

Re: HOCUS POCUS

Source: <http://sci.tech-archive.net/Archive/sci.physics.relativity/2006-09/msg01900.html>

- *From:* "PD" <TheDraperFamily@xxxxxxxx>
 - *Date:* 19 Sep 2006 15:19:00 -0700
-

mluttgens@xxxxxxxx wrote:

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mluttgens@xxxxxxxx
wrote:

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wrote:

There
are
three
facts
that
remain

—
The
correct
formula

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for
combining
velocities
(independent
of
what
assumptions
are
made
to
derive
that
formula)
has
been
completely
verified
in
particle
experiments.
Call
it
an
empirically
confirmed
formula,
if
you
like,
and
forget
about
deriving
it
from
any
assumptions.
It's
nevertheless
a
confirmed
relation.

Exact
references,
please.

I've
done

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this
for
you
before
Marcel,
almost
a
year
ago.
Your
memory
is
short.
That,
or
you
learn
nothing.
That,
or
you
didn't
read
what
I
pointed
you
to.
I
pointed
to
the
comparison
of
rapidity
distributions
in
proton-proton
collisions
at
fixed
target
and
collider
experiments.

Yes,
I
remember,

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but
I
didn't
find
any
experiment
whose
data
allowed
to
confirm
your
claim.

I gave you
references
to the
papers at
the time. I
don't know
why you
wouldn't
have been
able to "find
any
experiment".

Any experiment that
confirmed your claim.

The references I gave confirm this claim.

[...]

Proton–proton
collisions
and
electron–electron
collisions

(for
example)
are
routinely
done
in
both
collider
and
fixed–target
environments.
The
physics
of
these
collisions
is
simple
enough
(at
least
for
some
measurable
distributions)
that
comparison
of
the
nature
of
the
collisions
in
both
environments
is
tantamount
to
measuring
the
same
process
in
two
very
different
frames
of
reference.
The

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relative
speed
of
the
colliding
particles
is
measured
in
both
cases
as
a
normal
part
of
beamline
monitoring
operations.
The
fact
that
the
distributions
are
identical
if
and
only
if
the
relative
speeds
in
the
two
reference
frames
are
related
exactly
by
the
relativistic
prescription,
is
compelling
evidence
that
the
relativistic

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prescription
is
correct.

Exactly
related,
thus
no
error
bars
?

Don't
be
ridiculous.
A
measurement
always
involves
error
bars.
You
should
never
use
the
presence
of
experimental
error
bars
as
an
argument
that
the
evidence
is
not
compelling
enough.
The
measurement
should
be
sufficiently
precise
that
it

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can
clearly
distinguish
between
a
tested
model
and
a
competing
model.
That
is
true
in
this
case.

"Exactly"
was
a
bit
exaggerated.
Anyhow,
a
statistical
analysis
is
needed
to
claim
that
such
distinction
exists.
Where
is
it
?

In the
papers. This
is part and
parcel of an
experimental
paper. Hint:
You may
need to read

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some of the
references
to get a
complete
picture.

Among those many papers,
at least one of them should
be unambiguously
pertinent. Which one?

The two primary papers I gave you are
unambiguously pertinent. You want
to be spoon fed. I don't have a spoon that
will fit your mouth.

According to Google, there are about 52,200 papers about
rapidity
distributions
in proton–proton collisions, so I would be grateful if you
gave the
exact references
of those two primary papers. As you seem to be the only
person claiming
that
the relativistic addition formula is validated by experiments,
I am
convinced that
a lot of people would also be grateful to get those references.

As I told you Marcel, I did the work and gave you explicit references
nearly a year ago. You can surely do some Google work to at least find
out where I did that, especially since you say you remember it. I'm not
about to make things lapdog–easy for you, Marcel. I wouldn't do it for
a serious physics student, either, because knowing how to dig this
stuff up is as basic a skill as using a timing light is to a car
mechanic.

About one year ago, you claimed that papers about rapidity
verified length contraction. Now, they allegedly verify the
relativistic
addition of velocities.

Yes, indeed. Both are confirmed in the same pair of experiments.

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You are acting like a snake oil seller !
The snake oil is of course SR.

Well, once again, if experiment says the oil works, it works. You can call it snake oil all you want, but it works.

Now, you have three choices at this point:

- a) You can say you don't want to work that hard at figuring this stuff out, in which I'd invite you to steer clear of the business entirely because there are plenty who are willing and eager to do that.
- b) You can say that you don't know how to look stuff up, and you need more basic training in how to do library research and how to find out what work has been done on a topic that you're unfamiliar with. In this case, I can recommend some options for getting that training other than dawdling in newsgroups.
- c) You can say that you are willing to work hard at figuring this stuff out, and that you do know how to do library research, and then you go do it.

I will not repeat the "job" I did last year, as nowhere did I find a confirmation of your claims.

Really? Then apparently you don't know how to read scientific papers. In that case, I recommend option (b). Would you like some suggestions?

Did
the
experimenters
expressly

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claimed
that
their
observed
distributions
are
explained
by
the
relativistic
addition
of
velocities?

No,
they
did
not.
This
has
to
be
inferred
from
the
background
information
supplied
in
the
references.
If
you're
hoping
that
physics
articles
are
explicitly
laid
out
to
answer
the
specific
question
you
ask
(as
though

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the
article
is
written
in
response
to
your
question),
then
your
expectations
about
reading
scientific
articles
probably
needs
to
be
adjusted.

As
the
experimenters
seemingly
didn't
mention
your
alleged
relation
with
the
"relativistic
prescription",
I
presume
that
you
did
the
analytical
job
yourself.

Actually, I
didn't need
to do that
because I

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had already
read and
was
familiar
with most
of the
papers
referenced
in the
articles I
referred
to you, or I
was
separately
familiar
with their
results
through
contact
with the
experimenters.
Moreover,
I'm familiar
with the
definition
and
properties
of rapidity.
And so
when I see
rapidity
distributions
that
are identical
at the same
root-s, then
I know what
that means.
People
that aren't
as familiar
with the
field have to
do a bit
more
background
reading to
catch up,
but it's not a
huge issue.

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Iow, your experience of the subject matter justifies your personal conclusion. Scientifically, "seing" something is not enough. As you are so sure, why don't you publish a paper, that would bring you some fame?

I have my share of published papers, thanks, and I'm not in need of fame, thanks. And yes, indeed, my experience of the experimental evidence informs my personal conclusion. A personal conclusion that is not based on experimental evidence, on the other hand, but is based on intuition and incredulity, does not carry a whole lot of weight.

And you think that your experience of the experimental evidence alone does carry much weight ?

In science, Marcel, experimental evidence carries enormous weight. It trumps intuition, it trumps common sense, it trumps what looks to be really good ideas. If it ain't confirmed by experimental evidence, then it ain't worth squat.

Now, you may be of the opinion that for every bit of experimental evidence that supports one idea, then there is other experimental evidence that supports a completely different idea. This is tantamount to rejecting the value of experimental evidence in science. If this is what you think, then you either do not know what role experiment plays in science, or you shouldn't be trying to do science, or both.

Shaky or alledged experimental evidences demonstrate nothing.

Shaky? And on what basis do you say the evidence is shaky?
You are beginning to sound like Henri Wilson who says that any experiment that provides support for SR obviously has something wrong

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with it, without even looking at it.

As I said, if you're the kind of person that rejects the value of experimental evidence, or decides whether experimental evidence is shaky based on whether it agrees with your expectations, then you've got no business attempting to do science.

PD

Marcel Luttgens

PD

Marcel Luttgens

If
this
is
the
case,
perhaps
could
you
show
us
how
you
reached
your
conclusion.
Otherwise,
the
readers
could
remain
skeptical.

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It's not my
task to
make you
less
skeptical.
It's not my
job to
educate
you in a
newsgroup.
I am giving
you enough
information
so that you
can
correct your
misconception
and lack of
familiarity
with
experimental
results on
your own
without too
much
difficulty.
Now, do
some
homework.

Those are rather arrogant
words, revealing a strong
personality.

I don't care what you think about my
personality, Marcel. Physics
doesn't have to be nice. You *are* expected
to do some homework if you
want to do some physics.

Don't
forget
that

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the
burden
of
the
proof
lies
with
the
"claimant",
not
with
the
reader.

This
newsgroup
is not the
courtroom
where such
issues are
decided.
You
are owed no
burden of
proof here.
The
information
you've been
afforded
here in
response to
your error is
a luxury.
Anyone can
choose to
not
learn
something
if they're
really not
interested in
learning it,
and
that in no
way places
the burden
on anyone
to teach
them
anyway.

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In a sense, it **is**a
courtroom, where paranoia
is condemned.

Nonsense. Popularity contests and
impassioned debates about what
should be so have no place in physics.
Bucking the status quo for the
sake of doing just that serves no useful role
in physics. This ain't no
coffee shop, this ain't no steenking
philosophy club.

Marcel Luttgens

PD

Marcel
Luttgens

Where
can
their
articles
be
found
?
(Exact
references,
please).

Given
previously,
Marcel.
Do
your
own
homework
now.

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Marcel
Luttgens