Story of an algorithme

Bernard & Pierre-Marie GAGEY

In 2010, Bernard has found a new original mathematical solution to calculate the center of gravity from the vertical forces pressure center. I proposed the publication of this algorithm to the journal Medical and Biological Engineering and Computing. After several round trips, review, in 2013, refused to publish this algorithm in the context of a brief technical note because the mathematical solution is so simple that it does not deserve to be published: «the mathematics may not be sophisticated enough to warrant a specific publication.» [1]

In 2013 the journal Frontiers in Bioengineering refused to publish the algorithm on the opinion of one referee who did not understand the mathematical solution proposed by Bernard: "The remaining derivation (Eqs 2-6.) casts as CoF a function of the CoG (or just "G" in the equations). Thus could one, in theory compute CoF knowing CoG then one goal would need an independent measure of CoG (eg, from motion analysis, etc.). "The referee looked simply at the wrong equation, the right one is the equation (Eq. 1)!

In 2014, I proposed to the Journal of Biomechanics to publish this algorithm simply as a technical note. The referees agreed on the value of the mathematical solution "The proposed approach exploits the edge effect when solving differential equations, what is a smart and, apparently, original idea." But the magazine refused publication because I did not explain for what this calculation could be used ... This is another issue, but each journal may have its editorial policy.

En 2015, le Journal de biomécanique de nouveau refusé de publier le document présenté qui contenait les explications demandées. Les arbitres ont une nouvelle fois que «L'approche proposée exploite l'effet de bord lors de la résolution des équations différentielles, Qui est une apparence, idée originale et intelligente,.". Mais ils ne comprennent pas les explications fondées sur un principe épistémologique pourtant simple que les médecins français ont reconnu depuis 1916: l'objectivité d'un syndrome fonctionnelle basée intersubjectivité [2]. L'arbitre a écrit: «Que signifie cette phrase:« intersubjectivité est la base de l'objectivité de ces troubles fonctionnels »? Ce est un bon exemple du manque d'objectivité et de clarté du manuscrit dans son ensemble. "[3]

1. http://ada-posturologie.fr/CoP_CoG_New_Algorithm.pdf
2. Marie P. (1916) Les troubles subjectifs consécutifs aux blessures du crâne. Revue de Neurologie, 4-5: 454-476.
3. See the following texts that include the second grind of the article and the referees' comments to the two submitted grinds.

Original article

Subscription

Why and how to use the Center of Gravity in clinical Stabilometry

Bernard Gagey, Institute of Posturology, Paris

Olivier Bourdeaux, Institute of Posturology, Paris

Pierre-Marie Gagey, Institute of Posturology, Paris,

Dr Pierre-Marie Gagey 240, rue saint-jacques 75005 Paris France

Tel. : (33) 143295448

pmgagey@club-internet.fr

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Summary

Clinicians must be critical when treating patients who suffer from purely functional disturbances of the postural system — without any anatomic lesion of the central nervous system —. Stability proves to practise this criticism, both on the concept itself of stability and the means to objectify it. We must stop basing our stabilometric analysis only on the traditional 'center of pressure' which causes confusion between stability and stabilization. Many methods have been proposed to assess the position of the center of gravity starting from the center of pressure; a new method is presented here. choose one of these methods, its to confront the subject to a reference population must be taken into account. This may result in a choice different from the one made in laboratories studying the movement. Stabilometry is specified by its object, this paper deals with postural clinical stabilometry.

Why

WWI forced neurologists to consider the functional neurological disorders beyond their clinicopathological concept. Not only do they exist (Babinski and Froment, 1918) but some of them have an unquestionable objectivity when observing a cohort of patients who suffer from the same disorders «They all say the same thing, with the same words, we cannot think that they have agreed» (Marie, 1916). Intersubjectivity is the basis of the objectivity of these functional disorders.

But when a clinician observes an isolated functional patient, the criterion of intersubjectivity is blunted and the clinician needs to find other objectivity criteria behind the subjective complaints of his patient. Some neurologists have then tried to use the labyrinthine galvanic stimulation, unsuccessfully (Cestan et al., 1916, b; Cestan et al., 1916, a; Foy, 1919). As soon as they emerged in the clinical field, force platforms have raised hopes (Baron, 1964), hopes that were partially disappointed: the force platforms are unable, even today, to provide proof of the objectivity of the functional disorders presented by a patient, they are hardly able to provide a series of presumptions. This does not mean that platforms are no longer useful, they are useful and even more and more, but differently: they allow those who treat functional patients to validate the efficiency of their treatments and to access to a language whose rigor tries to rely on biomechanics.

This rigor of language has become strictly necessary since the discovery that the postural system operates as a nonlinear dynamic system (Baron, 1955; Cao et al., 1998; Firsov and Rosenbum, 1990; Fukuda, 1957; Gagey et al., 1998; Martinerie and Gagey, 1992; Micheau et al., 2001; Murata and Iwase, 1998; Myklebust et al., 1995; Peng et al., 2002; Peterka, 1999; Sasaki et al., 2001; Sasaki et al., 2006; Sasaki et al., 2002; Shimizu et al., 2002; Thomasson, 1995). The criterion of proportionality between cause and effect having disappeared, any minor event can be assumed to cause remarkable cures, for instance a few millimeters of additional thickness of the inner sole of a shoe positioned under a certain area of the footpad (Ehring and Kurzawa, 2012; Janin, 2007) , for example an optical prism half a diopter in front of one eye in a given position (Baron and Fowler, 1952; Marino and Quercia, 2007), or a tiny chip stuck to the lingual surface of a certain tooth (Marino and Quercia, 2007). Mathematically, we no longer have the right to say that these astonishing effects are impossible, but in practice we still need more proofs than just the subjective complaints of patients. A critical way of thinking had then to develop around these new phenomena, moving, uncertain, even strange in some ways, particularly in their therapeutic implications (Bricot, 1996; Gagey et al., 1980; Gagey and Weber, 1995b, 1997, 2000, 2001, 2008; Vallier, 2012; Willem, 2001). In France, this criticism of Posturology originated inside the «Association Française de Posturologie» (Gagey and Weber, 1995a; Lacour, 1997), then inside the «Association Posture et Équilibre» (Lacour, 1999a, b, 2001, 2004; Lacour and Borel, 2007; Lacour and Defebvre, 2011; Lacour et al., 2003, 2009; Lacour and Hamaoui, 2012; Lacour and Perennou, 2006; Lacour et al., 2012; Lacour and Rougier, 2006; Lacour and Thoumie, 2008; Lacour and Weber, 2005) and the «Association Internationale de Posturologie» (Villeneuve, 1996, 1998; Weber and Villeneuve, 2000, 2003, 2007, 2010, 2012).

This critical reflection was mainly clinical, but not only. In biomechanics, this criticism has tackled first the concept of equilibrium as defined among doctors: «For there to get balance, the line of gravity must simply fall within the base of support» André Thomas said (Thomas, 1940). But in fact, we find that the line of gravity of a «normal» man standing at rest remains «within a surface that is not even a centimeter square» (Toulon, 1956). Thus, it must be acknowledged that the subject can't be said «normal» simply because the line of gravity falls within the base of support. The medical concept of balance is a wrong idea, it does not allow to think about all that can be abnormal in patients complaining of instability, while their vertical of gravity remains inside their base of support. The physical concept of equilibrium shows that the normal man standing upright quiet is never in equilibrium, he is stable, his body returns next to its mean position when it is pushed aside from it. This concept of stability has quickly become the basic biomechanics concept of our studies. With the force platforms, we can measure the accuracy of the stability of a subject through the average deviation of his center of gravity (CoG) from its mean position, evaluate the energy spent by the system to achieve this stability, check if this energy expenditure is consistent with the resulting stability, consider the time constant of this stability.

Our critical think tank then addressed the use of the «Center of Pressure» (CoP) in stabilometry. It is not the projection of the center of mass (CoM)[1] on the plane of the platform (Murray et al., 1967; Thomas and Whitney, 1959). By equating the CoP to the projection of the CoM a mistake is made that can be of importance (Gurfinkel, 1973). The analysis of the stabilometric signal shows that the CoP has two clearly identified parts, either around one Hertz in frequency analysis (Gagey et al., 1985) or around one second in temporal analysis (Collins and Luca, 1993). The CoP signal comprises two series of information, one on the controlled variable, below 1 Hz ± and the other on the controlling variable, at higher frequencies (Winter and Eng, 1995). This signal of the CoP is not suitable for a rigorous study of the stability of the standing man since it mixes up these two types of information, doing so it blurs the information on the position, speed (fig. 1) and acceleration of the CoM. Stability and stabilization are two related phenomena, but different, as already detected by Thomas (Thomas, 1940).

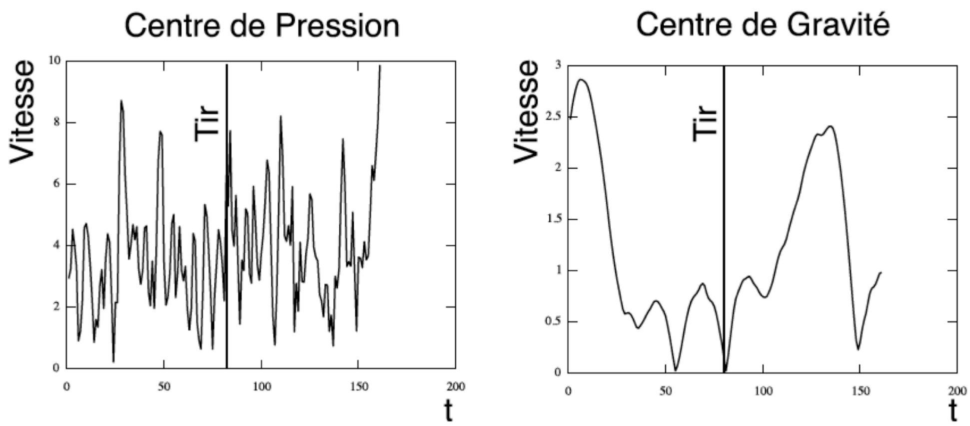


FIG. 1 — Speeds of the COP and of the CoG, from the same stabilometric recording of a rifle shooter, two seconds before and after shooting.

Alone the speed of the CoG shows that the shooter controls the speed of his center of gravity at the moment of firing. (Recording on Cyber-Sabots, sampling frequency 40Hz, in firing position. The CoG has been calculated by the algorithm presented in this article).

[1] Winter suggested to make a clear distinction in our language between the CoM «point of mass equivalent to the total mass of the body in the global reference system» and the CoG «Vertical projection of the CoM on the floor plan.» (Winter & Eng, 1995).

Both these criticisms explain and justifie why the use of the CoG in clinical stabilometry is required.

How

Historical

Borelli (Borelli, 1680) was the first to examine the gravity line of the «human machine» (Descartes, 1664). He situated the position of this gravity line by placing a subject on a platform resting on a knife (Murray et al., 1967). This technique was adopted by Hellebrandt (Hellebrandt, 1938) too and it helped Toulon to notice that the CoM of man is projected onto the plane of his support basis within an area of less than one inch (Toulon, 1956). But the study of the position and movements of the gravity line could not really develop until the force platforms were used.

The first force platforms were built in the mid-twentieth century (Babskii et al., 1955; Hirasawa, 1960; Soula, 1951). The first French platform was built by Lauru and Soula (1953). It measured the forces developed in response to the presence of a subject on the plateau and did so in the three directions of space (PF3D). The force sensors were piezoelectric quartz. With these sensors and electrometers without inertia, he had at hand, Lauru failed to measure the horizontal forces developed by a subject standing still on the platform; «The anteroposterior and lateral traces are silent, the layout is roughly that of a straight line» (Ranquet, 1953). Later, many authors confirmed that their PF3D, commercially available, measured incorrectly the horizontal forces (Caron et al., 1997; Hof, 2007; Karlsson, 1997; Levin and Mizrahi, 1996). Only Lafond (Lafond, 2005) seems satisfied. Since 2013, it is said that now these horizontal forces would be properly measured.

Measuring the CoG with 3D platforms

The news is interesting because these horizontal forces allow us to know immediately the horizontal acceleration of the CoM, in the direction of the force measured, by the application of the Newtonian equation:

$$F = mG'' \quad (1)$$

where F: force measured, m: mass of the subject, G" : horizontal acceleration of the CoM in the direction of the force.

The CoM horizontal accelerations concern the clinicians (Yu et al., 2008), but the position of the CoG and its speed as well. To calculate the position of the CoG from this second order differential equation, theoretically one just has to do a double integration, practically it raises two problems. Winter & Eng (Eng and Winter, 1993) noticed: «The double integration of the ground reaction forces is prone to integration error, especially during steady-state period.» Other authors, like Morasso (Morasso et al., 1999), have confirmed. To these difficulties of the numerical integration is added the fact that the differential equation (1) has an infinite number of solutions because its initial constants are not known.

Various solutions have been proposed to overcome this difficulty. Spaepen and coll. choose the initial data that produce the curve of the CoM movements which is closest to that of the movements of the CoP (Spaepen et al., 1977). Shimba (Shimba, 1984) and Levin and Mizrahi (Levin and Mizrahi, 1996) adjust by the least squares method two curves obtained by integration of two different mechanical equations. Zatsiorky and King (Zatsiorsky and King, 1998) note that if the horizontal force (F_h) in a given direction is zero, then the horizontal acceleration of the CoG in this direction is zero and its velocity is constant; two observations that allow to assess approximately the conditions of position and initial velocity, of the double integral. But it is unusual that sampling instants occur exactly at a time when $F_h = 0$;

generally, these moments must then be estimated. When $F_h = 0$ we know that the speed is constant, but we do not know its value. Assuming it is zero, we will have two constants of integration that allow to achieve a first integral calculus between two successive situations when $F_h = 0$. This calculation provides the measure of speed that allows to start again a correct integral calculus. This calculation technique is then repeated between every moment when $F_h = 0$. This long series of calculations makes the method difficult to use in clinical practice (Yu et al., 2008). However, despite the approximations involved and the problems of numerical integration, an assessment of the method by Lenzi et Coll. (Lenzi et al., 2003) recognizes the advantages of this method over methods using the model of the inverted pendulum.

Measuring the CoG by averaging and filters

It is not necessary to use the horizontal forces in order not to use the model of the inverted pendulum. Nashner (NeuroCom International, 1989) only makes a sliding average between successive sampled positions of the CoP, since the CoP navigates on either side of the CoG. But the results vary according to the size of the sliding window. Benda , Levine and Mizrahi, Brenière, Caron, Hugon propose to apply a low-pass filter to the stabilometric signal, since the CoG movements have lower frequencies than those of the CoP (Benda et al., 1994; Brenière, 1996; Caron et al., 1997; Hugon, 1999; Levine and Mizrahi, 1996). But the results vary according to the cut-off frequency and the type of filter; moreover the phase of the signal is changed. Hof (Hof, 2005) thinks that in some cases it is advisable to use the horizontal forces rather than a low-pass filter.

Measuring the CoG by the model of the inverted pendulum

The methods using the model of the inverted pendulum can rely only on measuring the vertical forces, easy to measure accurately.

Assimilate the human body to a pendulum pivoting around the ankles allows one to write mechanical equations that relate the position of the CoP to the position of CoG through couples and moments acting on this pendulum. The equation proposed by Winter and Eng (Winter and Eng, 1995) is illustrated in figure 2.

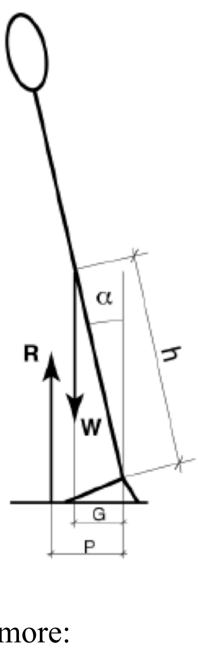


FIG. 2 — Diagram of Winter's mechanical equation

The body weight, W, and the reaction, R, opposed to weight by the platform, are two equal forces, opposite, seldom aligned, acting respectively on the pendulum at the distance G and P from the ankle joint. The resulting moment of the couples WG and RP is equal to the moment of inertia of the pendulum, multiplied by its angular acceleration, α":

$$WG - PR = I\alpha''$$

G expresses the distance of the CoG, and P of the CoP, from the axis of the ankle joint. The oscillations of the human pendulum being of low amplitude at rest, the angle α is not very different from its sinus in these conditions, so the angular acceleration of the pendulum is almost equal to the horizontal acceleration of the center of gravity G" divided by the distance, h, between the axis of the ankle and the center of gravity: furthermore:

$$R = W = mg$$

where m is the mass of the subject, g: the acceleration of gravity.
So the equation can be written as:

$$G - P = \frac{I}{mgh} G''$$

If we write

$$\frac{I}{mgh} = k^2$$

then

$$P = G - k^2 G'' \qquad (2)$$

You need only to solve this differential equation to determine the position of the CoG at every moment of the recording. The problem is that, on the one hand, the integration constants are not known and, on the other hand, numerical integration techniques applied to unstable systems pose problems.

Morasso (Morasso et al., 1999) gets around these problems by using the method of least squares in a standard approach of the approximate solution of overdetermined systems, with the best fitting B-spline functions. This method is of interest because it provides a function that describes the position of the CoG in relation to time. However, the error introduced by the series of approximations is unknown; moreover the anthropometric variations of the subjects being recorded does not interest Morasso.

Jacono (Jacono et al., 2004) uses the same mechanical equation of the inverted pendulum. From the Fourier transform of both sides of this equation, we can write the transfer function of a filter that lets you know the time evolution of the position of the CoG from the sampled positions of the CoP. The error of the curve obtained by this filter comes from the inverted pendulum model and is not estimated; secondly the filtering operation does not take into account the anthropometric data of the subject.

Barbier (Barbier et al., 2003), generalizing the Karlsson's method (Karlsson, 1997) to the three dimensions offers two smart solutions to eliminate the second derivatives of his mechanical equations by application of Newton's second law and the theorem of angular momentum. His solutions take into account the size of the subject.

Proposed method

Equation (2) can be expressed best at any moment, j, of measurement by a linear equation, replacing the second derivative with a finite difference approximation.

$$P = G_j - k^2 \frac{G_{j-1} + G_{j+1} - 2G_j}{\delta t^2}$$

For j from 1 to n, we can write a system of n linear equations with n unknowns, having a solution, Gj, and only one, assuming G_0 and G_{n+1} are zero.

Moreover the equation (2) has an infinite number of solutions, γ_i, but the difference between any two of these solutions for the same P_j is singular. Indeed, suppose «d» is the name of the difference between two solutions, γ_1 and γ_2, of the equation:

$$\gamma_1 - \gamma_2 = d$$

This difference can also be written:

$$P_j - P_j = \gamma_1 - \gamma_2 - k^2\left(\gamma_1'' - \gamma_2''\right)$$

or, by replacing γ_1 -γ_2 by d:

$$d = k^2 d''$$

All the solutions of this equation can be expressed by a two parameters function:

$$d_{a,b} = ae^{-qt} + be^{-q(F-t)}$$

t represents time, F: the value of t at n + 1, q = 1 / k

If we choose a function of $D = d_{a1,\, b1}$, with a_1 and b_1 such that $G_0 = D_0$ and $D_{n+1} = G_{n+1}$, then the solution we are looking for is G + D.

But we know neither G_0 nor G_{n+1}, so no more a_1 and b_1. However, it is known that G_0 and G_{n+1} are in the support basis, that a_1 and b_1 are almost equal to G_0 and G_{n+1}; based on this fact, if you look at the curve D (Figure 3 & 4), we can see that, apart from a few seconds at the beginning and end of the measurement interval, the function D is practically zero, whatever the values G_0 and G_{n+1}, bounded by the support basis.

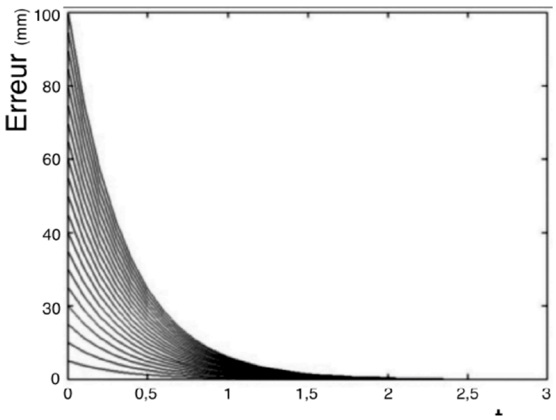
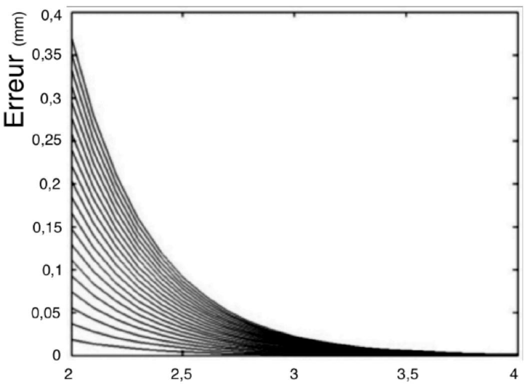


FIG. 3 — Evolution over time, from zero to three seconds, of the error introduced in the calculation by the arbitrary selection of G_o and G_{n+1}.

FIG. 4 — Zoom on the precedent figure.



This means that, apart from a few seconds at the beginning and end of the measurement interval, the resolution of the equation system gives the value of G pretty well.

Finally, the G research is therefore to build and solve the linear system of n equations — what, by now, a PC achieves in a few seconds — and to eliminate a few seconds of the measurement terminals.

The coefficient k^2 and anthropometry

$$k^2 = \frac{I}{mgh}$$

The coefficient k^2 involves specific anthropometric data for each subject under recording: moment of inertia of the body relative to the axis of the ankle (I), body mass (m), height of the CoM above the axis of the ankle joint (h). In clinical practice it is unthinkable to evaluate these data for each subject, or even to look for them in the anthropometric data tables, such as Winter's (Winter, 2009) for example. Given the importance of the coefficient k^2 in the context of standardized clinical stabilometry, the choice of the k^2 values should be the result of a consensus among clinicians, based on fundamental studies.

Discussion

The peculiarity of the proposed method is to take advantage of the edge effects when solving a differential equation the variable of which is bounded, which gives it a real simplicity. But we must aadmit that the method is based, like many other methods, on the model of the inverted pendulum, which is far from perfect. In addition to its significant reduction of the degrees of freedom of the human pendulum, this model confuses the axis of the ankle joint and the Henke's axis. The angular deflections of the body axis are assumed to be minimal, corresponding only to the case of static stabilometry. It reckons that the feet specific muscles are not involved in controlling the position of the CoP, which is an error (Tortolero et al., 2007). It would be advisable to replace this model by a better model, the model of the broom for example (Gagey et al., 2003; Roberts., 1995). But it is not formalized biomechanically yet.

Among the methods that do not use the model of the inverted pendulum, the King-Zatsiorsky was recognized particularly interesting. But no anthropometric data appear in its mechanical equations. Now, faced with his functional patient who brings him only subjective complaints, the clinician needs «something» reminding him of the intersubjectivity of the symptoms, basis of the objectivity of the syndrome. Stabilometry provides this «something» because it allows to compare the performance of the patient to a database... provided the database takes into account the anthropometric characteristics of individuals. That is why it is better to use a method based on the inverted pendulum, which remains «an acceptable model» (Gage et al., 2004; Winter et al., 1997).

Among the methods using the model of the inverted pendulum, there is no compelling reason to choose the method presented in this paper rather than the methods of Morasso, Jacono or Barbier. However, we believe all of us must decide to use the same method with a view to standardizing clinical stabilometry. It is not clear, in fact, that the differences between the methods, however small, will not have an impact on the calculation of certain stabilometric parameters.

Some additional reasons could guide the imperative choice of a single method toward the one presented in this article. It has been used for several years to study the marksman's posture and it showed, for example, the obvious interest to compare the movements of the CoG with the movements of the weapon (Dudde et al., 2014; Dudde et al., 2012; Gagey et al., 2014; Gagey et al., 2013). It is already used to build a database of reference values of the stabilometric parameters. It first appeared within a group that has been practising stabilometric standards for 30 years (AFP, 1985; Bizzo et al., 1985). All these arguments are worth quoting, they can guide the selection, but the other three methods allow as well to work with the CoG in order to improve the analysis of the stability, its accuracy, its cost, its speed, its tonic background.

The statistical measure of the average deviation of the CoG from its mean position, by a 90% confidence ellipse for example (Takagi et al., 1985), corresponds exactly to the definition of the accuracy of this stability, once accepteded the CoG may represent the body. While the average deviation of the CoP from its mean position certainly gives an idea of the accuracy of the stability of the subject but, also and above all, it shows the importance of the movements of his CoP in order to maintain his stability.

This difference between the CoP and the CoG is designated by the model of the inverted pendulum as the the expression of the acceleration of the CoM in connection with the puffs of phasic muscular contractions that control the accuracy of stability. The cost of this accuracy can be assessed by the extent of the acceleration of the CoM.

The relationship between the results and the means, between the accuracy of the stability, and its cost was already possible by the ratio of the area of the ellipse that contains 90% of the sampled positions of the CoP, to the length of the displacements of the CoP (AFP, 1985; Gagey, 1986; Imaoka et al., 1997). But this comparison is more stringent between, the accuracy of the stability and the acceleration of the CoG (Gély, 2014).

As Morasso (Jacono et al., 2004) has noted, the frequency of the puffs of phasic muscular contractions is significant in relation to the time management of stability. The acceleration of the CoM is designated as a basis to calculate the time constant of the upright postural control system (Dudde et al., 2012; Gagey et al., 2012a; Gagey et al., 2012b).

The stiffness of the tissues of the posterior lodges of the legs is not enough to stabilize the body of a man (Morasso and Schieppati, 1999; Winter et al., 1998), to compensate for the inadequacy of this stiffness, muscle contractions are required (Loram et al., 2005) and the importance of these contractions changes depending on the forwards inclination of the body that changes the stiffness (Gagey and Gentaz, 1993). The acceleration of the CoM allows to evaluate the basic stiffness of the subject, provided that the inclination of the body is taken into account (Dudde et al., 2012).

Conclusion

The therapists who use the properties of nonlinear dynamic systems to treat functional disorders of the upright postural control system need to criticize, their work and the allegations of their patients. The concept of stability and its measurement are currently the instrumental basis for this criticism. The measure of stability in all its aspects, is improved if the analysis of the signal is achieved from CoG and not from CoP, because the CoP confuses stability and stabilization. Among the methods for evaluating the CoG from measurements of a force platform, we must focus on those reflecting the anthropometric data of the subject in order to normalize the stabilometric parameters as much as possible. Among the presented methods that meet this criterion, only one method must be chosen, this choice is not ours.

References

AFP, 1985. Normes 85. ADAP, Paris 20 rue du rendez-vous 75012.

Babinski, J., Froment, J., 1918. Hystérie-Pithiatisme & troubles nerveux d'ordre réflexe en Neurologie de guerre, 2nd ed. Masson, Paris.

Babskii, E., Gurfinkel, V., Romel, E., Jakobson, I., 1955. [Nouvelle méthode d'examen de la stabilité de l'homme en station debout]. Fiziol Zh SSSR Im I M Sechenova 41, 423-427.

Barbier, F., Allard, P., Guelton, K., Colobert, B., Godillon-Maquinghen, A., 2003. Estimation of the 3-D Center of Mass excursion from Force-Plate data during standing. IEEE Trans Neural Syst Rehabil Eng 11, 31-37.

Baron, J., 1955. Muscles moteurs oculaires, attitude et comportement locomoteur des vertébrés. Thesis of Science, Paris.

Baron, J., 1964. Présentation d'un appareil pour mettre en évidence les déplacements du centre de gravité dans le polygone de sustentation. Arch. Mal. Prof. 25, 41-49.

Baron, J., Fowler, E., 1952. Prismatic lenses for vertigo and some experimental background of the role of the extrinsic ocular muscles in desequilibrium Trans. Am. Acad. Ophthal. Oto-laryngol. 56, 916-926.

Benda, B., Riley, P., Krebs, D., 1994. Biomechanical relationship between center of gravity and center of pressure during standing. IEEE Transactions on Rehabilitation Engineering 2, 3-10.

Bizzo, G., Guillet, N., Patat, A., Gagey, P., 1985. Specifications for building a vertical force platform designed for clinical stabilometry. Med Biol Eng Comput 23, 474-476.

Borelli, G., 1680. De motu animalium. Bernado, Roma.

Brenière, Y., 1996. Why we walk the way we do. J Motor Behavior 28, 291-298.

Bricot, B., 1996. La reprogrammation posturale globale. Sauramps, Montpellier.

Cao, L., KIim, B., Kurths, J., Kim, S., 1998. Detecting determinism in human posture control data. Int. J. Bifurcation Chaos 8, 179-188.

Caron, J., Faure, B., Brenière, Y., 1997. Estimating the centre of gravity of the body on the basis of the centre of pressure in standing posture. j Biomech 11-12, 1169-1171.

Cestan, R., Descomps, P., Euzière, J., Sauvage, R., 1916, b. La marche sous courant galvanique chez les commotionnés et les traumatisés du crâne. Bulletin de la Société médicale des Hôpitaux de Paris 40, 1730-1737. .

Cestan, R., Descomps, P., Sauvage, R., 1916, a. Les troubles de l'équilibre dans les traumatismes craniens. Paris-médical: la semaine du clinicien 19, 523-525.

Collins, J., Luca, C.d., 1993. Open-loop and closed-loop control of posture: a random-walk analysis of center-of-pressure trajectories. Exp. Brain Res 95, 308-318.

Descartes, R., 1664. Traité de l'homme, Première ed. Angot, Paris.

Dudde, R., Müller, G., Bourdeaux, O., Gagey, B., Weber, B., Gagey, P., 2014. Dialogue médecins/entraîneurs autour d'une plateforme de forces. Sci Sports.

Dudde, R., Muller, G., Bourdeaux, O., Weber, B., Gagey, B., Gagey, P., 2012. The Marksman's motor control. I) Stabilometric study of the changes during training, p. Original work.

Ehring, C., Kurzawa, S., 2012. Diminuer la douleur et corriger les dysfonctions rachidiennes des lombalgies chroniques, in: Villeneuve, P., Weber, B. (Eds.), Posturologie clinique. Comprendre, évaluer, soulager les douleurs. Elsevier-Masson, Paris, pp. 108-113.

Eng, J., Winter, D., 1993. Estimations of the horizontal displacement of the total body centre of mass: considerations during standing activities. Gait Posture1: 141—144 1, 141—144.

Firsov, V., Rosenbum, M., Year Chaotice dynamics of the upright human posture control In First World Congress on Biomechanics. La Jolla (CA).

Foy, R., Year Examen du labyrinthe vestibulaire par les épreuves thermiques (air froid) et par le courant galvanique. In XXXe congrès français d'ORL. Paris.

Fukuda, T., 1957. Undô to Heikô no hansha seiri. Igaku Shoin, Tokyo.

Gage, W., Winter, D., Frank, J., Adkin, A., 2004. Kinematic and kinetic validity of the inverted pendulum model in quiet standing. Gait Posture 19, 124–132.

Gagey, P., 1986. Postural disorders among workers on building sites, in: Bles, W., Brandt, T. (Eds.), Disorders of Posture and Gait. Elsevier, Amsterdam, pp. 253-268.

Gagey, P., B, G., Ouaknine2012) From the center of pressure to the center of gravity through a simple calculation. ISPGR congress, -.J., Trondheim., Year From the center of pressure to the center of gravity through a simple calculation. In ISPGR congress. Trondheim.

Gagey, P., Baron, J., Ushio, N., 1980. Introduction à la posturologie clinique. Agressologie 21, 119-124.

Gagey, P., Bizzo, G., Debruille, O., Lacroix, D., 1985. The one Hertz phenomenon, in: Igarashi M, B.F. (Ed.), Vestibular and visual control on posture and locomotor equilibrium. Karger, Basel, pp. 89-92.

Gagey, P., Bizzo, G., Ouaknine, M., Weber, B., 2003. Deux modèles mécaniques de stabilisation posturale: la Tactique du Centre de Gravité et la Tactique du Centre de Pression.

Gagey, P., Dudde, R., Gagey, B., Weber, B., Bourdeau, O., 2014. Comment le tireur décide de l'instant du tir. Neurophysiologie clinique 44.

Gagey, P., Dudde, R., Weber, B., Leblanc, V., Villeneuve, P., Villeneuve, S., 2013. Contrôle postural et entraînement au tir à la carabine.

Gagey, P., Gagey, B., Ouaknine, M., Year From the center of pressure to the center of gravity through a simple calculation. In ISPGR congress. Trondheim.

Gagey, P., Gentaz, R., 1993. Le paramètre "VFY" en stabilométrie. Agressologie 34, 183-185.

Gagey, P., Martinerie, J., Pezard, L., Benaim, C., 1998. L'équilibre statique est contrôlé par un système dynamique non-linéaire. Ann. Oto-Laryngol., 115, 161-168.

Gagey, P., Weber, B., 1995a. Entrées du système postural fin. Masson, Paris.

Gagey, P., Weber, B., 1995b. Posturologie Régulation et dérèglements de la station debout. Masson, Paris.

Gagey, P., Weber, B., 1997. Posturologia, Regolazione e perturbazioni della stazione eretta. Marrapese, Roma.

Gagey, P., Weber, B., 2000. Posturología. Regulação e distúrbios da posição ortostática. segunda edição. Manole, São Paulo.

Gagey, P., Weber, B., 2001. Posturologia. Regulación y alterciones de la bipedestación. Masson, Barcelona.

Gagey, P., Weber, B., 2008. [Posturologie Régulation et dérèglements de la station debout]. Publishing House SPbMAPO, Saint-Pétersbourg.

Gély, A., 2014. Quels sont les paramètres subjectifs ou objectifs qui sont prédictifs d'une performance sur une course d'endurance telle que The North Face Ultra Trail du Mont Blanc® ? Marseille University.

Gurfinkel, V., 1973. Physical foundations of stabilography. Agressologie 14, 9-14.

Hellebrandt, F., 1938. Standing as a geotropic reflex. Am. J. Physiol 121, 471-473.

Hirasawa, Y., 1960. [A study on the stability of Human standing posture, Relation to the contact surface of a foot sole.] Mie Igaku 4,, 2241-2257.

Hof, A., 2005. Letter to the EditorJ. Biomechanics 38m2134-2135. J Biomech 38, 2134-2135.

Hof, A., 2007. The equations of motion for a standing human reveal three mechanisms for balance. J Biomech 40, 451–457.

Hugon, M., 1999. Du centre de pressions au centre de gravité en stabilographie statique, in: Lacour, M. (Ed.), Posture et Equilibre : Entrées sensorielles, méthodes d'exploration et applications. Sauramps, Montpellier, pp. 89-106.

Imaoka, K., Murase, H., Fukuhara, M., 1997. [Collection of data for healthy subjects in stabilometry]. Equilibrium Research Supp. 12, 1-84.

Jacono, M., Casadio, M., Morasso, P., Sanguineti, V., 2004. The Sway-Density Curve and the Underlying Postural Stabilization Process. Motor Control 8, 292-311.

Janin, M., 2007. Effet d'une stimulation de la voute plantaire par un élément médio-interne de 3mm, in: Borel, L., Lacour, M. (Eds.), Contrôle postural et représentations spatiales de la neurobiologie à la clinique. Solal, Marseille, pp. 305-312.

Karlsson, A., 1997. Analysis of postural sway strategies using an inverted pendulum model and force plate data. Gait Posture, 198–203.

Lacour, M., 1997. Posture & Équilibre, Pathologies, vieillissement, Stratégies, Modélisation. Sauramps, Montpellier.

Lacour, M., 1999a. Posture & Équilibre, Aspects développementaux, méthodologiques, perceptifs et cliniques. Sauramps, Montpellier.

Lacour, M., 1999b. Posture & Équilibre, Entrées sensorielles, Méthodes d'exploration et applications. Sauramps, Montpellier.

Lacour, M., 2001. Posture & Équilibre, Nouveautés 2001, conceptuelles, instrumentales et cliniques. Solal, Marseille.

Lacour, M., 2004. Nouvelles Méthodes de traitement du signal posturographique, applications en recherche fondamentale et clinique. Solal, Marseille.

Lacour, M., Borel, L., 2007. Contrôle postural et représentations spatiales. De la neurobiologie à la clinique. Solal, Marseille.

Lacour, M., Defebvre, L., 2011. Posture et Locomotion. Solal, Marseille.

Lacour, M., Dupui, P., Montoya, R., 2003. Posture & Équilibre, Physiologie, Techniques, Pathologies. Solal, Marseille.

Lacour, M., Dupui, P., Montoya, R., 2009. Posture, exercice physique, vieillissement et pathologies. Solal, Marseille.

Lacour, M., Hamaoui, A., 2012. Du contrôle postural à l'exécution du mouvement. de Boeck Solal, Paris.

Lacour, M., Perennou, D., 2006. Efficience et déficiences du contrôle postural. Solal, Marseille.

Lacour, M., Pérennou, D., Nougier, V., Honoré, J., 2012. Contrôle postural, espace, locomotion. de Boeck Solal, Paris.

Lacour, M., Rougier, P., 2006. De Marey à nos jours, un siècle de recherches sur la posture et le mouvement. Solal, Marseille.

Lacour, M., Thoumie, P., 2008. De la recherche à la pratique clinique. Solal, Marseille.

Lacour, M., Weber, B., 2005. Bipédie, contrôle postural et représentation corticale. Solal, Marseille.

Lafond, D., 2005. Correspondence. J Biomech 38, 1737–1740.

Lauru, L., Soula, C., 1953, Méthode de mesure du travail. In XIXe Congrès International de Physiologie. Montréal.

Lenzi, D., Cappello, A., Chiari, L., 2003. Influence of body segment parameters and modeling assumptions on the estimate of center of mass trajectory. J Biomech 36, 1335-1341.

Levin, O., Mizrahi, J., 1996. An iterative model for estimation of the trajectory of center of gravity from bilateral reactive force measurements in standing sway. Gait and Posture 4, 89-99.

Loram, L., Maganaris, C., Lakie, M., 2005. Human postural sway results from frequent, ballistic bias impulses by soleus and gastrocnemius. J Physiol 564.

Marie, P., 1916. Les troubles subjectifs consécutifs aux blessures du crâne. Revue de Neurologie 4, 454-476.

Marino, A., Quercia, P., 2007. Stimulations trigéminales bipolaires : vers une orthodontie neuro-sensorielle au cours de la dyslexie de développement, in: B.Weber, Villeneuve, P. (Eds.), Posturologie clinique. Dysfonctions motrices et cognitives. Masson, Paris, pp. 76-81.

Martinerie, J., Gagey, P., 1992. Chaotic analysis of the stabilometric signal, in: M Woollacott, H.F. (Ed.), Posture and gait: control mechanisms. University of Oregon Books, Portland, pp. 404-407.

Micheau, P., Kron, A., Bourassa, P., Year Analysis of human postural stability based on the inverted pendulum model with time-delay in feedback. In american control conference.

Morasso, P., Schieppati, M., 1999. Can muscle stiffness alone stabilize upright standing? J. Neurophysiol 82, 1622-1626.

Morasso, P., Spada, G., Capra, R., 1999. Computing the COM from the COP in postural sway movements. Human Movement Science 18, 759-767.

Murata, A., Iwase, H., Year Chaotic analysis of body sway. In 20th annual international conference of the ieee engineering in medicine and biology society.

Murray, M., Seireg, A., RC Scholz, 1967. Center of gravity, center of pressure, and supportive forces during human activities. J. Appl. Physiol 23, 831-838.

Myklebust, J., Prieto, T., Myklebust, B., 1995. Évaluation of non linear dynamics in postural steadiness time series. Ann. Biomed. Engin., 711-719.

NeuroCom International, I., 1989. Equitest System Operator's Manual. NeuroCom.

Peng, C., Mietus, J., Liu, Y., Lee, C., Hausdorff, J., Stanley, H., Goldberger, A., Lipsitz, L., 2002. Quantifying fractal dynamics of human respiration: Age and gender effects. Annals of biomedical engineering 30, 683-692.

Peterka, R., 1999. Quiet stance centre of pressure predicted by a simple feedback model of human postural control. Gait and Posture 9.

Ranquet, J., 1953. Essai d'objectivation de l'équilibre normal et pathologique. Paris.

Roberts., T., 1995. Understanding BalanceChapman & Hall, Glasgow. Chapman & Hall, Glasgow.

Sasaki, O., Gagey, P., Ouaknine, M., Martinerie, J., Quyen, M.V., Toupet, M., L'Heritier, A., 2001. Nonlinear analysis of orthostatic posture in patients with vertigo or balance disorders. Neuroscience Research 41, 185-192.

Sasaki, O., Gagey, P., Usami, S., Sakura, S., 2006. Vertiginous Attacks in Ménières's Disease can be anticipated by Nonlinear Analysis of Posturography. Equilibrium Research 65, 35-47.

Sasaki, O., Usami, S., Gagey, P., Martinerie, J., Quyen, M.L.V., Arranz, P., 2002. Role of visual input in nonlinear postural control system. Ex. Brain Res 147, 1–7.

Shimba, T., 1984. An estimation of center of gravity from force platform data. j Biomech 17, 53-60.

Shimizu, Y., Thurner, N., Ehrenberger, K., 2002. Multifractal spectra as a measure of complexity in human posture. Fractals-complex geometry patterns and scaling in nature and society 10, 103-116.

Soula, C., 1951. Analyse physiologique du mouvement volontaire. Presse médicale 59, 1173-1174.

Spaepen, A., Vranken, M., Willems, E., 1977. Comparison of the movements od the center of gravity and of the center of pressure in stabilometric studies. Agressologie 18, 109-113.

Takagi, A., Fujimura, E., Suehiro, S., 1985. A new method of statokinesigram area measurement. Application of a statistically calculated ellipse., in: Igarashi, M., Black, F. (Eds.), Vestibular and visual control of posture and locomotor equilibrium. Karger, .Basel, pp. 74-79.

Thomas, A., 1940. Équilibre et équilibration. Masson, Paris.

Thomas, D., Whitney, R., 1959. Postural movements during normal standing in man. J. Anat. 93, 524-539.

Thomasson, N., 1995. Traitement du signal stabilométrique par les techniques d'analyses non linéaires. LENA Salpêtrière, Paris.

Tortolero, X., Masani, K., Maluly, C., Popovic, M., 2007. Body movement induced by electrical stimulation of toe muscles during standing. Artif Organs 32, 5-12.

Toulon, R., 1956. Équilibration humaine et évaluation de la posture debout. Maloine, Paris.

Vallier, G., 2012. Traité de posturologie clinique et thérapeutique. Posturopole, Perpignan.

Villeneuve, P., 1996. Pied, équilibre et rachis. Frison-Roche, Paris,.

Villeneuve, P., 1998. Pied, équilibre et posture. Frison-Roche, Paris.

Weber, B., Villeneuve, P., 2000. Pied, équilibre et mouvement. Masson, Paris.

Weber, B., Villeneuve, P., 2003. Pied, équilibre et traitements posturaux. Masson Paris.

Weber, B., Villeneuve, P., 2007. Dysfonctions motrices et cognitives. Posturologie clinique. Masson, Paris.

Weber, B., Villeneuve, P., 2010. Tonus, Posture et attitudes. Posturologie clinique. Masson, Paris.

Weber, B., Villeneuve, P., 2012 Comprendre, évaluer, soulager les douleurs. Posturologie clinique. Masson, Paris.

Willem, G., 2001. Manuel De Posturologie. Frison-Roche, Paris.

Winter, D., 2009. Biomechanics and Motor Control of Human Movement. John Wiley & Sons, Toronto.

Winter, D., Eng, P., 1995. Human balance and posture control during standing and walking. Gait & Posture 3, 193-214.

Winter, D., Patla, A., Prince, F., MIshac, M., Gielo-Perczak, K., 1998. Stiffness control of balance in quiet standing. J. Neurophysiol 80, 1211-1221.

Winter, D., Prince, F., Patla, A., 1997. Validity of the invertum pendulum model of balance in quiet standing. Gait and Posture 5, 153-154.

Yu, E., Masaki, A., Masani, K., Kawashima, N., Eto, F., Haga, N., Nakazawa, N., 2008. Evaluation of postural control in quiet standing using center of mass acceleration: Comparison among the young the lelderly and People with stroke. Arch Phys Med Rehabil 89, 1133-1139.

Zatsiorsky, V., King, D., 1998. An algorithm for determining gravity line location from posturographic recordings. J. Biomechanics 31, 161–164.

Referees comments:

Reviewer #1:

The paper presents a thorough overview of the history of stabilometry/posturography using force plates, and presents the authors' own method for making the computations of Center of Gravity based on measurements of Center of Pressure. It presents a characterization of the decay in error in CoG position that results from inaccurate initial conditions. It advocates for standardization in stabilometry and posturography, as well as for adoption of the authors' current technique.

However, the method described here is described incompletely, so that it would be very hard to implement in practice. It is unclear which equations are background development, and which are part of the actual computation.

Furthermore, the final recommendation is to use either this method or any of three others (Morasso, Jacono, or Barbier), which are claimed to be all equivalent. Without diving into the fine details, the description given here suggests that all those methods also sound equivalent to Hof et al (2005), which is by far the simplest method to implement (given as 7 lines of MATLAB code in a letter to the editor, J. Biomechanics 38: 2134-5). So, this appears to be an addition to the methodology, that is not different from other existing methods.

The quantification of edge effects is worth knowing, but it is also a trivial result once the solution to the equation is examined: namely, that k is a time constant describing the rate of decay of the system's history. This is equivalent to acknowledging that applying a filter to a data set will create edge effects, which will obey the filter dynamics, so that the data should only be used at times away from the edges.

Specific comments below:

In line 231, there is discussion of "taking advantage of edge effects", but the only advantage I see is that they are quantified; they are certainly not used to improve the prediction of CoG.
242-50: It is not explained why motion of the COG should require anthropometric data from the subject. If a method allows you to determine CoG motion without relying on anthropometric data, that's a more robust method. There is no reason not to use anthropometric data to normalize or to clinically evaluate the importance of the results (e.g., X cm of CoG displacement means something different for a tall vs a short person). But the method without the anthropometric data is actually more robust in producing the CoG motion estimate.

255: "It is not clear, in fact, that the differences between the methods, however small, will not have an impact on the calculation of certain
stabilometric parameters."

This is a valid question, and a study evaluating the different methods would be worthwhile.

Reviewer #2:

General Comments

The manuscript proposes an approach to estimate the CoG trajectory from the CoP measurements on the basis of an inverted-pendulum model. The proposed approach exploits the

edge effect when solving differential equations, which is a smart and, apparently, original idea. However, the manuscript suffers from several weaknesses. First, authors do not offer any assessment of the errors in CoG estimation due to model simplifications and assumptions on the anthropometric parameters. Also, there is no evidence this method is superior in any aspect to others available, which is admitted by the authors themselves. This alone raises the question on the contribution of the manuscript but these are not the only problems. It cannot be said the manuscript is well written and organized. Many sentences and paragraphs are confusing and the Why section, particularly, is very difficult to read and quite irritating as evidences in my specific comments. I also consider the manuscripts' title a
little pretentious considering paper's content. Particularly, the why question on the title is not well addressed. Authors fail in explaining the importance of estimating the CoG in opposition to the CoP. Finally, the lack of rigor in the treatment of the model and some concepts in mechanics is of concern.

Specific Comments
- The title appears a little inadequate and pretentious considering the content of the presented manuscript. Particularly, the why question is only partially addressed.
- Summary needs improvement- some sentences sound odd such as "Stability proves to practice this criticism … "
- Line 2 - What does WWI mean?
- Line 6 - What does this sentence mean "Intersubjectivity is the basis of the objectivity of these functional disorders"? This is a good example of the lack of clarity and objectivity of the manuscript as a whole. The writing style is really difficult.
- Second paragraph - I could not understand the point here. First, authors mention that force plates cannot provide "proof of the objectivity of the functional disorders" (whatever this means). Then, they proceed to say that force platforms are more and more important because they allow "to validate the efficiency of their treatments". It makes no sense.
- Third paragraph - again this paragraph does not make sense. What does the nonlinearity of the system has to do with the topic of this manuscript? By the way, authors should know nature is nonlinear and it is by no means surprising that the phenomena involved in postural stability are nonlinear. What about tiny chips and optical prims?
- The sentence spanning lines 30 to 32 is again nonsense, I am obliged to say.
- Lines 64 and 65 - a frequency of 1Hz corresponds to a 1 second-period. So, these are not different things as the text may imply. This needs rephrasing.
- Lines 67 to 71 - it is not clear why the CoP should not be used to investigate stability. Authors mention there is a

difference between stability and stabilization, but they do not offer any explanation. This is necessary as authors' point seem to be that the difference between these two concepts explain the importance of assessing the CoM as opposed to the CoP only. So, the Why section does clearly explain why assessment of CoG is important. Considering the title of the manuscript, this is not acceptable. An appropriate explanation must be offered in the manuscript to justify the effort required to estimate the CoG and the approach proposed by the authors.

- Line 88 - what does it mean a sensor without inertia?
- Line 109 - use initial conditions instead of initial constants.
- Line 145 - couple and moment are the same thing.
- Line 145 - the model, not the equation proposed by Winter and Eng.
- The List of References have several errors.
- Line 148 - In general, R and W are not equal forces. These forces are approximately equal for certain conditions such as slow, small oscillations. By the way, the lack of rigor in the treatment of the inverted-pendulum model is recurrent and somewhat irritating. Authors simply ignore approximations such as disregarding centrifugal forces, or the moment of the horizontal forces due to the fact that the ankle is not on the ground, or the fact that the body is articulated, e.g. at the hip and knee.
- Eq. at line 193 - P should read Pj.
- Line 212 - Figs. 3 and 4 do not show the "few seconds" at the end of the measurement interval.
- Is Fig. 4 a zoom out of Fig. 3? This should be clearly stated.
- Line 219 - authors mention the necessity of eliminating a few seconds of the measurement initial and final intervals. How many seconds would be recommended? It is desirable authors elaborate on this.
- Line 225 to 227 - I do not see why it is unthinkable to look at anthropometric tables for estimates of m, h and I. These values can be easily determined using simple relationships from subject´s weight and height, which would be readily available in a clinical framework. An important question, not addressed by the authors, is the effect of inaccurate estimates of k on the CoG position estimates. This is important if the method is meant to be of any use. Accurate estimations of k may, indeed, be more difficult to obtain in a clinical framework and this could compromise the viability of the approach in case it requires accurate estimates of k. Authors should elaborate on that.
- Lines 253 to 256 - I completely disagree with authors' opinion here. I don't think all must elect a single method in favor of standardization, no matter the errors in estimations. A much better strategy, in my opinion, is developing methods that guarantee small errors in estimations of the G position.

If methods are developed whose estimation errors are sufficiently small, then one could choose any of them. The authors of this manuscript could start by assessing objectively the estimation errors expected by using their proposed approach, which is unfortunately not done.
- Lines 270 to 272 - confusing sentence.
- Discussion section - the last paragraphs contain a collection of facts and studies on stability with no direct connection to the approaches used to estimate the CoG which is the focus of the manuscript. Authors should be more objective.

Minor Corrections
- Line 13 - "hopes" appears twice.
- Number all equations in the manuscript.
- Line 233 - correct aadmit
- Line 269 - correct accepteded

Referees comments on the first submission of the algorithm

Reviewers' comments:

Reviewer #1:

General comments:
The authors try to bring a mathematical tool to determine the CoG position from the CoP position. The manuscript is not easy to read, particularly in the mathematical equations section. One could wonder how clinicians can use this "simple" tool. Finally, this short communication did not answer to the main question: is the CoP postion is the same/different (?) of the CoG, which was the crucial question raised by the authors in their introduction.
The authors should develop why the knowledge of CoG position is important, and / or why only knowledge of CoP is incomplete (for what?).
In my opinion, trying to relate the time course of CoP and CoG is a difficult objective and requires specific conditions. An extreme example: the CoP displacement of a subject standing on a narrow beam is very "small" while the CoG would show huge oscillations.

Specific questions: How was the CoP trace obtained as shown in Fig. 3. Does it belong to a subject standing

"quietly" on a the force platform ? or is it a pure simulation?
CoG is in 3D and CoP in 2D, so the authors should clarify their definition of CoG used in the manuscript, is it the vertical projection on the plane of CoP ?

Reviewer #2:

General Comments

The manuscript proposes a method to estimate the position of the center of gravity (CoG) of the body in balance experiments from measurements of the center of pressure (CoP) position using a vertical force plate. The approach relies on an inverted pendulum model of the body and on the solution of a differential equation resulting from an approximate relationship between the position of the CoP and the second time derivative of the CoG position.

The proposed idea is interesting and original according to authors and to my knowledge. However, the manuscript suffers from important and fundamental weaknesses which are discussed in my specific comments below. The discussion and conclusion sections are poorly written and the methods sections are confusing and difficult to understand. The literature review is incomplete and does not provide a good picture of the state of the art, neither does it clarify the advantages of the proposed method compared to existing ones.

Furthermore, while authors claim this is the "best" method from a "metrological point of view as it provides its limits of uncertainty", many approximations which will cause estimation errors are not even mentioned in the manuscript as discussed in my specific comments. The model itself, which considers the body as an inverted pendulum moving only in the sagittal plane, is obviously a very limited representation of the body and neglects knee bending and hip flexion, motion in other planes, and ankle joint height with respect to the floor. Also, the resulting differential equation is valid only for small angular displacements and neglects, for instance, centrifugal forces. Al these simplifications will cause errors in the CoG estimations but are not even mentioned in the manuscript, which I consider unacceptable.

Another important weakness of the manuscript is not providing sufficient test data to support the correctness and accuracy of the proposed approach compared to other established methods or gold standards. The few comments on the accuracy of the method are vague and there is no description of the data or reference experiment(s) used to draw the presented conclusions. A new approach or method should be accompanied by a solid demonstration of its power and accuracy using typical measurement data , but this is completely absent in the submitted manuscript.

In summary, although I consider the proposed idea interesting and original, the manuscript suffers from important and fundamental weaknesses and is, in my opinion, unacceptable in its current form.

Specific Comments

- Summary - as mentioned by the authors, 3D platforms are becoming increasingly affordable. In fact, most motion analysis labs around the world have 3D platforms. In this context, a method for vertical ground platforms looses importance. Authors could perhaps expand the discussion on this.
- Summary - the summary as such should state the main features of the proposed method as well as highlight its importance considering other existing approaches, since the proposed approach is the focus of the manuscript. In its current form, summary is not informative and is excessively vague.
- Summary - what do authors mean by "waiting for 3D platforms"? This should be clarified.
- Summary - instead of referring to the "constraints of this model" I suggest authors use "limitations of the approach" as the model has not

been introduced or mentioned at this point of the manuscript.

- Manuscript has some typos and grammar mistakes. In some parts clarity of writing deserves more attention.
- Lines 7-8 - the total vertical force does provide information on the vertical acceleration of the CoP. Authors claim is only true in the horizontal.
- Line 10 - in spite of the "hundreds of articles" mentioned, literature review is excessively short and incomplete, even considering this manuscript is intended to be a short communication. Because paper proposes a new or alternative method, authors should provide a clear overview of the existing methods and their limitations along with some comments on the advantages of the proposed method compared to those. Provided information is not sufficient to give reader a reasonable picture of the current state of the art.
- Line 22 - is their uncertainty unacceptable or is their quantification of the uncertainty unacceptable? Clarify as there is a fundamental difference between these two formulations.
- Line 30 - authors write "We know that the mechanical equation…".Authors may know but this is not necessarily true for the reader of the manuscript. More information on Eq. (1) should be provided, including all the simplifications assumed, see next item.
- Eq. (1) - this equation implies many important simplifications and assumptions which are not mentioned in the manuscript. I consider this unacceptable as the whole method is based on this equation. Just to mention some:

a) Model is valid for an ideal inverted pendulum moving in the sagittal plane. It does not take knee bending or hip flexion into account, neither does it consider motion in the frontal plane or rotation about the vertical axis. This restricts application of the approach to cases where knee and hip joints remain fully extended.

b) Equation (1) is valid for small angular displacements only and neglects, for instance, centrifugal forces. This will incur in errors for greater angular displacements or angular velocities, which are not quantified or discussed by the authors.

c) Ankle height with respect to the force plate is neglected, and ankle joint is assumed to be positioned on the floor. This introduces errors which are not quantified or discussed by the authors.

- Eq. (5) - correct this equation as the exponential functions should not be superscript.
- Eq. (5) - as I understand it, G0 is actually the initial value of d(t), i.e. G0 = d(t=0), rather than the "initial solution" and GF is the final value of d(t), i.e. GF = d(t=F), rather than the "final solution". Correct or clarify. Also, again according to my understanding, Eq. (5) is valid only if F goes to infinity, or is an approximation for large F values. This should be explicitly stated and/or clarified.
- Eq. (6) - replace dt^2 by delta_t^2 as this is a finite difference approximation of the second time derivative of G(t).
- Eq. (6) - I do not see the solution of this set of equations as a trivial task in a clinical framework.
- Line 75 - G0 refers here to the initial position of G (i.e. G0 = G(t=0)) while G0 refers to something different in Eq. (5) as mentioned previously in this review. Correct and clarify.
- Line 75 - mentioning that Eq. (6) "is equated with its average value around each measurement point" is confusing and somewhat misleading. I recommend authors mention simply that Eq. (6) is a finite-difference approximation of the second time derivative of G.
- Paragraph after Eq. (6) - authors investigate the influence of the sampling frequency on error, but the conclusions will depend on motion features and experimental data processing such as filtering. I expect, for instance, that the discretization error will increase as the body oscillation frequency increases. Therefore, it is important authors explain for which conditions of motion pattern and data processing the mentioned conclusions on appropriate sampling frequency can be drawn. The manuscript does not provide any information on the employed reference data.
- Fig. (3) - is confusing. How is the mean value computed? How is the results compared to the mean

data? I could not understand the units of the y-axis. What does it mean " "thousands"? This figure must be much better explained.

- Section, line 84 - this section is very confusing and poorly written. It would be sufficient to explain that the anthropometric data in Eq. (7) can be found in different literature sources, e.g., anthropometric tables in Winter's book (1991). Instead, authors go into details of how to calculate mass moment of inertia and into very confusing computations of the constant k. I could not understand at all where the two last equations of the section come from.
- Same section - authors mention that estimation errors for k are around 5% or 1%. This is excessively optimistic and completely unrealistic. Estimation errors for the position of the center of mass and, particularly, for the mass moment of inertia of the body, which are needed for the computation of k, are probably larger than 10%. This is evidenced by the large discrepancies found in the literature for standard anthropometric values. Reported estimations can differ as much as 30 %. So, unless authors develop or employ measuring techniques to estimate individual anthropometric features of the subjects, expected errors of k will certainly be much larger than the claimed 1 to 5%.
- Discussion section is poor and needs a reformulation. Most of the discussion should be in the introduction section. In the discussion, I would expect a discussion on the particularities of the proposed method, of its application potential as well as on its limitations compared to other available approaches to estimate the CoG position.
- Line 144 - I do not understand what authors mean by "stability of the point P". Stability is a very far-reaching concept. A better explanation of what authors mean by stability of the point is needed.
- The conclusion is also poor and incomplete, and needs substantial improvement.

Minor corrections and remarks

- Lines 21-22 - using "bad" is not appropriate. I suggest "incorrect" or "inaccurate".
- Line 38 - In fact, eq. (1) has an infinite number

of solutions if the initial conditions are unknown. So, I recommend replacing "very many" by "infinite" for rigor.

- Line 78 - assimilation sounds odd here. Correct.
- Lines 133-134 - what do authors mean by " reduces the amount of information contained in the position of the point P"?

Reviewer #3:
This manuscript adds to the literature on using vertical-axis force platform with COP measurements to estimate motion of the body's center of gravity. It uses standard techniques to make the actual COM calculation, but the advancement presented here is an estimate of the uncertainty of the calculation.

This advantage of quantified uncertainty is worth knowing about and reporting, and is an appropriate topic for a short communication. The appropriate conclusion is that cutting off 3 seconds from the beginning and end of posturography trials is likely sufficient to ensure accurate COG estimates. The technical development is simply an application of standard techniques in differential equations, but its application to clarifying the error term is a useful extension in this application.

There should, however, be some significant changes, to make this work and its relation to other literature clearer.

First, I would like to see a brief mention of how the technical treatment relates to simple filtering techniques, specifically the simplest version described by Hof (Journal of Biomechanics 38 (2005) 2134-2135) that uses a forward-and-backward first-order low-pass filter. The mathematics of Eqn 6 are related, but may not be identical, to Hof's method.

Second, the present paper should make it clearer that the term "d" is what they are calling an "uncertainty", and that the various symbols "G" are COG position at individual points in time. These facts are a bit lost in the current description, so it is harder to follow the argument than it should be.

Third, the paper isn't really about "uncertainty" in general, but rather about "edge effects" due to unknown boundary conditions. Using the words "edge effects" would make it clearer exactly how the findings relate to other work in differential equations and signal processing. Uncertainty in general remains unknown, because there are additional effects of noise and measurement error.

The portion of the paper that evaluates uncertainty in the term "k^2", effectively the square of the complex exponent of decay and oscillation, is not terribly significant, as it only estimates the difference between some reference (which is not clearly specified) and an approximation based on height (the derivation of which is not clearly specified). This section should be reduced. The mathematical underpinning of the parallel axis theorem (reducing the ankle-based moment of inertia to a COG-based moment of inertia) could be omitted, and replaced with the details of the development of the approximation used in lines 119-124.

The most compelling discussion surrounds lines 139-150, where the authors point out the weaknesses in common usage of COP measures to assess stability. This section could be expanded to include their recommendations for which metrics are best used to indicate quality of balance.