additional existing pathways. These relate to the indirect effect of the moderator (*JS*) ($\gamma 2 * \beta 1$) and its interaction (*COL*JS*) ($\gamma 3 * \beta 1$) on *Y through SM*. Specifically for the two groups, these two controlled pathways become:

a) $\gamma 2 * \beta 1$ (labelled *g2Ajs*b1Ajs* for Group A and *g2Bjs*b1Bjs* for Group B)
b) $\gamma 3 * \beta 1$ (labelled *g3Axz*b1Axz* for Group A and *g3Bxz*b1Bxz* for Group B)

To control for the two additional pathways, a concurring tripartite estimation is required. Muthén (2011: 39-40) provides a detailed and technically complex explanation for the single-mediation estimation. In a double moderated mediation model, any ignored covariates affect each pathway differently and so the estimation of the mediation effect $\gamma 1*\beta 1$ (which is our primary focus) must account for ignored covariates in all three pathways. Figure 2 demonstrates the location of each pathway for the concurring tripartite estimation. The numerical sensitivity is estimated at the same time, and this supplies the 95% *CI* upper and lower bounds of the unbiased mediation effects for each pathway (see also Model 3 in Table 2). The appendix provides the syntax used (see under the heading '`MODEL CONSTRAINT`' in Model 3). Although our primary focus is on estimating `indAcol` and `indBcol`, the syntax demonstrates how to estimate the additional pathways.

What is the outcome of testing for non-accounted confounders and the sensitivity analysis?

The results showed that the 'purified' mediational effects for the pathway through *SM*, (unstandardized β): $\gamma 1col* \beta 1col$ (= $\gamma 1* \beta 1$) are for the psychologists .59 with 95% *CI*: .42 - .78; and for the psychiatrists .24 with 95% *CI*: .04 - .45. Thus the effects are always positive for both groups. These results are not that dissimilar to the original mediation estimated effects of societal meaningfulness (β=.62; 95% *CI*=.45–.81 for psychologists and β=.28; 95% *CI*=.08–.49 psychiatrists - see Models 2 and 3 in Table 2). The reductions in the mediation effect due to the previously ignored covariates are not large. Nonetheless, the explained variances are substantially reduced for both psychologists (R^2=20% (from 37%); 95% *CI*=14 – 26%) and for psychiatrists (R^2=16% (from 44%); 95% *CI*=10 - 22%). This decrease is 17% for Group A and 28% for Group B and suggests that unaccounted confounders linked to profession-related variables play a stronger role in Group B. There are also still clear effects of professional context moderation in terms of the mediation pathway (their 95% *CI*s do not overlap although they are close with end values of .42 and .45). The sensitivity of the interaction pathway $\gamma 3*\beta 1$ (for psychologists: -.06 with 95% *CI*: -.23 - +.10; and for psychiatrists: .13 with 95% *CI*: -.06 - +.33) crossed zero in both groups. We interpret this as indicating a lack of a simultaneous effect from the confounding influence of covariates upon the mediation due to the interaction *XZ,* and that this happens in both professions. The sensitivity of pathway $\gamma 2*\beta 1$ (psychologists: .04 with 95% *CI*: .01 - .08; psychiatrists: .05 with 95% *CI*: .01 - .08) is always positive for both groups. We interpret this as indicating a simultaneous effect in terms of the confounding influence of covariates upon the mediation because of *Z,* and this occurs equally for both professions.

Our conclusion is that the *SM* mediation effects passes the sensitivity test and the changes to the coefficients are small. This was also the case with the moderating effect of professional context although the explained variance has decreased. The decrease in R^2 shows that the original error term e_1^n is not only affected by unaccounted confounders; it is also affected inconsistently within each group.

Discussion

We aimed to provide an example of how to conceptualise, specify and estimate models when needing to simultaneously account for double moderated mediation involving nominal and continuous (Likert type) variables (see Cox, 1980 and Mattell and Jacoby, 1972 for the properties of Likert scale measures). We also address reservations concerning the biases inherent to the implicit sequential ignorability assumption that is regularly made in management research. Management researchers regularly address similar contexts and an awareness of what solutions are available is important. Our use of a context case highlights the complexities that regularly face management researchers and new methods, such as proposed here, are best unveiled through similar detailed explanations.

In so doing, we *first* demonstrate *the simultaneous* functioning, and concurring impact, of the relevant causal pathways with both mediation and moderation natures. Using such moderator variables offer to the intellectual debate something beyond and above their contribution as individual elements. For instance, the selected variables enabled us to contrast facets of institutional influence and of self-influence on individual decisions taken and capture their combined effects with respect to simpler mediation models. Such an approach exposes the intermingled nature of the impacts of contexts and the complex nature of resulting causal pathways of concurring influence.

Second, our empirical implementation provides a way to resolve important empirical problems facing researchers by applying novel statistical techniques. We demonstrate in our modelling how to use Bayesian statistics and their value when accounting for both dichotomous and continuous moderators in a mediation context.

Third, we demonstrate how to formulate such a model for an investigation of confounding effects. We demonstrated, by testing for variables that are conventionally ignored, how the explained variance of the dependent variable *Y* can change substantially. Here, Imai *et al.* (2010b) and Muthén (2011) argued that the assessed impact of confounding effects should be supplemented by an estimation of the sensitivity of these results. Our study is one of the first to investigate the problematic issue of confounder ignorability that plagues much past management research (Antonakis *et al.* 2010)We concentrated on only the M-Y relationship and clarified the number of assumptions inherent in such modelling efforts.

Based on our findings, we would advise researchers when conceptualising their theoretical problem as a double moderated model mediation to carefully address the following issues:

a) What is the nature of the mediation paths, the independent and dependent variables and also the pattern of responses for these variables? Categorical or nominal variables introduce important statistical estimation issues, especially when the interaction terms or mediation variables are non-continuous. Following from this, care is needed in checking the pattern of responses (these may also be censored or truncated). In addition, if fewer points are used by respondents (say 4 responses on a 7 point Likert scale), the responses cannot be assumed as continuous. Patterns of missing values is also a pertinent aspect.

b) What is the exact nature of the moderation variables, and also what is the pattern of responses?

c) What is the nature of the interface among the moderator variables? One should attempt to identify any multilevel effects among moderators and/or the mediator and the dependent variable (see also Preacher *et al.*, 2007). The interface between two simultaneously controlled moderator variables may hide substantial conceptual causal links between them and also disguise data pattern issues. Constructs/variables on different levels (e.g., level 0/1/2) all inherit variance that is attributable to their conceptual location and theoretical role. Thus, model conceptualisation and specification at the same level will inevitably confound variances attributable to the conceptual level of the construct/variable. Consequently, double moderated mediation models should be used with care, and researchers be alert to clustering effects that are inherited by variables/latent constructs on different levels.

d) What are the multipartite pathways of concurring unaccounted covariates? Research should specify and estimate mediation effects while also simultaneously controlling for theoretically driven co-influencing pathways.

e) What are the correct interpretations of the identified coefficients? Grand-centring effects lead to different interpretations than group-centring effects. Similarly, interactions bear a meaning that is pertinent not only to the underlying nature of the involved variables but also to the distribution of respondent responses. Again, attention needs to be given to interpretation difficulties with higher-order interactions. Confusion can easily result and theoretical interpretations become less than robust.

f) What analytical approaches should one use? We employed a combination of traditional and Bayesian estimation approaches to reap the benefits of both. Research can benefit greatly from the increased sophistication and precision allowed by Bayesian approaches. For instance, research could employ different informative priors (i.e., different averages) and/or different breadths (e.g., narrower versus wider standard deviations) as well as distributional shapes to contrast alternative theoretical stances.

g) What are the direction, the shape and the lower/upper bounds across the entire range of moderator values?

h) Assumptions inherent in the model and potential biases require testing and correction. These may be strong and sometimes implausible, make modelling efforts complex and require researcher energy but are important in order to secure accuracy of estimates.

Having highlighted issues for further consideration and alert about the need for a valid approach to such analyses (see also Kline, 2015), we see the current endeavour as a potential stepping-stone towards improved conceptualisation of pertinent theoretical issues, increased methodological robustness and a reduction in the analytical errors that can all, too easily, occur.

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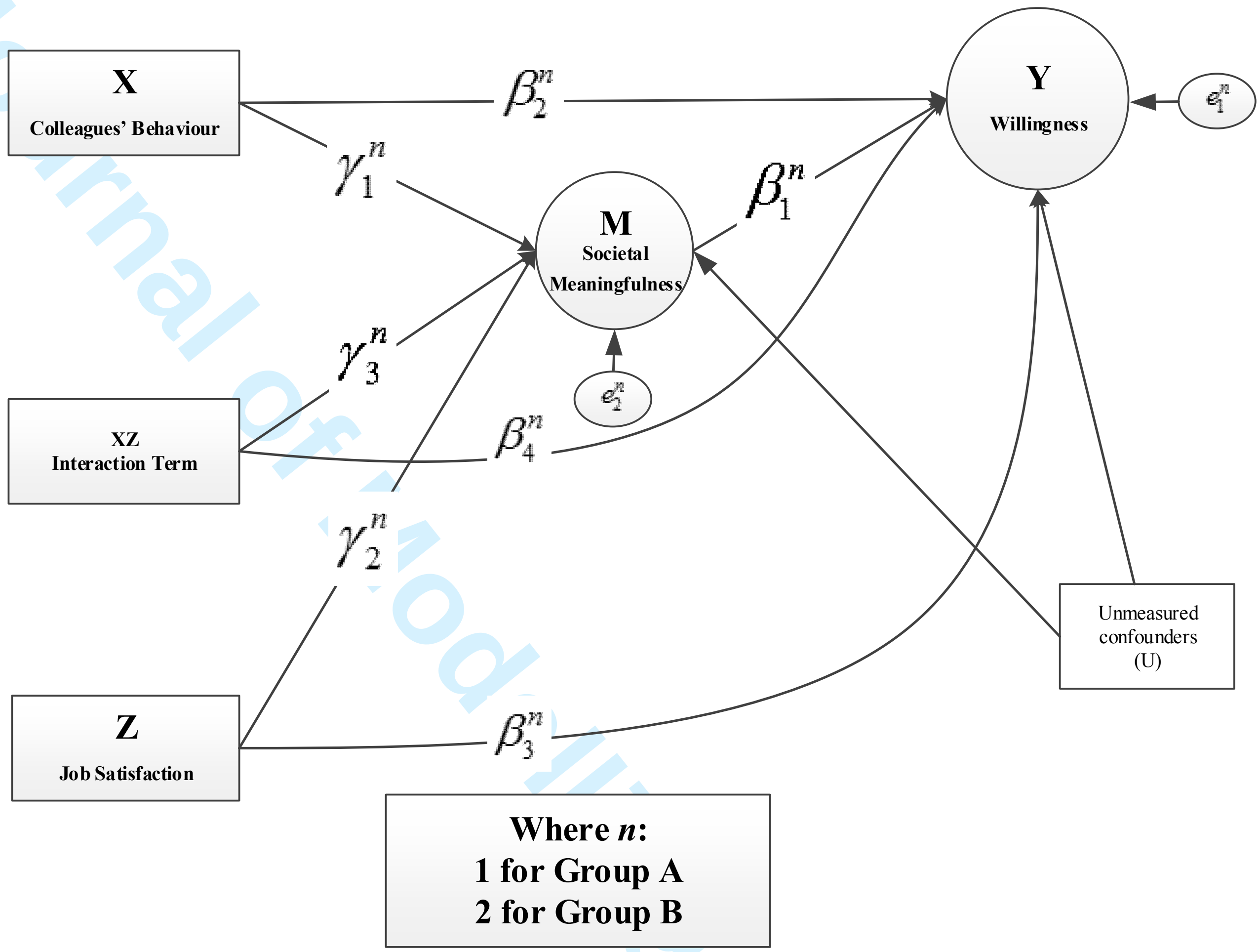

Figure 1. Depiction of the theoretical framework and the effect of unaccounted confounders

Note: The figure depicts the influence of professional colleagues (*our independent variable X (=COL)*) being in favour of implementing the DRG policy (our dependent *Y (=Will)*) is mediated by societal meaningfulness (our Mediator *M (=SM)*) and moderated by job satisfaction (our moderator Z *(=JS)*) and by type of healthcare professional (our moderator *N (1=Group A / 2=Group B)*). It also shows why the moderated mediation effects are biased due to the effects of unaccounted confounders (U) in the link between the mediation and outcome.

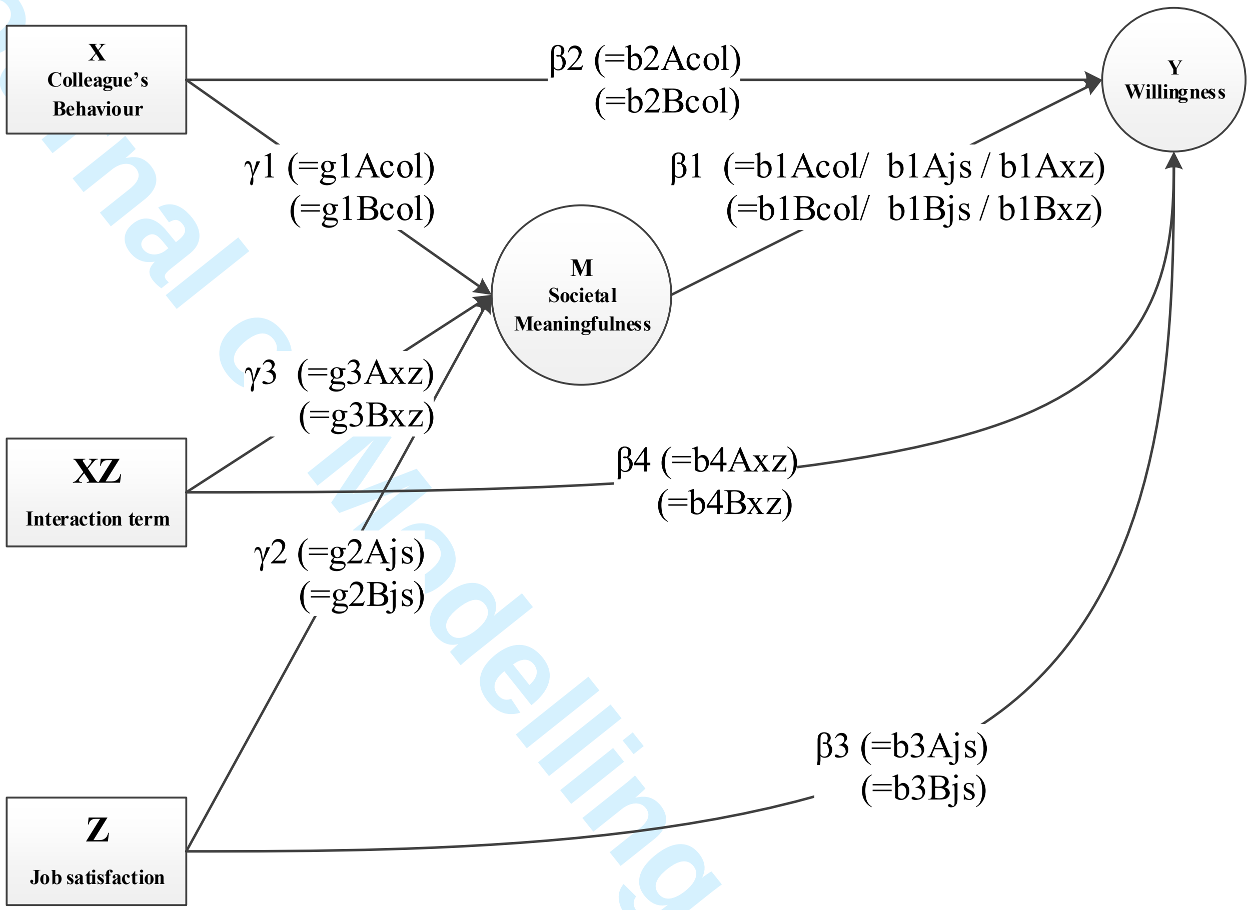

Figure 2. Conceptual framework for testing the sensitivity of double moderated mediation

Note: mediation pathway: $\gamma 1 * \beta 1$
(notation used in the syntax: Group A =g1Acol*b1Acol; Group B=g1Bcol*b1Bcol)

controlled pathway: $\gamma 2 * \beta 1$
(notation used in the syntax: Group A =g2Ajs*b1Ajs; Group B=g2Bjs*b1Bjs)

controlled pathway: $\gamma 3 * \beta 1$
(notation used in the syntax: Group A =g3Axz*b1Axz; Group B=g3Bxz*b1Bxz)

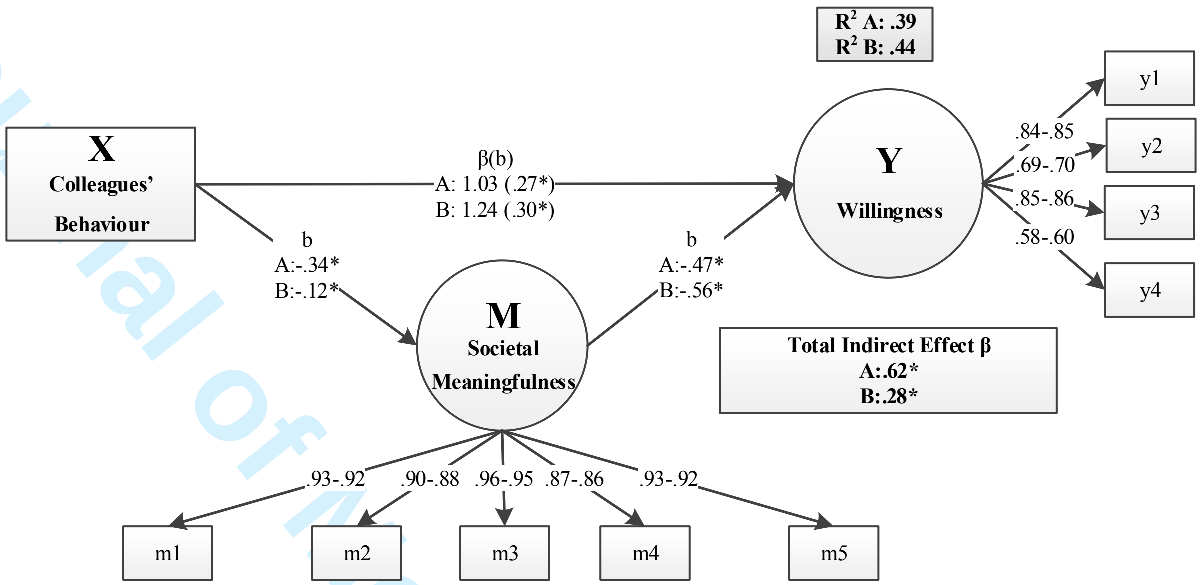

Figure 3. Single moderated mediation model estimates (by professional context) (β = Unstandardised Coefficients; b= Standardised Coefficients)

Note:

A refers to estimates for Group A (Psychologists) and B refers to estimates for Group B (Psychiatrists).

The single moderated mediation effects are indicated in the figure as Total Indirect Effect β.

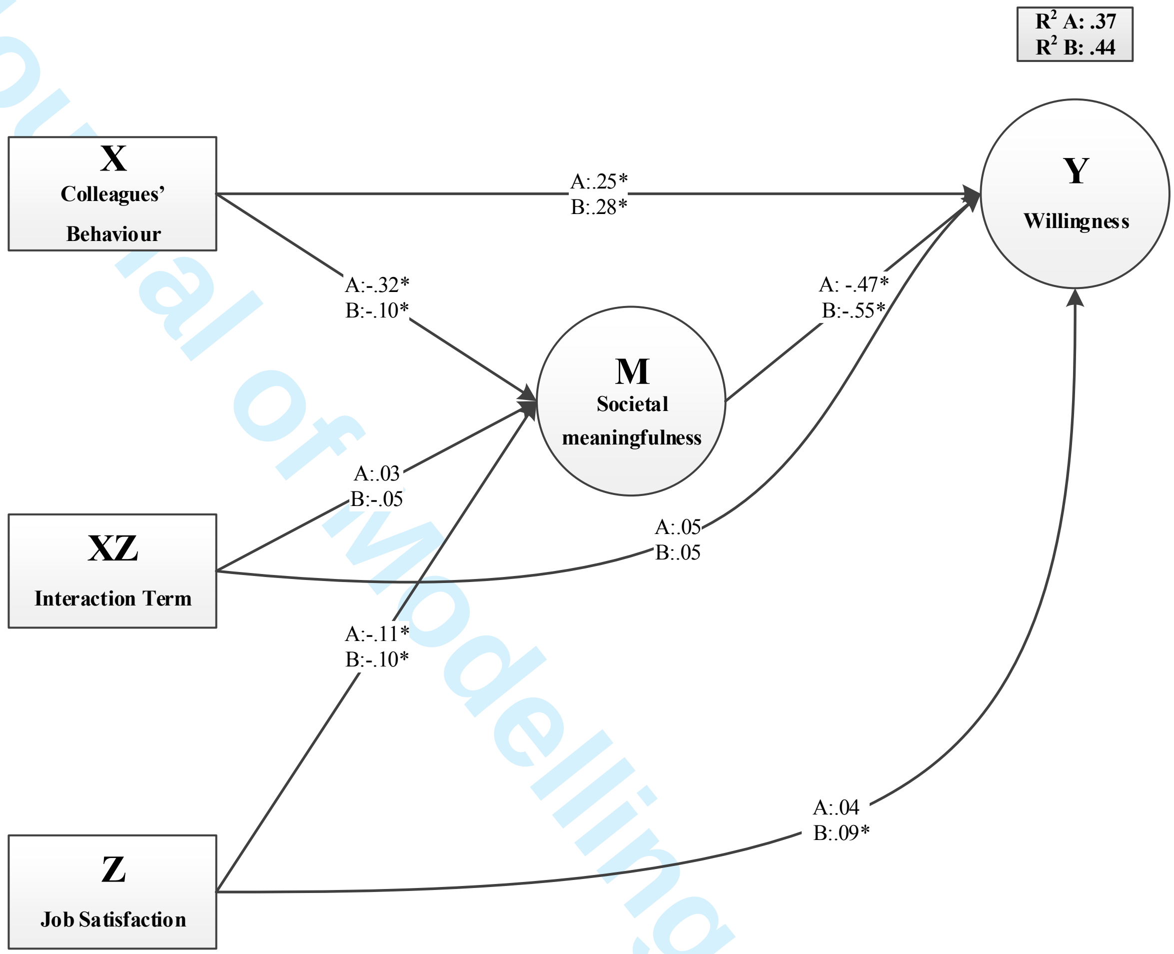

Figure 4. Double moderated mediation model, standardized (b) estimates (A= Estimates for Group A; B= Estimates for Group B)

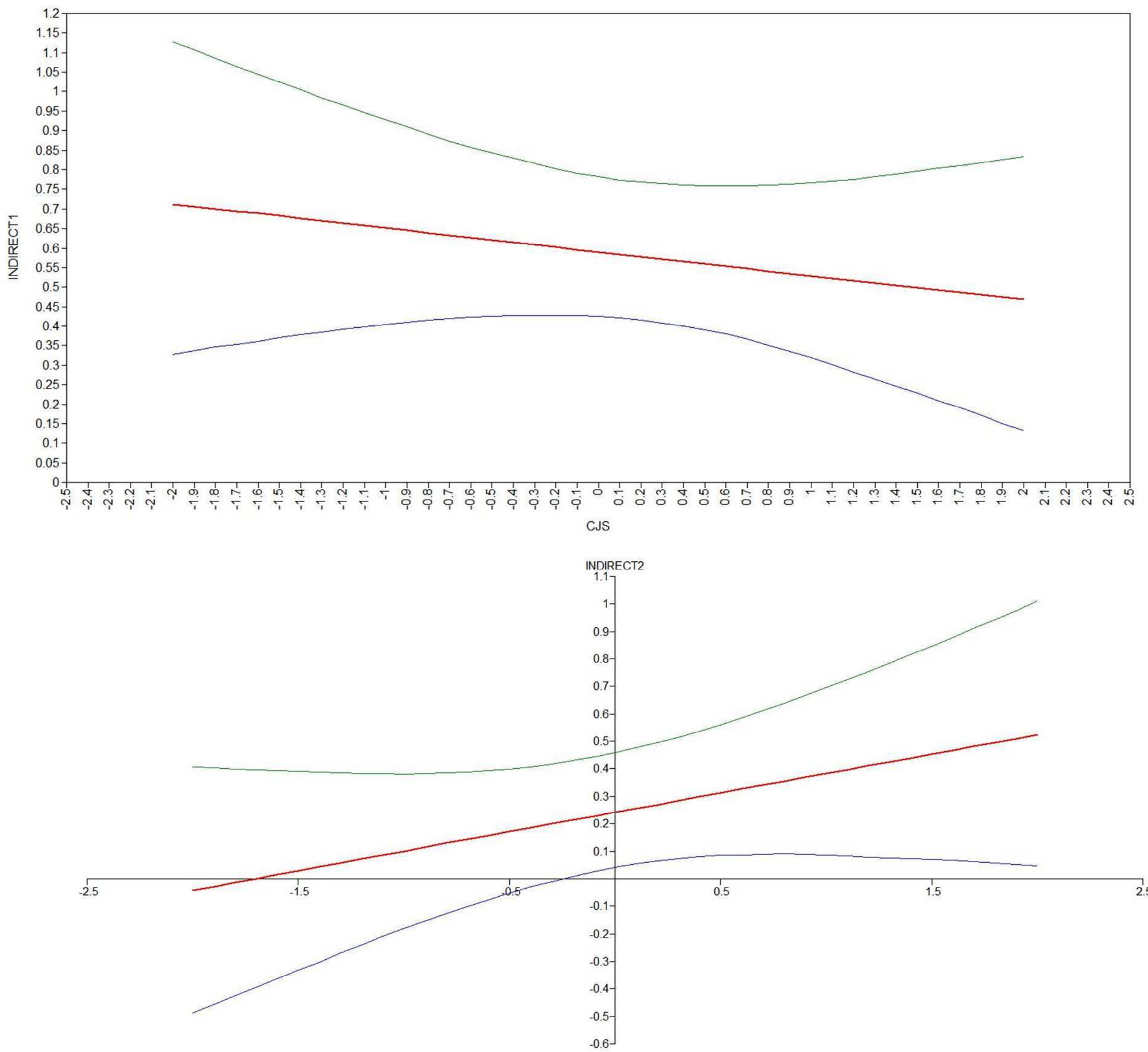

Figure 5. Moderated mediation for Group A (upper) and Group B (lower)

Note: Figure shows the moderating effect of job satisfaction (our moderator Z = *JS*) on the relationship between the behaviour of colleagues (our independent X = *COL*) and willingness to implement the DRG policy (our dependent Y =*Will*), mediated by societal meaningfulness (our mediator M =*SM*)

Table 1 Variables, means and (standardized) correlation coefficients for Group A (psychologists) and Group B (psychiatrists) (all correlation coefficients *p*< 0.001 unless otherwise stated)

Variable	Y(Will)	X(COL)	M(SM)	Z(JS)
Y=Willingness to implement (*Will*)[a]	.24/.00[b]			
X= Colleagues' Behaviour (*COL*)	.44/.35	.49/.45		
M=Societal Meaningfulness (*SM*)[a]	-.57/-.59	-.34/-11*	-.37/.00 [b]	
Z=Job Satisfaction (*JS*)	.18/.18	.22/.17	-.17/-.10*	4.22/3.95 [b]

Note:

In each table cell, the left hand value relates to estimates for Group A (psychologists) and the right hand value to estimates for Group B (psychiatrists). The means for Job Satisfaction (*JS*) are based on a single item while the means for *COL* are calculated from a formative index. The latent mean scores for *Will* and *SM* were obtained using CFA where the latent mean scores for Group A are estimated but fixed at zero by default for Group B.

* p<.05

[a] Since these constructs are latent variables, the means are standardized with Group B (psychiatrists) being used as the reference group.

[b] Significant group difference, p<.01

Table 2: Structural Paths: Unstandardized β (Standardized b) Parameter Estimates per group (Group A= Psychologists; Group B= Psychiatrists)

	Group A		*Group B*	
Structural Path (*coefficient*)	**β (b)**	**b 95% C.I.**	**β (b)**	**b 95% C.I.**
Model 0 (No Mediation)				
Intercept *Will* (β_{0i})	.15* (.21*)	.09-.33	0[a]	-
COL→*Will* (β_2)	1.61* (.42*)	.35- .48	1.45* (.37*)	.29- .45
Residual Variance *Will* (e_1)	**.42* (.82*)**	**.76- .87**	**.42* (.86*)**	**.79- .91**
Explained R^2 of Will	**.17**	**.12-.23**	**.14**	**.08-.20**
Model 1 (Single Moderated Mediation)				
Intercept *Will* (β_0)	.05 (.07)	-.04- .18	0[a]	-
SM→*Will* (β_1)	-.35* (-.47*)	-.54- -.40	-.46* (-.56*)	-.61- -.49
COL→*Will* (β_2)	1.03* (.27*)	.19- .34	1.24* (.30*)	.22- .37
Intercept *SM* (γ_0)	-.30* (-.32*)	-.44- -.20	0[a]	-
COL→*SM* (γ_1)	-1.76* (-.34*)	-.41- -.27	-.60* (-.12*)	-.20- -.03
Residual Variance *Will* (e_1)	**.30* (.60*)**	**.54- .67**	**.30* (.55*)**	**.49- .62**
Residual Variance *SM* (e_2)	**.78* (.87*)**	**.82- .92**	**.78* (.98*)**	**.95- .99**
Explained R^2 of Will	**.39**	**.32-.45**	**.45**	**.37-.51**
Explained R^2 of SM	**.12**	**.07-.17**	**.01**	**.001-0.04**
Indirect (mediation *β* effect) (COL->SM->Will)	**.62***	**.45-.81**	**.28***	**.08-.49**
Model 2 (Double Moderated Mediation)				
Intercept *Will* (β_0)	.03 (.05)	-.05- 1.16	0[a]	-
SM→*Will* (β_1)	-.35* (-.47*)	-.54- -.40	-45* (-.55*)	-.61- -.48
COL→*Will* (β_2)	.97* (.25*)	.17- .32	1.17* (.28*)	.21- .35
JS→*Will* (β_3)	.03 (.04)	-.03- .12	.08* (.09*)	.02- .16
*COL*x*JS*→*Will* (β_4)	.22 (.05)	-.02- .13	.24 (.05)	-.01- .12
Intercept *SM* (γ_0)	-28 (-.30)	-.42- -.18	0[a]	-
COL→*SM* (γ_1)	-1.68* (-.32*)	-.39- -.24	-.53* (-.10*)	-.19- -.01
JS→*SM* (γ_2)	-.12* (-.11*)	-.19- -.02	-.11* (-.10*)	-.19- -.02
*COL*x*JS*→*SM* (γ_3)	.17 (.03)	-.05- .11	-.31 (-.05)	-.14- .02
Residual Variances *Will* (e_1)	**.30* (.62*)**	**.55- .68**	**.30* (.55*)**	**.48- .62**
Residual Variances *SM* (e_2)	**.78* (.87*)**	**.82- 92**	**.78* (.97*)**	**.94- .99**
Explained R^2 of Will	**.37**	**.31-.44**	**.44**	**.38-.51**
Explained R^2 of SM	**.12**	**.08-.17**	**.02**	**.008-.06**
Model 3 (Sensitivity of Mediation Effects in the Double Moderated Mediation)				
Mediation pathway: *γ1col*β1col*	**.59***	**.42-.78**	**.24***	**.04- .45**
Controlled pathway: *γ3xz*β1xz*	-.06	-.23- .10	.13	-.06- .35
Controlled pathway: *γ2js*β1js*	.04*	.01- .08	.05*	.01- .09
Explained R^2 of Will	**.20**	**.14-.26**	**.16**	**.10-.22**
Explained R^2 of SM	**.13**	**.08-.19**	**.03**	**.00-.06**

Fit indices	Model 0	Model 1	Model 2	Model 3
Df	15	35	50	45
Bayesian Posterior Predictive *p*-value	0.001	0.000	0.000	0.000
Deviance (DIC)	9703.891	17264.512	18371.851	18.003
Estimated number of parameters (pD)	11.951	25.335	50.764	32.499
Bayesian (BIC)	9784.543	17457.440	18623.651	18251.856
Group A				
Posterior Predictive *p*-Value	0.055	0.000	0.000	
Deviance (DIC)	4013.032	8430.328	10403.997	
Estimated number of parameters (pD)	8.975	3.529	18.579	
Group B				
Posterior Predictive *p*-Value	0.011	0.000	0.000	
Deviance (DIC)	3825.896	7051.773	7885.651	
Estimated number of parameters (pD)	6.111	28.655	-3.559	

[a] these parameters are fixed at zero so that they can serve as a reference category.

Appendix – Input Syntax

```
MODEL 2
DATA: FILE IS D:\name.dat                              !File location
VARIABLE: NAMES ARE variables in dataset follow here   !not mentioned here
USEVARIABLES ARE                                       !names of used variables
y1 y2 y3 y4                       !dependent (Y)
m1 m2 m3 m4 m5                    !mediating variable (M)
ccol                              !formative independent (X) variable centered
cJS                               !moderating variable (Z) centered
xz;                               !moderating effect = ccol*cjs (interaction effect)
                                  !XZ needs to be declared here; it is defined later

MISSING ARE ALL (-9999);          !How missing values were coded
!GROUPING IS BCPsych (0 = psychologists; 1 = psychiatrists)
KNOWNCLASS IS g (BCPsych=0 BCPsych=1);
!Identify how each group is coded in the dataset
CLASSES IS g(2);
!We have two groups (g here refers to our notation n (N0= Group A; N1= Group B)

DEFINE:                           !Section defines new variables
!We first request centering of the observed using grandmean
CENTER m1 m2 m3 m4 m5 (GRANDMEAN);
xz = ccol*cjs;                     !The moderation interaction XZ is computed

ANALYSIS:                         !Section identifies how to perform the analysis
type is mixture;                  !treats as a mixture model
estimator is bayes;               !request Bayesian estimation
chains is 8;                      !requests 8 chains
processors is 8;                  !requests use of 8 logical processors
stvalues = ml ;                   !request to use ML estimates as starting values
bseed is 10000;                   ! seed for MCMC random number generation;
biterations 100000(20000);       ! maximum (minimum) iterations for each MCMC
bconvergence = .01;              !convergence criterion
starts 50 10;
!specifies that 50 random sets of starting values are generated in the
!initial stage and 10 optimizations are carried out in the final stage

MODEL:                            !Section = model specification
%overall%                         !overall model
!Our dependent Y (Willingness Factor= Will)
Will by y1@1
y2-y4 (1-3);
[y1-y4](10-13);

!Our Mediator M (Societal Meaningfulness Factor= SM)
SM BY m1@1
m2 m3 m4 m5 (101-104);
[m1 m2 m3 m4 m5] (110-114);

Will on SM ccol cJS xz;           !Equation (1)
SM on ccol cJS xz;                !Equation (2)
cJS;
xz;

%g#1%
!Section requests to re-run the model for Group A
!Equation 1 (below) & 2 (further below); each predictor has been assigned a label
Will on
SM             (b11)
ccol           (b21)
cJS            (b31)
xz             (b41)
```

```
;

SM on
ccol          (g11)
cJS           (g21)
xz            (g31)
;


%g#2%
!Section requests to re-run the model for Group B
[Will@0];
!Fixes Y factor means at zero so estimates can have a meaningful interpretation
[SM@0];
!Fixes M factor means at zero so estimates can have a meaningful interpretation

Will on
SM            (b12)
ccol          (b22)
cJS           (b32)
xz            (b42)
;

SM on
ccol          (g12)
cJS           (g22)
xz            (g32)
;

MODEL CONSTRAINT:
!Section estimates the Loop for the moderated mediation per Group A and Group B

PLOT(indirect1 indirect2 direct1 direct2);
LOOP(cJS, -2, 2, 0.1);                  !provides the range of values & step =0.1
indirect1 = b11*(g11+g31*cJS);          !moderation effect on indirect-A Group
direct1 = b21+b41*cJS;                  !moderation effect on direct-A Group
indirect2 = b12*(g12+g32*cJS);          !moderation effect on indirect-B Group
direct2 = b22+b42*cJS;                  !moderation effect on direct-B Group

PLOT:
!Section requesting the plot
TYPE = PLOT2;
sformat=hdf5;

OUTPUT:
TECH1 TECH8 STAND(STDYX);
! Standardisation simultaneously considers both dependent and independent variables
```

```
MODEL 3
DATA: FILE IS D:\name.dat;
VARIABLE: NAMES ARE variables in dataset follow here
USEVARIABLES ARE                        !as explained in Model 2
Y1 y2 y3 y4
m1 m2 m3 m4 m5
ccol
cJS
xz;

MISSING ARE ALL (-9999);                !as explained in Model 2
KNOWNCLASS IS g (BCPsych=0 BCPsych=1);
CLASSES IS g(2);

DEFINE:                                 !as explained in Model 2
CENTER m1 m2 m3 m4 m5 (GRANDMEAN);
xz = ccol*cjs;

ANALYSIS:                               !as explained in Model 2
type is mixture;
estimator is bayes;
chains is 8;
processors is 8;
stvalues = ml ;
bseed is 10000;
biterations 100000(20000);
bconvergence = .01;
starts 50 10;

MODEL:                                  !as explained in Model 2
%overall%
!Willingness Factor
Will by y1@1
y2-y4 (1-3);
[y1-y4](10-13);

!Societal Meaningfulness Factor
SM BY m1@1
M2 m3 m4 m5 (101-104);
[m1 m2 m3 m4 m5] (110-114);


[Will]         (k0);                    !means of Y is given the label k0
Will on ccol   (b2col)                  !b2 is given the label b2col
          xz   (b4xz)                   !b4 is given the label b2xz
         cjs   (b3js);                  !b3 is given the label b3js
[SM]           (g0);                    !means of M is given the label g0
SM on ccol*1   (g1col)                  !g1 is given the label g1col
       xz *1   (g3xz)                   !g3 is given the label g3xz
      cjs *1   (g2js);                  !g2 is given the label g2js
Will *1        (sig);                   !variance of Y is given the label sig
SM *1         (sig2);                   !variance of M is given the label sig2
Will WITH SM   (cov);                   !covariance of Y with M is given the label cov

%g#1%
!Section requests to re-run the model for Group A
 [Will]        (k0A);                   !same as above-added 'A' in label for Group A
Will on ccol   (b2Acol)
          xz   (b4Axz)
         cjs   (b3Ajs);
[SM]           (g0A);
SM on ccol*1   (g1Acol)
      xz  *1   (g3Axz)
```

```
      cjs *1   (g2Ajs);
Will *1        (sigA);
SM *1         (sig2A);
Will WITH SM  (covA);


%g#2%
!Section requests to re-run the model for Group B
 [Will@0]     ;                         !fixed at 0
Will on ccol  (b2Bcol)                  !same as above-added 'B' in label for Group B
        xz    (b4Bxz)
         cjs  (b3Bjs);
[SM@0]              ;                   !fixed at 0
SM on  ccol *1(g1Bcol)
         xz *1(g3Bxz)
        cjs *1(g2Bjs);
Will*1         (sigB);
SM *1         (sig2B);
Will WITH SM  (covB);


MODEL CONSTRAINT:
!Section applies the Muthen (2011) procedure.
!We are primarily interested in estimating the bias for the effect
! γ1X*β1M which is: for Group A = g1Acol*b1Acol & for Group B = g1Bcol*b1Bcol;
!This estimation requires however controlling for the effects γ2Z*β1M & γ3ZX*β1M
!which are in our notation below respectively:
!for Group A = g2Ajs*b1Ajs & for Group B = g2Bjs*b1Bjs;
!&
!for Group A = g3Axz*b1Axz & for Group B = g3Bxz*b1Bxz;
! the primary interest focuses on estimating indAcol and indBcol

New(
! section below specifies the parameters to estimate for the mediation pathway g1*b1
! for the two different Groups (A & B)

rhoAcol rhocAcol b1Acol b2Acol b0Acol sig1Acol indAcol dirAcol
rhoBcol rhocBcol b1Bcol b2Bcol b0Bcol sig1Bcol indBcol dirBcol

! section below specifies the parameters to estimate for the controlled pathway g3*b1
! for the two different Groups (A & B)

rhoAxz rhocAxz b1Axz b2Axz b0Axz sig1Axz indAxz dirAxz
rhoBxz rhocBxz b1Bxz b2Bxz b0Bxz sig1Bxz indBxz dirBxz

! section below specifies the parameters to estimate for the controlled pathway g2*b1
! for the two different Groups (A & B)

rhoAjs rhocAjs b1Ajs b2Ajs b0Ajs sig1Ajs indAjs dirAjs
rhoBjs rhocBjs b1Bjs b2Bjs b0Bjs sig1Bjs indBjs dirBjs
);


! section below specifies how to estimate the parameters re: mediation pathway g1*b1
! for Group A

rhocAcol=covA/(sqrt(sigA)*sqrt(sig2A));
rhoAcol=0;
b1Acol=(sqrt(sigA)/sqrt(sig2A))*
(rhocAcol-rhoAcol*sqrt((1-rhocAcol*rhocAcol)/(1-rhoAcol*rhoAcol)));
b2Acol=b2Acol-b1Acol*g1Acol;
b0Acol=k0A-b1Acol*g0A;
sig1Acol=(rhocAcol*sqrt(sigA)-b1Acol*sqrt(sig2A))/0.5;
```

```
indAcol=b1Acol*g1Acol;
dirAcol=b2Acol;

! section below specifies how to estimate the parameters re: mediation pathway g1*b1
! for Group B

rhocBcol=covB/(sqrt(sigB)*sqrt(sig2B));
rhoBcol=0;
b1Bcol=(sqrt(sigB)/sqrt(sig2B))*
(rhocBcol-rhoBcol*sqrt((1-rhocBcol*rhocBcol)/(1-rhoBcol*rhoBcol)));
b2Bcol=b2Bcol-b1Bcol*g1Bcol;
b0Bcol=0-b1Bcol*0;
sig1Bcol=(rhocBcol*sqrt(sigB)-b1Bcol*sqrt(sig2B))/0.5;
indBcol=b1Bcol*g1Bcol;
dirBcol=b2Bcol;


! section below specifies how to estimate the parameters re: controlled pathway g2*b1
! for Group A


rhocAjs=covA/(sqrt(sigA)*sqrt(sig2A));
rhoAjs=0;
b1Ajs=(sqrt(sigA)/sqrt(sig2A))*
(rhocAjs-rhoAjs*sqrt((1-rhocAjs*rhocAjs)/(1-rhoAjs*rhoAjs)));
b2Ajs=b3Ajs-b1Ajs*g2Ajs;
b0Ajs=k0A-b1Ajs*g0A;
sig1Ajs=(rhocAjs*sqrt(sigA)-b1Ajs*sqrt(sig2A))/0.5;
indAjs=b1Ajs*g2Ajs;
dirAjs=b2Ajs;

! section below specifies how to estimate the parameters re: controlled pathway g2*b1
! for Group B

rhocBjs=covB/(sqrt(sigB)*sqrt(sig2B));
rhoBjs=0;
b1Bjs=(sqrt(sigB)/sqrt(sig2B))*
(rhocBjs-rhoBjs*sqrt((1-rhocBjs*rhocBjs)/(1-rhoBjs*rhoBjs)));
b2Bjs=b3Bjs-b1Bjs*g2Bjs;
b0Bjs=0-b1Bjs*0;
sig1Bjs=(rhocBjs*sqrt(sigB)-b1Bjs*sqrt(sig2B))/0.5;
indBjs=b1Bjs*g2Bjs;
dirBjs=b2Bjs;

! section below specifies how to estimate the parameters re: controlled pathway g3*b1
! for Group A

rhocAxz=covA/(sqrt(sigA)*sqrt(sig2A));
rhoAxz=0;
b1Axz=(sqrt(sigA)/sqrt(sig2A))*
(rhocAxz-rhoAxz*sqrt((1-rhocAxz*rhocAxz)/(1-rhoAxz*rhoAxz)));
b2Axz=b4Axz-b1Axz*g3Axz;
b0Axz=k0A-b1Axz*g0A;
sig1Axz=(rhocAxz*sqrt(sigA)-b1Axz*sqrt(sig2A))/0.5;
indAxz=b1Axz*g3Axz;
dirAxz=b2Axz;

! section below specifies how to estimate the parameters re: controlled pathway g3*b1
! for Group B

rhocBxz=covB/(sqrt(sigB)*sqrt(sig2B));
rhoBxz=0;
b1Bxz=(sqrt(sigB)/sqrt(sig2B))*
```

```
(rhocBxz-rhoBxz*sqrt((1-rhocBxz*rhocBxz)/(1-rhoBxz*rhoBxz)));
b2Bxz=b4Bxz-b1Bxz*g3Bxz;
b0Bxz=0-b1Bxz*0;
sig1Bxz=(rhocBxz*sqrt(sigB)-b1Bxz*sqrt(sig2B))/0.5;
indBxz=b1Bxz*g3Bxz;
dirBxz=b2Bxz;


PLOT:                            !produces the plots of Figure 4
TYPE = PLOT3;

OUTPUT:                          !produces Model 3 coefficients of Table 2
TECH1 TECH8 STAND(STDYX);
```