Dirac deltas and discontinuous functions

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It is a commonplace—some would say the defining property of the Dirac delta function $\delta(x)$—that

$$\int_{-\epsilon}^{\epsilon} f(x)\,\delta(x)\,dx = f(0), \tag{1}$$

where $f(x)$ is any ''ordinary'' function, and $\epsilon>0$. But what if $f(x)$ is *discontinuous* at $x=0$? The fact that $\delta(x)$ is an even function suggests that the right-hand side becomes the *average* at the discontinuity:

$$\int_{-\epsilon}^{\epsilon} f(x)\,\delta(x)\,dx = \frac{1}{2}\,[f(0^+)+f(0^-)]. \tag{2}$$

But this interpretation, however reasonable it may seem, cannot be sustained in general, and although the error has been known to some for many years[1] it continues to embarrass the unwary.[2] We thought, therefore, that it might be useful to call attention to the problem in the simplest possible context.[3]

Consider the differential equation

$$\frac{df}{dx} = f(x)\,\delta(x). \tag{3}$$

For $x<0$ the delta function is zero, so

$$\frac{df}{dx} = 0, \quad \Rightarrow \quad f(x) = a_1 \tag{4}$$

(a constant). Likewise, for $x>0$,

$$\frac{df}{dx} = 0, \quad \Rightarrow \quad f(x) = a_2. \tag{5}$$

The question is how to connect these solutions at $x=0$. The standard trick is to integrate Eq. (3) across the boundary:

$$\int_{-\epsilon}^{\epsilon} \frac{df}{dx}\,dx = \int_{-\epsilon}^{\epsilon} f(x)\,\delta(x)\,dx. \tag{6}$$

The left-hand side is $f(\epsilon)-f(-\epsilon)=a_2-a_1$; if we accept Eq. (2) the right-hand side is $\frac{1}{2}(a_2+a_1)$, and we conclude that

$$a_2 = 3a_1. \tag{7}$$

On the other hand, if we interpret the delta function as the limit of a sequence of rectangles,

$$\begin{cases} \dfrac{1}{2\epsilon} & \text{if } -\epsilon<x<\epsilon \\ 0 & \text{otherwise,} \end{cases} \tag{8}$$

we can solve Eq. (3) in all three regions:

$$x<\epsilon: \quad df/dx = 0 \Rightarrow f(x) = a_1,$$

$$-\epsilon<x<\epsilon: \quad df/dx = (1/2\epsilon) f \Rightarrow f(x) = a_3 e^{x/2\epsilon},$$

$$x>\epsilon: \quad df/dx = 0 \Rightarrow f(x) = a_2.$$

Continuity at $x=-\epsilon$ yields

$$a_1 = a_3 e^{-1/2};$$

continuity at $x=\epsilon$ implies

$$a_2 = a_3 e^{1/2}.$$

It follows that

$$a_2 = e a_1, \tag{9}$$

which—obviously—is inconsistent with Eq. (7) (except for the trivial case $a_1=a_2=0$).

Of course, Eq. (9) was obtained using a very particular model for $\delta(x)$, and one might naturally wonder whether triangles, or Gaussians, for example, would lead to entirely different results. The answer is *no*, for in this case we can solve the differential equation directly.[4]

$$\frac{df}{f} = \delta(x)\,dx \Rightarrow \ln\left(\frac{f(x)}{f(-\infty)}\right) = \int_{-\infty}^{x} \delta(x)\,dx = \theta(x), \tag{10}$$

where $\theta(x)$ is the unit step function. Thus

$$f(x) = a_1 e^{\theta(x)} = \begin{cases} a_1 & \text{if } x<0, \\ e a_1 & \text{if } x>0. \end{cases} \tag{11}$$

Evidently *any* model of $\delta(x)$ as the limit of a sequence of functions of area one must yield this same result.

ACKNOWLEDGMENT

We thank Raymond Mayer for useful discussions.

[1]See, for example, B. Sutherland and D. C. Mattis, ''Ambiguities with the relativistic δ-function potential,'' Phys. Rev. A **24**, 1194 (1981); B. H. J. McKellar and G. J. Stephenson, Jr., ''Relativistic quarks in one-dimensional periodic structures,'' Phys. Rev. C **35**, 2262–2271 (1987); M. G. Calkin, D. Kiang, and Y. Nogami, ''Proper treatment of the delta function potential in the one-dimensional Dirac equation,'' Am. J. Phys. **55**, 737–739 (1987); C. L. Roy, ''Boundary conditions across a δ-function potential in the one-dimensional Dirac equation,'' Phys. Rev. A **47**, 3417–3419 (1993); F. A. B. Coutinho, Y. Nogami, and J. F. Perez, ''Generalized point interactions in one-dimensional quantum mechanics,'' J. Phys. A **30**, 3937–3945 (1997).

[2]See, for example, I. R. Lapidus, ''Relativistic one-dimensional hydrogen atom,'' Am. J. Phys. **51**, 1036–1038 (1983), corrected by M. G. Calkin, D. Kiang, and Y. Nogami, ''Proper treatment of the delta function potential in the one-dimensional Dirac equation,'' Am. J. Phys. **55**, 731–739 (1987); T. H. Solomon and S. Fallieros, ''Relativistic One-dimensional

Binding and Two-dimensional Motion,'' J. Franklin Inst. **320**, 323–344 (1985); D. J. Griffiths, ''Boundary conditions at the derivative of a delta function,'' J. Phys. A **26**, 2265–2267 (1993), corrected by F. A. B. Coutinho, Y. Nogami, and J. F. Perez, ''Generalized point interactions in one-dimensional quantum mechanics,'' J. Phys. A **30**, 3937–3945 (1997). and by G. Barton and D. Waxman, ''Wave Equations with Point-Support Potentials Having Dimensionless Strength Parameters,'' Sussex report 1994 (unpublished); M. A. Maize and C. A. Burkholder, ''Electric polarizability and the solution of an inhomogeneous differential equation,'' Am. J. Phys. **63**, 244–247 (1995), corrected by F. A. B. Coutinho, Y. Nogami, and F. M. Toyama, ''Logarithmic perturbation expansion for the Dirac equation in one dimension: Application to the polarizability calculation,'' *ibid.* **65**, 788–794 (1997); M. A. Maize, S. Paulson, and A. D'Avanti, ''Electric polarizability of a relativistic particle,'' *ibid.* **65**, 888–892 (1997).

[3]The trouble was first encountered by people attempting to solve the one-dimensional Dirac equation with a single delta-function potential (''one-dimensional hydrogen'') or with an array of delta functions (''relativistic Kronig–Penney model''). Allowed energies and scattering amplitudes derived using (2) do not agree with those obtained from the appropriate limit of rectangular potentials. Incidentally, the same difficulty arises for the Schrödinger equation with a potential proportional to the *derivative* of a delta function.

[4]There may be other ways to interpret Eq. (3), but this much is certainly true: naive application of Eq. (2) is likely to lead to serious inconsistencies.

Role of the centrifugal force in vehicle roll

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I was recently asked to appear as an ''expert'' witness in a court case involving a van that rolled on its side when turning a corner. The lawyers required a simple argument or demonstration that it wasn't the driver's fault, despite the fact that he was drunk at the time. The case rested on the fact that several passengers, who were also drunk, were leaning out the windows on one side of the van. The paper by Swinson[1] provided most of the physics that I needed. However, Swinson did not allow for the intoxicated passengers, since he assumed that the center of mass remained at the center of the vehicle. Swinson compared vehicle rollover with the toppling of a rectangular block subject to a horizontal force, but the analogy is incomplete unless one invokes the centrifugal force. Since the centrifugal force is a fictitious force, it tends to be avoided at all costs when teaching junior physics.[2,3] Nevertheless, it can be useful when analyzing phenomena in a rotating coordinate system.[4]

Consider a vehicle at rest on a horizontal road surface as shown in Fig. 1. The center of mass (CM) is located at a horizontal distance x from the wheels on the left side and a horizontal distance w from the wheels on the right side. If M is the mass of the vehicle plus its load, then the normal reaction forces N_1 and N_2 acting on the wheels are given by $N_1+N_2=Mg$ and $N_1x=N_2w$. Now suppose that the vehicle is subject to a horizontal force F acting at a height h above the road surface. Motion of the vehicle will then be opposed by horizontal friction forces f_1 and f_2 acting on the left and right wheels, respectively. If F is sufficiently large, the vehicle will start to slide or it might tip over, depending on the height h. If F is sufficiently small, then the vehicle will remain at rest. The friction and normal reaction forces are related by $f_1 \leqslant \mu N_1$ and $f_2 \leqslant \mu N_2$ where μ is the coefficient of static friction, typically about 0.8 or 0.9 for tyres in good condition on a dry bitumen surface.

If the vehicle remains at rest, then $N_1+N_2=Mg$, $f_1+f_2=F$ and the torque, τ, acting about the point P in Fig. 1 will remain zero. Hence, $\tau=Fh+N_2(x+w)-Mgx=0$ so $N_2=(Mgx-Fh)/(x+w)$. As one might expect, the same result is obtained if one considers the torque acting about the CM. N_2 will therefore decrease as F increases and will decrease to zero when $F=Mgx/h$. All the weight will then be transferred to the wheels on the left side of the vehicle and any further increase in F will cause the vehicle to tip over. However, if h is sufficiently small, the vehicle will slide rather than tip over. f_1+f_2 cannot exceed μMg, so the vehicle will start to slide when $F=\mu Mg$. If $\mu<x/h$, or $h<x/\mu$, sliding commences before the vehicle tips, but if $h>x/\mu$, the vehicle will tip before it starts to slide. As described by Swinson, a simple and elegant demonstration of this effect can be presented using a rectangular block of wood or a toy vehicle.

Now consider a vehicle making a right turn at speed v along a circular path of radius R. An external observer notes that the driver has rotated the wheels and that the tyres are pushing against the road surface. The only horizontal forces on the vehicle are the friction forces f_1 and f_2 acting on the tyres. There is no other external horizontal force to counter the friction forces, so the vehicle accelerates toward the center of the circular path, with $f_1+f_2=Mv^2/R$. The vehicle does not accelerate in the vertical direction so $N_1+N_2=Mg$. The clockwise torque acting about the point P is given by $Mgx-N_2(x+w)$. If this is zero, then $N_2=Mgx/(x+w)$, which is the result that one obtains when the vehicle is at rest and when there are no horizontal forces acting on the vehicle. Something appears to be wrong with this analysis.

A different result is obtained, for a vehicle making a right turn, if one considers the torque acting about the CM. The net torque in the counterclockwise direction is $(f_1+f_2)H+N_2w-N_1x=Mv^2H/R+N_2(x+w)-Mgx$, where H is the height of the CM above the road surface. If the speed is sufficiently small, the torque will remain zero, the vehicle will not roll over, and $N_2=M(gx-v^2H/R)/(x+w)$. However, N_2 will drop to zero when $v^2/R=gx/H$. The torque

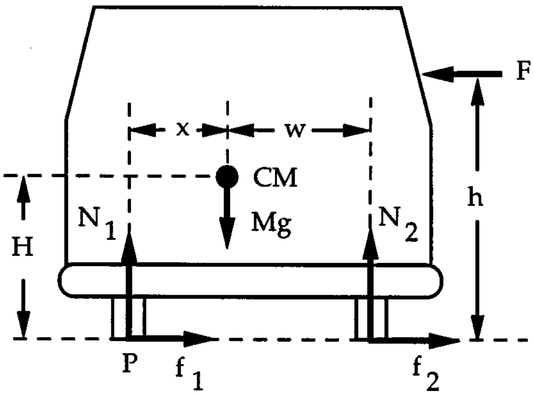

Fig. 1. The forces acting on a vehicle on a horizontal surface (viewed from the rear), when subject to a horizontal force F.

about the CM is then $f_1H - N_1x = M(v^2H/R - gx)$. Consequently, the condition for rolling is that $v^2/R = gx/H$.

The difference between these two sets of results can be reconciled if one changes to a reference frame where the vehicle is at rest. In this frame, the vehicle experiences a horizontal centrifugal force $F = Mv^2/R$ acting through the CM. The situation is then the same as described in the first example where an external force F is applied to the vehicle, provided that h is taken as the height of the CM above the road surface. If $\mu < x/H$, the vehicle will slide out of control when $v^2/R = \mu g$. If $\mu > x/H$, the vehicle will roll over when $v^2/R = gx/H$. For most vehicles, $x/H \sim 1.1$ and $\mu < 1$. Most vehicles are therefore stable against rolling, but a vehicle that is normally stable can still roll if the load is distributed in such a way that $\mu > x/H$ or if the vehicle slides into a curb and is then tripped by the curb.

An alternative solution of the problem, from an external observer's point of view, is that the torque acting about the point P is not zero since the angular momentum of the vehicle about point P does not remain constant. The velocity changes direction with time and changes vectorially by an amount $\Delta v = v\Delta s/R$ while the vehicle traverses an arc of length Δs. During the same time, the angular momentum about P changes by $(M\Delta v)H = MvH\Delta s/R$. The change in v is directed horizontally toward the center of the circular path, but the *change* in angular momentum and the torque are both parallel to the velocity vector. The rate of change of velocity is v^2/R and the rate of change of the angular momentum is Mv^2H/R. The torque about P is therefore $Mgx - N_2(x + w) = Mv^2H/R$ so $N_2 = M(gx - v^2H/R)/(x + w)$, as obtained above. Personally, I prefer the centrifugal force argument since it is simpler, more intuitive, and it preserves the analogy with the rectangular block. Readers who dislike centrifugal forces should read the article by De Jong[3] who presents a case for avoiding the term ''centripetal force'' since it can be interpreted by students as a fictitious additional force.

[1]D. B. Swinson, ''Vehicle Rollover,'' Phys. Teach. **33**, 360–366 (1995).
[2]R. P. Bauman, ''What is centrifugal force?'' Phys. Teach. **18**, 527–529 (1980).
[3]M. L. De Jong, ''What name should be used for the force required to move a mass in a circle?'' Phys. Teach. **26**, 470–471 (1988).
[4]J. M. Goodman and D. S. Chandler, ''A rotating coordinate frame visualizer,'' Am. J. Phys. **39**, 1129–1133 (1971).

Ampère was not the author of ''Ampère's Circuital Law''

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The original motivation for this note was an interest in finding out exactly what the contribution of Ampère was to the subject of the magnetic fields due to steady currents. At the outset I had the notion that Ampère was responsible for the Maxwell equation commonly labelled ''Ampère's Circuital Law.'' This law is the familiar

$$\oint_C \mathbf{B}\cdot\mathbf{ds} = \mu_0 i, \qquad (1)$$

that is, that the line integral of the magnetic induction **B** around a closed path C equals μ_0 times the current crossing the area bounded by C. I had not, for example, reflected on the strange fact that at the time of Ampère, circa 1820, the use of the vector **B** had not yet appeared on the electromagnetic scene, so that it was chronologically impossible that Eq. (1) should really be due to Ampère.

I consulted a recently published biography of Ampère[1] and found therein a statement that the Ampère Circuital Law was not due to Ampère:

> ''Ampère's own achievements should not be confused with a quite different law that is misleadingly named after him. Sometimes referred to as 'Ampère's circuital law' or more simply as 'Ampère's law,' this law depends upon field theoretic concepts and is often stated in the form:
>
> $$\oint \mathbf{B}\cdot\mathbf{ds} = \mu\Sigma I$$
>
> ... Maxwell discussed this law in his *Treatise on Electricity and Magnetism* and he correctly did not attribute it to Ampère.... The contrast between Am-

père's own theorems and 'Ampère's Law' reflects the conceptual gap that separates him from the field theorists of Maxwell's generation.''[2]

It is clear that ''Ampère's Circuital Law'' is not due to Ampère. It expresses a property of the vector **B**.

I found a very interesting book entitled *Early Electrodynamics, The First Law of Circulation*, written by R. A. R. Tricker.[3] In this book, what is known in the U.S.A. as Ampère's Circuital Law is called ''the first law of circulation.'' This confirmed the idea that the title of ''Ampère's Circuital Law'' was a misnomer.

I also investigated two papers in the French journal, *Revue d'Histoire des Sciences et de leurs Applications*. The first, a paper by Hamamdjian,[4] defended the idea that there was some justification for calling the First Law of Circulation by the title of ''Ampère's Circuital Law'' or ''Ampère's theorem.'' Hamamdjian claimed that Ampère's theorem ''expresses, in the best possible way the essence of the thinking of Ampère on magnetism, electromagnetism and electrodynamics, the substance of his deepest intuitions and convictions.''[5] Hamamdjian admits that the theorem was not formulated by Ampère, but rather by Maxwell. However, he said that to to find the theorem Maxwell based himself on Ampère's work, especially on Ampère's concept of the magnetic shell. Hamamdjian also based his claim on an unpublished manuscript by Liouville containing notes on lectures given by Ampère. These notes concerned the topic of the line integrals of forces around closed paths, and specifically the line integral of the magnetic force on an isolated magnetic pole as it moved around a current-carrying wire. J-P. Mathieu subsequently published a paper in the same journal[6] where he at least partly challenged Hamamdjian's paper and attributed the First Law of Circulation to Maxwell. Specifically, Mathieu gave as the first expression of the First Law of Circulation a statement by Maxwell in 1856 (note that this is 30 years after Ampère's death in 1836) as follows:

''the total intensity of magnetizing force in a closed curve passing through and embracing the closed current is constant, and may therefore be made a measure of the quantity of the current.''[7] Mathieu found that the First Law of Circulation (Ampère's Circuital Law) was a consequence of Maxwell's desire to establish a field theory. He argued that what he called the ''local, vector form of Ampère's theorem,'' i.e.,

$$\boldsymbol{\nabla}\times\mathbf{H}=\mathbf{j},$$

owed nothing to Ampère.

We agree with Mathieu's criticism of Hamamdjian's paper. Ampère's Circuital Law concerns the line integral of **B**, a magnetic field vector which is absent from Ampère's work. Ampère developed an action-at-a-distance theory of a Newtonian central force which acted between infinitesimal directed current elements. Ampère's formula is the mathematical expression of this central force and was considered by Ampère as the heart of his *electrodynamics*, a term which he coined.

The incorrect assignment of the name Ampère's Circuital Law was probably due to the desire to associate an important law of magnetism with the name of the historical father of electrodynamics. We must remember that Ampère's formula, the formula for the force between two infinitesimal current elements, on which Ampere placed so much importance as the foundational law of electrodynamics, has received hardly any attention at all in our century (Tricker's book is the exception). Indeed, this was already largely the case in the second half of the nineteenth century. Maxwell himself, who agreed with Ampère that his formula was truly foundational, nevertheless did not use this formula but instead used the concept of the electromagnetic field as foundational. Contrast Maxwell's praise for Ampère's formula in his *Treatise*:

> ''The experimental investigation by which Ampère established the laws of the mechanical action between electric currents is one of the most brilliant achievements in science.
>
> The whole, theory and experiment, seems as if it had leaped, full grown and fully armed, from the brain of the 'Newton of electricity.' It is perfect in form, and unassailable in accuracy, and it is summed up in a formula from which all the phenomena may be deduced, and which must always remain the cardinal formula of electro-dynamics,''[8]

with, in the same *Treatise*, his considering Ampère's action-at-a-distance formula as only worth an ''outline'' before continuing with his discussion of the Faraday–Maxwell field theory:

> ''We have considered in the last chapter the nature of the magnetic field produced by an electric current, and the mechanical action on a conductor carrying an electric current placed in a magnetic field. From this we went on to consider the action of one electric circuit upon another, by determining the action of the first due to the magnetic field produced by the second. But the action of one circuit upon another was originally investigated in a direct manner by Ampère almost immediately after the publication of Oersted's discovery. We shall therefore give an outline of Ampère's method, resuming the method of this treatise in the next chapter.''[9]

This ambiguity of Maxwell concerning Ampère was not lost on Heaviside, who in 1888 remarked:

> ''It has been stated, on no less authority than that of the great Maxwell, that Ampère's law of force between a pair of current elements is the cardinal formula of electrodynamics. If so, should we not be always using it? Do we *ever* use it? Did Maxwell in his Treatise? Surely there is some mistake. I do not in the least mean to rob Ampère of the credit of being the father of electrodynamics: I would only transfer the name of cardinal formula to another due to him, expressing the mechanical force on an element of conductor supporting current in any magnetic field—the vector product of current and induction. There is something real about it; it is not like his force between a pair of unclosed elements; it is fundamental; and, as everybody knows, it is in continual use, either actually or virtually (through electromotive force), both by theorists and practicians.''[10]

Of course, it is clear that Heaviside's ''the vector product of current and induction'' should, strictly speaking, not be attributed to Ampère, since the ''vector product'' and the magnetic ''induction'' were foreign to Ampère. Nevertheless, in the spirit of the modern Biot-Savart law, it would indeed make sense to call this Ampère's Law. This was precisely the title given to it in a well-known American physics text of the 1930s by Haussman and Slack. In that text one reads:

> ''For a straight current-carrying wire of length l that is perpendicular to the flux of constant density, the force in mks units is
>
> $$F=Bil$$
>
> where F is in newtons, B in webers per square meters, i in amperes, and l in meters... . The foregoing expression is a mathematical statement of Ampère's Law and is the key equation of electromagnetism; it can be applied to all forms of circuits and is the operating principle of electric motors and electromagnetic devices.'' [11]

This statement by Haussman and Slack, in a widely used text, surely indicates the persistence of the desire to associate the name of Ampère with some important law of electromagnetism.

As closely as we can tell it was around the period of the first edition of the Haussman and Slack text that the myth of ''Ampère's Circuital Law'' became part of the established repertoire of American texts. The first instance we have been able to find was in the influential 1940 text by N. H. Frank of M.I.T. wherein it is stated ''There is a a more general relation, than Eq. (24) between the magnetic intensity H and the steady current i which produces it, and this relation is known as the *circuital law*.'' [12]

The law which is currently called ''Ampère's Law,'' or ''Ampère's Circuital Law'' is a misnomer and should not be attributed to Ampère. The law which has a proper historical basis to be called Ampère's Law is the force law for the magnetic force on a current element, which expresses the fact that this force is perpendicular to the element.

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[1]James R. Hoffman, *André-Marie Ampère* (Cambridge U. P., New York, 1996), first published (Blackwell, Oxford, 1995).
[2]*Ibid.*, p. 349.
[3]R. A. R. Tricker, *Early Electrodynamics, The First Law of Circulation* (Pergamon, Oxford, 1965).
[4]P-G. Hamamdjian, ''Contribution d'Ampère au 'théorème d'Ampère,'' Revue d'Histoire des Sciences et de leurs Applications **31**, 249–268 (1978).
[5]*Ibid.*, p. 250.
[6]J-P. Mathieu, ''Sur le théorème d'Ampère,'' Revue d'Histoire des Sciences et de leurs Applications **43**, 333–338 (1990).
[7]*Ibid.*, p. 335 (Maxwell quotation from his paper entitled ''On Faraday's lines of force'').
[8]J. C. Maxwell, *A Treatise on Electricity and Magnetism* (Clarendon, Oxford, 1891; Dover, New York, 1954), Vol. 2, p. 175.
[9]*Ibid.*, Vol. 2, p. 158.
[10]As quoted by E. Whittaker in his *A History of the Theories of Aether and Electricity* (two volumes, originally published by Nelson and Sons, London, 1910) (revised and enlarged in 1951, reprinted as a Harper Torchbook in 1960), p. 88. Whittaker gave the source of the Heaviside quotation as *Electrician* (28 Dec. 1888); O. Heaviside's, *Electrical Papers*, ii, p. 500.
[11]E. Hausman and E. P. Slack, *Physics* (van Nostrand, Princeton, NJ, 1935; 2nd ed. 1939; 3rd ed. 1948, 4th ed. 1957).
[12]N. H. Frank, *Introduction to Electricity and Optics*, 1st ed. (McGraw–Hill, New York, 1940), p. 103.

Comment on ''Specific Heat Revisited,'' by C. A. Pizarro, C. A. Condat, P. W. Lamberti, and D. P. Prato [Am. J. Phys 64 (6), 736–744 (1996)]

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We want to comment on some of the results of Pizarro *et al.*,[1] in particular the problem of the specific heat of a particle in a box.

As is well known, the eigenfunctions and energy eigenvalues of the problem are obtained straightforwardly in many textbooks.[2] The analytical calculation of the partition function Z [their Eq. (39)] and the expression for the specific heat C_v [their Eq. (40)] are neither so common nor easy. To have a rough idea of how lower states contribute to the specific heat, we compute C_v including only the ground state and the first excited state in the partition function Z as function of KT/E_1; the result of this calculation is in curve (a) of Fig. 1. We repeated the calculation of C_v including the second excited state, too, obtaining curve (b); finally, we made the calculation with the necessary terms to obtain a convergent series with a precision of 1×10^{-5}, and the result is shown in curve (c).

It is easily seen that our results differ from those obtained

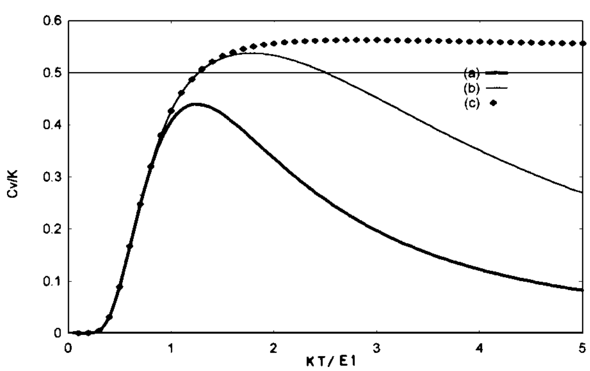

Fig. 1. Specific heat as function of KT/E_1, where $E_1=\pi^2\hbar^2/2ma^2$ is the ground state energy of a particle in a box of length a. Curve (a) was computed including only energies for the ground and first excited state in the partition function Z, whereas, curve (b) was computed including second excited state too. Curve (c) is the exact result. The curves do not show peaks, and curve (c) increases above 0.5 C_v/K and then decreases asymptotically to the classical limit 0.5 C_v/K.

by Pizarro *et al.* (their Fig. 11, Ref. 1, p. 742). We found that curve (a) has a maximum around $KT \approx 1.4E_1$ and curve (b) has its maximum shifted to about $KT \approx 1.9E_1$, while Pizarro *et al.* found a well-defined peak in $KT \approx 1.5E_1$ followed by a wide minimum in both cases. The numerical values on the horizontal axis in Fig. 11 appear to be in error by a factor of 3. Curve (c) also shows a very different behavior from the predictions of Pizarro *et al.*, since this curve rises up above $0.5C_v/K$ at $KT \approx 1.3E_1$ and later decreases asymptotically to $0.5C_v/K$, that is, the classical limit. We must remark that our results in curve (c) agree with those obtained by Rosentock.[3] Also, it must be noted that curve (c) computed by Pizarro *et al.* (their Fig. 11) always remains below the horizontal line $0.5C_v/K$.

We conclude that the curves do not show peaks, only very soft maxima, and that curve (c) does not always remain below the horizontal line $0.5C_v/K$.

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[a)]On sabbatical year from ESFM of IPN, COFFA fellow.

[1]C. A. Pizarro, C. A. Condat, P. W. Lamberti, and D. P. Prato, ''Specific heat revisited,'' Am. J. Phys. **64**, 736–744 (1996).

[2]R. M. Eisberg, *Fundamentals of Modern Physics* (Wiley, New York, 1961); J. J. Brehm and W. J. Mullin, *Introduction to the Structure of Matter, a Course in Modern Physics* (Wiley, New York, 1989); L. I. Schiff, *Quantum MeChanics* (McGraw-Hill, New York, 1968).

[3]H. B. Rosentock, ''Specific Heat of a Particle in a Box,'' Am. J. Phys. **30**, 38–40 (1962).

THE ANTHROPIC COSMOLOGICAL PRINCIPLE

The anthropic cosmological principle provides a promising selection rule: the important events and objects are those that, if they had been very different from what they are, we would not be here to ask questions about. In addition, the idea that the universe must have certain properties in order for observers to appear can function as a sort of explanation or cause for phenomena that might otherwise seem to be happy accidents—at least until we can find a better explanation in terms of other phenomena. In both these cases, the anthropic principle serves as a sort of tool, enabling us to get on with the business of doing science.

Virginia Trimble, *Visit to a Small Universe* (The American Institute of Physics, New York, 1992), p. 95.